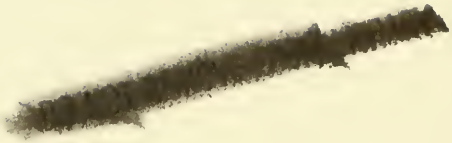

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(Vol. XXI.—July, 1889.)

AMERICAN RAILROAD BRIDGES.

By THEODORE COOPER, M. Am. Soc. C. E.

The existing and the accepted types of bridges in use to-day on American railroads being the results of a true evolution, no attempt to present them intelligently would be complete without a brief sketch of the past history of bridges in America.

The rapid development of the new world and the enormous number of bridges that has been built within the limits of the nineteenth century, have furnished us with a wide experience, from which we have been able to select the good and reject much that was bad or undesirable.

The pioneer life, not only of the earlier settlers, but of each generation to the present day, has developed to a high degree the energies, ingenuity and self-reliance of the American people. These pioneers were compelled to be men of all trades. Their limited resources and the lack of time or opportunity to seek for past precedents, impelled them to solve each problem anew. They, "thought with vigor and were not fettered with the trammels of science, before they were capable of exerting their mental faculties to advantage," as Sir Joseph Banks wrote to Thomas Paine in 1788.

Having no educated "lines of least resistance," they were better able to solve the many problems before them by new and novel methods.

The bridging of small streams was a part of the pioneers' labor. The crossing of the larger rivers developed specially gifted men, like Timothy Palmer, Theodore Burr, Lewis Wernwag, and others less well known, who built timber bridges that are looked upon as wonderful structures, even to the present day. The records of the early bridges of America are very incomplete, but enough remains to show what admirable work these early bridge builders could do.

1. Wooden Bridges.

The earliest bridges, where single timbers were not sufficient to stretch from bank to bank, were short spans supported on piles, or, where these could not be used, on timber cribs filled with stone. Where the conditions would not allow of structures of this character, arch spans were usually adopted.

In 1660, "The Great Bridge," as it was then called, was built across Charles River, between Old Cambridge and Brighton. It was a pile bridge.

In 1761 Samuel Sewall planned and built a bridge over York River, Maine, 270 feet long, supported on thirteen piers. Rebuilt in 1793.

In 1786 Mr. Sewall built a bridge over the Charles River, at Boston, 1 503 feet long, supported on seventy-five piers. A year or so later bridges on the same plan were built at Malden and Beverly, Mass.

In 1792 Colonel William P. Riddle built the Amoskeag Bridge at Manchester, N. H. It was 556 feet long, and supported on five piers and two abutments. It was commenced on the 3d of August, "at which time the timber was growing, and the rocks dispersed in the river," and completed on September 29th.

Between 1785–92 Colonel Enoch Hale built over the Connecticut River, at Bellows Falls, a bridge 368 feet long, in two spans, taking advantage of a rock in the middle of the river for his center pier. The West Boston Bridge over the Charles River, 3 583 feet long, and supported on one hundred and eighty pile bents, was finished in 1793.

Near the end of the eighteenth century a bridge was built over Cayuga Lake, N. Y. It was a pile bridge in 25-foot spans, one mile in length.

In 1795 a bridge was built over the Mohawk River, 960 feet long, supported on thirteen piers.

In 1792 Timothy Palmer built the Essex-Merrimack Bridge over the Merrimack River at Deer Island, about 3 miles above Newburyport, Mass. It consists of two bridges resting on Deer Island in the midst of the river (Plate I). "An arch of 160 feet span and 40 feet above the level of high-water connects this island with the mainland on one side; the channel on the other side is wider, but the center arch is but 113 feet." That part of the bridge on the Newbury side, the 160-foot span, was removed in 1810 and replaced by a chain suspension bridge. The part on the Salisbury side remained until 1883. The chain bridge was built by John Templeman, of the District of Columbia. "It was the first chain bridge built in New England." Its span is 244 feet between bearings on towers; the towers are timber frames covered with boards and shingles. February 6th, 1827, one of the chains broke under a heavily loaded wagon drawn by four oxen and one horse. The bridge was again rebuilt, and is still in use. The bridge, as it now exists, consists of two independent roadways, each 15 feet 6 inches wide. Each roadway is supported on two sets of chain cables, each set being composed of three chains. These chains seem to have been repaired in many places with different sized links. The chains generally are formed of links about 2 feet long, made of 1-inch square iron. For about 6 feet over the bearings on the towers, each chain is spliced or replaced by three smaller chains with links about 1 foot long, and of about ½-inch square iron. The floor is hung from these chains every 7 feet by suspenders, formed indifferently of bars 1 inch square, straps 2 x ½ inch, or pieces of chains (Plates II and III).

In 1793 Timothy Palmer built another bridge over the same river at Andover. Rebuilt in 1803.

In 1794 he built the Piscatauqua Bridge, 7 miles above Portsmouth, N. H. It was 2 362 feet long. The greater part consisted of pile work.

"But that part which engages the attention of travelers, is an arc nearly in the center of the river, uniting two islands over water 46 feet deep. This stupendous arc of 244 feet chord is allowed to be a masterly piece of architecture, planned and built by the ingenious Timothy Palmer, of Newburyport, Mass."

"The chord of this arch is 244 feet 6 inches. The versine of the arch was 27 feet and 4 inches, and depth of the frame-work of the arch

18 feet and 3 inches. There are three concentric ribs, the middle one carrying the floor of the bridge; they were selected from crooked timbers, so that the fiber might run nearly in the direction of the curves, and are connected together by pieces of hard and incompressible wood, with wedges driven between, the ribs being mortised to receive them; thus the ribs are kept at a regular and parallel distance from each other. Each rib is formed of two pieces laid side by side about 15 feet in length; they are all disposed in such a manner as to break joints, the end of one timber coming in the middle of the length of the other which is near it; their ends all abut with a square joint against each other and are neither scarfed nor mortised, the two pieces of timber being held together by transverse dovetail keys and joints; all the timbers are admirably jointed and freely exposed to the action of the air; any piece may also be removed in case of its requiring separation without injury to the rest of the structure."

The bridge was 38 feet wide and had three arched trusses. Another description states that the second rib carrying the floor of the bridge was of a larger radius than the lower to facilitate the traveling; the upper rib served for a railing.

In 1794 the bridge at Haverhill was built by Timothy Palmer, "contemporaneous with the first stage coach and the first newspaper." It consisted of three arches of 180 feet; each is supported on three handsome piers 40 feet square; it had as many defensive piers or sterlings extending 50 feet above and a draw 30 feet over the channel.

In 1796 he built a bridge over the Potomac River, at Georgetown.

In 1796 Rufus Graves built a bridge over the Connecticut River, at Hanover, N. H., consisting of a single arch of 236 feet. He patterned his bridge after the Piscatauqua Bridge built by Palmer. The roadway followed the line of the arch and was some twenty feet higher at the center than at the abutments. The bridge was formed of the largest selected white pine, hewed to 18 inches square, some of them 60 feet long. This bridge fell in 1804, without warning, and by its own weight; its destruction being hastened by the undermining of one of the abutments, through deficient waterway. The builder of this bridge studied divinity, graduated at Dartmouth College, became a merchant, then a bridge builder, then an officer of the United States Army, and finally a physician.

In 1795 a bridge was erected at Holt's Rock, between Newbury and

PLATE I. ESSEX-MERRIMACK BRIDGE. 1792. Vol. XXI, p. 4.

PLATE II. ESSEX-MERRIMACK BRIDGE. 1810. Vol. XXI, p. 4.

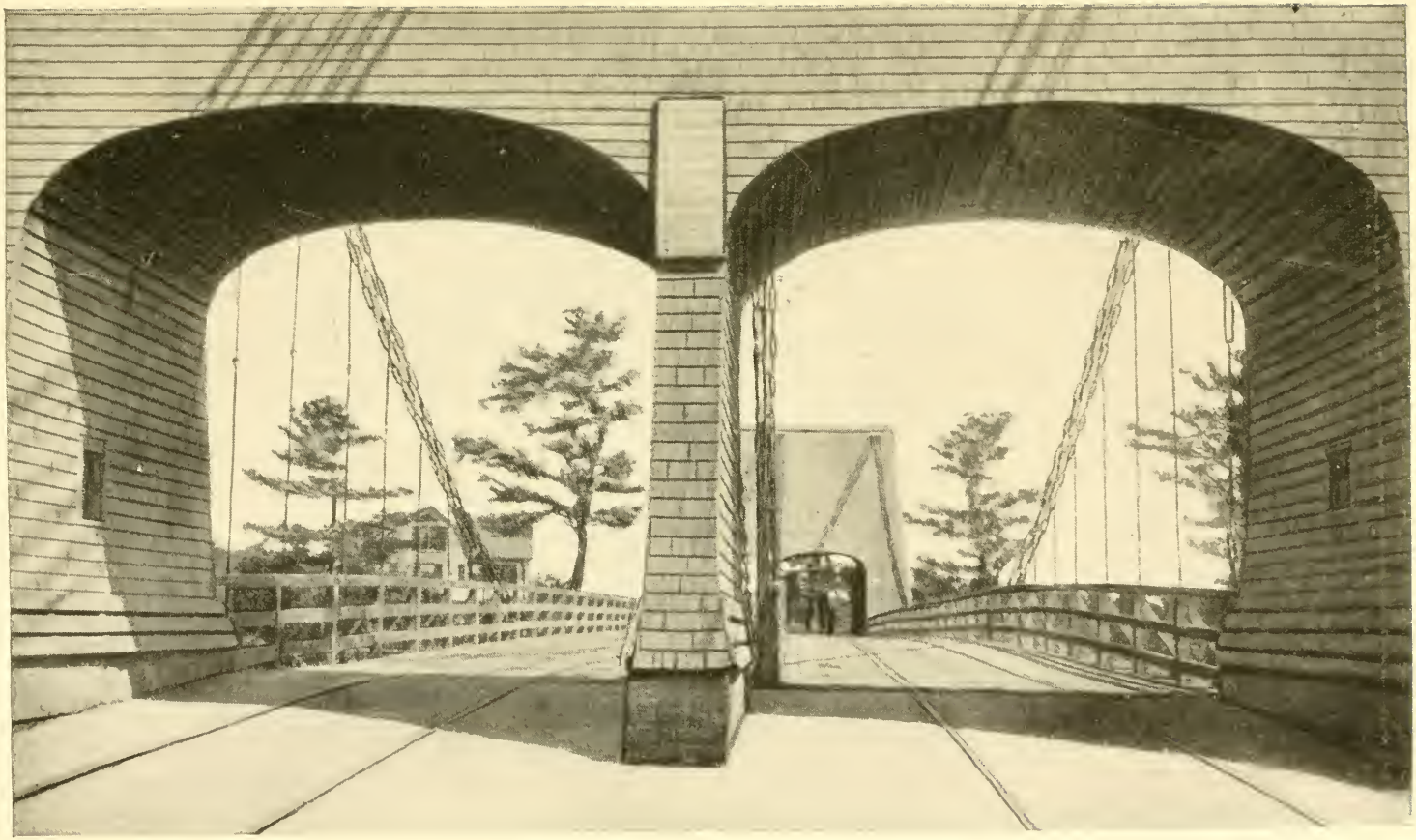

PLATE III. ESSEX-MERRIMACK BRIDGE. 1810. Vol. XXI, p. 4.

Haverhill. It was 1 000 feet in length and consisted of four arches and one draw-span. It was carried away by the ice in 1818.

In 1796 a bridge was built between Harlem and Morrisania, over the Harlem River.

About the end of the last century a bridge was built at Windsor, Vt., with two spans of 144 feet, and one at Romley, Mass., consisting of 8 arches of a total length of 870 feet; one at Howland's Ferry, R. I., 900 feet long, with a sliding draw, supported on 42 pile bents, and the Weybasset bridge at Providence, R. I., "160 feet long, supported by two wooden trestles and two stone pillars."

There were also two bridges over the Lehigh River, one at Bethlehem and one at Easton.

These are the records as I have been able to gather them, of bridges of any importance built in the eighteenth century.

From 1804 to 1806 "The Permanent Bridge" over the Schuylkill River at Philadelphia was built. It consisted of two arches of 150 feet clear and one of 195 feet clear.

"The plan was furnished by Mr. Timothy Palmer, of Newburyport, Mass., a self-taught architect. He brought with him Mr. Carr as his second and four other workmen from New England. They at once evinced superior intelligence and adroitness in a business which was found to be a peculiar art, acquired by habits not promptly gained by even good workmen in other branches of framing in wood." "The frame is a masterly piece of workmanship, combining in its principles that of king post and braces or trusses, with those of a stone arch."

There were three truss and arch frames. The timber was of the best white pine. Width of the bridge, 42 feet. The bridge was covered and closed in from the weather. Mr. Palmer stated that from his experience, wooden bridges uncovered would become unsafe in ten to twelve years.

"I am an advocate for weather boarding and roofing, although there are some who say it argues much against my own interests; notwithstanding I am determined to give my opinion as appears to be right. It is sincerely my opinion that the Schuylkill Bridge will last thirty and perhaps forty years if well covered. You will excuse me in saying, that I think it would be sporting with property, to suffer this beautiful piece of architecture (as you are sometimes pleased to call it), which has been built at so great expense and danger, to fall into ruin in ten or twelve years."

Plate IV is copied from an old engraving. The superstructure was entirely renewed on a more modern plan of a wooden arch bridge in 1850, and widened for also carrying a railroad track. The foundations

for this bridge were very difficult of execution, owing to the lack of experience at that day in work of this kind. Depth of water at east pier 24 feet, and at west pier 41 feet, rock bottom with slight covering. The foundations were laid in coffer-dams. Mr. William Weston, an eminent English engineer, who was in Philadelphia at that time, furnished the plan for the coffer-dam; but "declared that he should hesitate to risk his professional reputation on the event." Afterwards, in a congratulatory letter to the President, he says:

"I most sincerely rejoice at the final success that has crowned your persevering efforts in the erection of the west pier; it will afford you matter of well founded triumph when I tell you that you have accomplished an undertaking unrivaled by anything of the kind that Europe can boast of." "I have never, in the course of my experience or reading, heard of a pier founded in such a depth of water on an irregular rock affording little or no support to the piles."

This pier, however, was not carried to the rock, for the weakness and leakage of the coffer-dam became so threatening, the masonry was started three or four feet above the rock.

In 1805 Timothy Palmer completed the bridge over the Delaware River at Easton, Pa. It consists of three spans, 163 feet clear. This bridge, after eighty-four years' service, is still in use and about five-sixths of the original timber is in good condition. It has always been covered in from the weather. (See Plate V.)

In 1804, Theodore Burr built the Waterford Bridge over the Hudson River, consisting of four arch spans, one 154 feet, one 161 feet, one 176 feet, and the other 180 feet, clear spans. The timber is hewn yellow pine. This bridge was not covered until 1814, but is still in use, and in reasonably good condition. (See Plate VI.) The footings of some of the arches required renewal a couple of years ago. The drawings of the Easton and Waterford bridges are made from sketches taken from the existing structures. Burr also built another bridge over the Hudson at Fort Miller.

A bridge over the Connecticut River at Springfield was built about this same date by Mr. Walcott.

From 1804-06, Theodore Burr built the bridge over the Delaware River at Trenton (Plate VII) consisting of five arch spans, two of 203 feet, one of 198 feet, one of 186 feet, and one of 161 feet in the clear. Each span had five arched ribs, formed of white pine plank, from thirty-five to fifty feet in length and 4 inches thick, repeated one over the other, breaking joints, until they formed a depth of 32 inches. The lower

chord was composed of two sticks 6½ x 13½ inches. The roadway was suspended from the arch ribs by vertical chains. The arch was counter-braced by diagonal braces, formed of two sticks 6 x 10 inches spiked to the lower chord and secured to the arch above by iron straps. Outside of the exterior ribs wing arches 50 feet long, splayed out so as to widen the bearing on the piers and abutments. The bridge had no other wind bracing. This bridge is erroneously credited to Lewis Wernwag. The arch footings required renewal in 1832, owing to decay of the timber. In 1848 the bridge was remodeled by removing the wing arches and adding a new and stronger arch rib on the south side, and at the same time strengthening the adjacent old arch rib, by increasing its depth to 4 feet, to carry a railroad track. In 1869 it was again strengthened. It was finally removed in 1875, and replaced by an iron structure.

This bridge may, therefore, be considered as a connecting link between the early wooden long-span bridges for highway traffic and those for railroad purposes.

In 1808, Theodore Burr built the bridge shown on Plate VIII, at Schenectady, N. Y. This was the second bridge built at that place by Burr, the first falling before or soon after completion. The traditions in regard to the first bridge are not very reliable. The contract for the one shown, however, states that it is to be "exactly on the site of the bridge built by the said Theodore at the same place," and apparently with different spans, as new piers are provided for, by the contract. There may, therefore, be some truth in the tradition, that the first bridge was to consist of two spans of 450 feet; that one span was completed and the other partially, when it fell or was swept away by floods.

The bridge shown is a curiosity in bridge construction. It is a suspension bridge of wood. The curved ribs are formed of eight 4 x 14-inch planks spiked or bolted together. The planks are cut to long bevels at the ends for splicing. The ribs are three in number, spaced about 13 feet apart. The timber was white pine. The plan shown is copied from Burr's original drawing attached to the contract.

The bridge was used for twenty years in this condition. It then became necessary to build additional piers under the middle of each span to stop the excessive sagging of the bridge. In this condition it stood till 1873, when it was replaced by an iron bridge.

From 1812-16, Theodore Burr built the bridge at Harrisburgh, Pa., over the Susquehanna River, consisting of twelve spans of about 210 feet. The half of this bridge which is south of the Island still remains in use.

In 1812 Lewis Wernwag built his "Colossus" bridge over the Schuylkill at Fairmount, Philadelphia. (See Plate IX.) It had a clear span of 340 feet 3¾ inches. The eastern abutment was founded on the rock, and the western one on piles driven to the rock. This bridge was destroyed by fire, September 1st, 1838.

In 1814, he built the New Hope Bridge, Plate IX, over the Delaware River, consisting of six arch spans of 175 feet; versed sine of arch, 13 feet. Each span consisted of three arch ribs, formed of four sticks 6 x 15 inches, dressed to the curve and confined by iron clamps and bolts.

Wernwag's practice was to saw all his timbers through the heart, to detect unsound wood and permit good seasoning of the timbers. He used no timbers of a greater thickness than 6 inches, and separated all the sticks of his arches by cast washers, to allow free circulation of the air.

In 1810 he built a wooden cantilever bridge over the Nashammony River, Pa. He called it his "Economy" bridge, and claimed that it could be used to advantage up to 150 feet spans.

From this date to 1836 he built many bridges. In 1830 he built his first railroad bridge at Manoguay, on the Baltimore and Ohio Railroad.

His last bridge was the railroad bridge over the Canal and Potomac River, at Harper's Ferry, in 1836.

The previously mentioned bridges with spans of any magnitude appear to have been arch bridges.

The first marked step toward bridges of the modern truss form was the wooden lattice bridge, patented by Ithiel Town, January, 1820. Plate X.

The horizontal members or chords of this bridge were composed of two or more parallel sticks, spaced so that the diagonal web members or lattice work would pass between them. The timbers were usually 2 to 4 inches in thickness, and 10 to 12 inches wide. The web members were placed closer together for the longer spans. The chords were also made in two sets for the longer spans, one placed above the other.

The web members at every crossing and at their junction with the chord pieces were fastened with wooden treenails.

The timbers being of one size and of reasonable dimensions, were readily obtainable. The absence of all bolts, straps or rods of iron, and the simplicity of the mechanical operations required to connect together the parts of this bridge, made it a cheap and popular structure.

A great number of bridges of this class, up to spans of 220 feet, were built throughout the United States, both for highway and railroad purposes. There are many of them still in existence.

These bridges were made of uniform section throughout. In order to get sufficient support at the ends, they were usually extended over the abutment a distance about equal to their depth, and were made continuous over all the piers.

From the thinness of the web system the trusses were apt to warp, and as they aged they became very flexible, owing to the want of rigidity of the treenail connections.

Mr. Town claimed, at least in his later pamphlets (1831), that these trusses could be made of wood, wrought or cast-iron.

The next step toward simplicity and concentration of parts appears to have been the truss known as the Long truss, patented by Brevet-Lieutenant-Colonel Long of the United States Engineers, in March, 1830, and November, 1839.

This also was a form of truss in which iron did not enter as a necessary part, the connections being made by framing the parts together, or by use of wooden keys or treenails. Many bridges were built of this form, but it never became widely popular.

Both the Lattice and the Long bridges were combined with the arch in many cases, especially for the longer spans.

No further important advance in the styles of the trusses was made until 1840, when William Howe patented the truss which was the basis for the present Howe truss bridge. Plate XI.

This form of truss grew rapidly into favor, from its simplicity of construction, perfection of detail and satisfactory action under service. For some years it has been the standard form of wooden bridge in use upon our railroads.

The chords and braces are made of timber, and the vertical web members (tension) of round iron, with screw ends, usually upset to give a stronger section through the screw threads than in the body of the rods. The chords are made of uniform section throughout, and both the top and bottom chords of the same number of timbers, and timbers

of the same width and spacing. The sticks of the lower chord (for through bridges) are made deeper than those of the top chord, on account of the loss of section from splicing, the lower strains used in tension and to resist the bending strains due to the cross floor timbers, which rest directy on the lower chord. The chord sticks are spaced two or more inches apart, to allow the passage of the vertical rods without cutting away any of the timber, to give room for the packing and splice blocks, and to admit free circulation of air between the timbers. The number and length of the chord timbers are arranged so that no more than one stick need be spliced in any one panel of the truss. The chord sticks may be spliced either with oak or iron splices.

The braces and counterbraces are square-ended, and rest upon the inclined faces of the angle blocks. They are held from displacement by lugs or dowels. The angle blocks were formerly made of oak, but now are made of cast-iron. They are provided with square faced projections, which are let into the chord timbers, to take up the horizontal shear of the braces by direct fiber compression.

The "gib plates," as the plates on which the nuts of the web rods bear are called, are usually made of thick, flat plates of wrought-iron.

The space between the chord timbers below the angle blocks contains cast tube blocks, made a little short to allow for the shrinkage of the timber.

The vertical shear of the braces is transferred through the angle blocks and these tube blocks to the gib plates and web rods, thus avoiding any crushing action across the fiber of the wooden chord pieces.

The timber in a properly designed Howe truss is only strained longitudinally with the fiber.

The very wide experience in this class of bridges has enabled the American bridge builders to so proportion them in all their details that the best results can be obtained from the material at a moderate cost. But for the two objections, their liability to destruction from fire or from decay, no better railroad bridge up to 150 feet spans could be desired. For most of our country the cost of good timber makes iron bridges the cheaper, at first cost, for spans over 150 feet.

Very few Howe truss bridges are now being built upon the older and financially better roads, all wooden bridges being replaced with iron structures.

In April, 1844, Thomas W. and Caleb Pratt patented the truss known as the Pratt truss. It differed from the Howe in making the diagonal members of the web system of iron (tension) and the vertical members (compression) of wood, the reverse of the Howe principle. While many bridges were built upon this form, it never succeeded in attaining an equal popularity with the Howe; as it required a greater quantity of the more expensive material, wrought-iron, and was not so well suited to the joint use of the two materials, wood and iron.

It became, however, the favored form, afterwards adopted, for iron bridges, and is therefore one of the steps in the development of American bridges.

In addition to the general forms of structures heretofore mentioned, the variations of these broad types and other special forms were numerous. Each individual builder had his own favorite at some time. It would be impossible and useless to take up those numerous individual cases, though they undoubtedly served a good purpose in the general evolution.

The development of wooden bridges was entirely empirical. They were generally of uniform section, and their proportions determined either from the result of previous failures or from the study of models. The advancement was, however, toward simplicity of form and construction, and greater perfection of the details of the connection of the parts.

2. Iron Bridges.

As early as 1786 the need of a material for long span bridges, more suitable than wood, engaged the attention of thinking men.

Thomas Paine, in 1803, wrote as follows, giving an account of his advocacy and efforts toward the construction of iron bridges:

"As America abounds in rivers, * * * I turned my attention, after the Revolution was over, to find a method of constructing an arch that might, without rendering the height inconvenient or ascent difficult, extend at once from shore to shore over rivers of three, four or five hundred feet and probably more. The principle I took to begin with and work upon, was that the small segment of a large circle was preferable to the great segment of a small circle. The architects I conversed with in England denied the principle; but it was generally supported by mathematicians, and experiment has now established the fact."

It should be borne in mind that the only iron bridge then in existence in England was the cast-iron arch over the Severn near Coalbrook Dale; which arch was nearly a semicircle, having a span of 100 feet and a versine or rise of 45 feet.

Between 1786–87 Paine made three models, "one in wood, one in cast-iron and one in wrought-iron connected with blocks of wood representing cast-iron blocks." This is Paine's wording; but as the Wearmouth Bridge was copied after Paine's model or bridge afterwards built from this model, it would be clearer to say, the last model consisted of wooden blocks representing cast-iron voussoirs, spliced together by wrought-iron bands fitting into recesses in the blocks and secured by screws.

This last model he took to Paris in 1787 and presented it as a model for an arch bridge of 400-feet span to the Academy of Sciences for its opinion. Its committee, consisting of M. Le Roy, Abbe Bosson and M. Borde, the last two celebrated mathematicians, reported that an arch upon the principle and construction of the model might be extended to the span proposed—400 feet. Paine then had a couple of arch ribs of 90-feet span and 5 feet rise cast at the foundry of Messrs. Walker, at Rotheram, Yorkshire, and tested with double their own weight. On the success of this experiment Paine had a complete bridge of five arch ribs of 110 feet span and 5 feet rise made by the same founders and sent it to London, "as a specimen for establishing a manufactory of bridges to be sent to any part of the world." Soon after this his interest in bridges was obliterated by his interest in the French Revolution. His bridge was sold to satisfy his creditors.

In the memoir from which the above information is taken, dated 1803, he proposes that the Congress of the United States erect an experimental arch of 400 feet span "to remain exposed to public view, that the method of constructing such arches may be generally known." He offers to furnish the proportions of the several parts and to superintend its construction without compensation.

From this time no further consideration seems to have been given to the use of iron for bridges until about 1830, when Long and Town both suggested that their bridges could be made in iron or wood.

In 1833 August Canfield took out the first patent for an iron truss bridge.

The first iron truss bridge built is believed to be the one erected over the Erie Canal at Frankford, N. Y., in 1840, by Mr. Earl Trumbull.

"The truss was a combination of the truss and suspension principles, and was formed of—first, seven cast-iron sections or panels of about 11

feet in length and 7 in depth, cast solid, each segment consisting of an upper chord, a pair of diagonal braces and half of a hollow cylindrical post at each end (of the segment), except that the end segments had full cylindrical posts at the abutments. These semi-cylinders being bolted or clamped together in series, formed full cylindrical posts, which were flanged at the bottom, and through which were passed vertical bolts securing them to wooden transverse floor beams. Second, two wrought-iron suspension rods (1½ inches diameter) attached to the top end of the end posts, and sagging in a parabolic curve, so as to pass under and support the two centermost floor beams, and under lugs cast at proper elevations upon the posts intermediate between the centermost and the end posts, whereby such intermediates were supported. Cross-sections of chords and diagonal braces of the + formed section."

In the same year (1840) Mr. Squire Whipple built his first iron bridge. It was a bow-string truss bridge in which the compression members were of cast-iron and the tension members of wrought-iron. He took out Letters Patent for this style of bridge April 24th, 1841. A large number of bridges on this plan with spans from 50 to 100 feet have been built, and some few with spans as long as 180 feet.

In 1846–47 James Millholland built a boiler plate tubular girder 55 feet long for the Baltimore and Ohio Railroad at Bolton depot.

In 1846 Frederick Harbach patented an iron Howe truss; the top chord and braces were of cast-iron and the lower chord and vertical rods were of wrought-iron. Each chord and the main braces were hollow cylinders in pairs, the lower chord was of boiler iron riveted together as continuous tubes. The braces and chords were connected by cast-iron saddles shaped to fit the chords.

A bridge of this style, 30 feet clear span, 6 feet depth and 4 feet panels was built in 1846–7 on the North Adams branch of the Boston and Albany Railroad near Pittsfield, Mass.

About 1847–50 Nathaniel Rider built a number of bridges for the New York and Harlem, Erie and other railroads. The Rider bridge was composed of parallel top and bottom chords and multiple systems of vertical posts and diagonal ties. The top chord and posts were of cast-iron; the lower chord and ties of flat bars of wrought-iron. A wedge was inserted at the top of each post to tighten up the systems. The members were bolted together. The failure of one of these Rider bridges on the Erie Railroad in 1850, following the failure in England of the

bridge over the River Dee, influenced the officials of that road to the decision that iron bridges were untrustworthy, and to direct that all iron bridges, consisting of several Rider bridges and some which had been built by Squire Whipple, be removed and replaced with wooden structures.

"The first impulse to the general adoption of iron for railroad bridges was given by Mr. Benjamin H. Latrobe, chief engineer of the Baltimore and Ohio Railroad. When the extension of this road from Cumberland to Wheeling was begun he decided to use this material in all the new bridges. Mr. Latrobe had previously much experience in the construction of wooden bridges in which iron was extensively used; he had also designed and used the fish-bellied girder constructed of cast and wrought-iron; he had adopted on the older portion of that road the Bollman plan of bridge for short spans. For the bridges west of Cumberland he adopted the plans submitted by Mr. Albert Fink, his assistant."

In 1852 a span of 124 feet upon the Bollman plan was completed at Harper's Ferry. Bollman's truss is shown on Plate XII.

In 1851–52 three spans of 205 feet each were built over the Monongahela River upon the Fink plan. Plate XIII.

Both of these styles of bridge were of the suspension truss form, being different developments of the trussed beam.

The chords and posts were of cast-iron and the tension members of wrought-iron.

The wrought-iron tension bars had eyes at the lower ends and were connected to the feet of the posts by pins. At the top these bars had screw ends for adjustment. Plates XIV and XV show other forms of the Fink truss.

In 1852–53 Squire Whipple built upon the Rensselaer and Saratoga Railroad, 7 miles north of Troy, N. Y., an iron bridge of 146 feet clear span. (See Plate XVI.)

It was a double intersection Pratt truss with inclined end posts, a form which was known afterwards as the Whipple truss. The top chord and posts were of cast-iron. The lower chord was composed of links of wrought-iron. The web rods were round rods of wrought-iron with eyes at the upper end and screw ends below. They connected to a pin through the top chords and pass through holes in the cast-iron shoe at the foot of the posts. This cast-iron shoe had oblong trunnions at each side to receive the lower chord links. Although this bridge was proportioned for a rolling load of one ton per lineal foot only, it continued to do good service until 1883, when it was taken down, as it was

deemed prudent to replace it with a bridge of wrought-iron adapted to the requirements of the then existing traffic (the engines in use upon the road having increased in weight from engines of 35 to 40 tons to engines of 50 to 68 tons).

A few months before this bridge was taken down one of the pair of main web rods 1½ inches diameter broke with a "suspicious looking fracture;" the remaining rod, however, had sufficient strength to prevent a collapse of the truss.

From 1851–61 many iron bridges from 65 to 110 feet spans were built on the western and mountain divisions of the Pennsylvania Railroad. There were Pratt trusses stiffened with arches, the top chords, posts and arches being of cast-iron. They were built at the company's shops.

In 1857 Mr. F. C. Lowthorp built his first railroad bridge on the Catasauqua and Foglesville Railroad, Pennsylvania. It consisted of 11 spans, total length 1122 feet, supported on iron trestle towers 89 feet high. The chords and posts of this bridge were of cast-iron and the tension members of wrought-iron with adjustable screw ends.

About 1856 John W. Murphy built a Whipple bridge over the Saucon Creek for the North Pennsylvania Railroad, of 152 feet clear span. The road was never completed on this particular line, and the bridge still stands as an isolated specimen of one of the earliest forms of iron railroad bridges of the Whipple type.

In 1858–59 Murphy built for the Lehigh Valley Railroad a Whipple-Murphy bridge of 165 feet span, over the Canal at Phillipsburg, N. J. In this bridge he substituted for the cast trunnions on the post feet of the Whipple bridge pins of wrought-iron, unturned. The lower chord was formed of wrought-iron elongated links similar to the Whipple form. The main web bars were wrought-iron bars with looped eyes at each end. The counter bars were also bars with looped eyes, but the lower eye was elongated and fitted with gib castings and keys for tightening the bars.

This is the first truss bridge, as far as the author has been able to discover, which was pin connected throughout.

In 1869 this bridge was taken down, and put up as the middle span in a long wooden bridge of nine spans, at Towanda, Pa., to reduce the liability of destruction of the whole bridge by a fire.

In 1879 it was again removed, and rebuilt by substituting wrought-iron compression members for the cast-iron, turning the pins, reboring

the links, etc., and put up at Shepherd's Creek, on the Southern Central Railroad.

In 1861 Mr. J. H. Linville built a bridge on the Delaware extension of the Pennsylvania Railroad over the Schuylkill River, in which were used for the first time wide forged eye-bars and posts formed of wrought-iron sections.

In 1863 John W. Murphy built a bridge over the Lehigh River at Mauch Chunk for the Lehigh Valley Railroad, in which he used wrought-iron for both the posts and top chord sections. This is probably the first American truss bridge in which the tension and compression members were of wrought-iron. He used, however, cast-iron joint blocks and pedestals.

In 1865 Mr. S. S. Post built the first iron bridge of the style since known as the Post Truss, for the Erie Railroad, at Washingtonville, on the Newburgh branch.

Between 1865–80 a large number of bridges on this style were built. From its peculiar form, the counter system of web members consisted of a single system, which met at the center a double sytem of direct web members, thus rendering the strains ambiguous. Much of its early popularity was due to the apparent stiffness of this form of truss under moving loads—the truss being put under an initial strain by the counter system which extends the whole length of the truss.

In 1859 Mr. Howard Carroll commenced building riveted lattice bridges for the New York Central Railroad and its connections. The bridges built by him were mostly short spans, varying from 40 to 90 feet. They were well proportioned both in strength of parts and in the detail of the riveted connections, for the service then considered as sufficient.

This early work of an excellent character gave the preference to this class of bridges upon this road and other neighboring systems.

Mr. Charles Hilton, a pupil of Mr. Carroll, continued his work and extended the system to longer spans, the longest being the bridge built by him over the Connecticut River at Springfield, Mass., in 1874. It consisted of seven spans of 180 feet.

In 1864 Mr. Felician Slataper designed and built for the Pittsburgh, Fort Wayne & Chicago Railroad a bridge over the Allegheny River at Pittsburgh. It consists of five riveted lattice spans, 178 feet each.

In 1865–66 William Fairbairn & Co., of England, built the iron lattice

bridge over the Connecticut River on the Hartford and New Haven Railroad, near Windsor Locks, from designs by Mr. James Laurie (Past Prest. Am. Soc. C. E.), the longest span being 177¼ feet.

Long Span Bridges.

The era of long span truss bridges in America may be considered as dating from the building of the first bridge over the Ohio River at Steubenville, between 1863-64, by Mr. J. H. Linville. The channel span was 320 feet long and 28 feet deep. The top chord and posts were made of cast-iron. It was proportioned for a rolling load of 3 000 pounds per foot of track, a notable increase in the load heretofore in use. (See Plate XVII.) This bridge is now being removed.

This was followed by the bridge over the Ohio River at Louisville, built by Mr. Albert Fink in 1868-70, with two main spans of 360 and 390 feet. (See Plate XVIII.)

In 1870 Mr. Linville built the bridges over the same river, at Parkersburg and Bellaire, with channel spans of 340 feet.

And in 1872 he built the Newport and Cincinnati Bridge, with a channel span of 420 feet.

During 1868-74 James B. Eads built the bridge at St. Louis over the Mississippi River, consisting of two arches of 502 and one of 520 feet clear span.

In 1876 the Cincinnati Southern Railroad bridge over the Ohio River was built by the Keystone Bridge Company under specifications prepared by Mr. G. Bouscaren, the design being made by Mr. J. H. Linville. The channel span was 519 feet, the longest truss span ever built up to that time.

The channel spans of the Henderson Bridge, 522 feet, and of the Ohio River Bridge, Kentucky Central Railway, at Cincinnati, 550 feet, are the only independent truss bridges up to the present time with greater spans.

There are now in existence on American railroads over five miles of bridges with spans from 300 to 400 feet, nearly four miles with spans from 400 to 500 feet, and two and a half miles of bridges with spans exceeding 500 feet, estimated as single track bridges, all exclusive of wire suspension bridges.

The first iron cantilever bridge of importance was the Kentucky River Bridge designed and built by C. Shaler Smith in 1876-77. It is

1 125 feet long, and consists of three equal spans 375 feet. The second, also by the same engineer, was the Minnehaha Bridge over the Mississippi, near St. Paul. It was built during the winter of 1879–80, and consists of one center span 324 feet, and two shore spans 270 feet.

The third and fourth were the Niagara and Frazer River Bridges, designed by Mr. C. C. Schneider, the first being completed in 1883 and the second in 1885.

The St. John River cantilever was built in 1885. Since that time many have been built, the one of greatest magnitude being the Poughkeepsie Bridge, built by the Union Bridge Company and already described in our Transactions (Vol. XVIII, June, 1888).

3. Combination Bridges.

This kind of bridge is one in which all the tension members of the web and of the lower chord are made of wrought-iron and all or most of the compression members of wood.

In the South and West a great number of these combination bridges have been used. They are cheaper than bridges of all iron parts and are more permanent than wooden bridges, the wooden members being less liable to destruction from fire and decay, where the timber is only used in compact forms and under compressive strains. They are generally designed so that the greater part, at least, of the iron members can be used again, when it becomes desirable to renew them as wholly iron bridges. While more permanent than wooden, they are less so than iron bridges.

The question of the adoption of this class of structure instead of all wood or all iron bridges, must be determined for each locality by the relative economy as determined by their first cost and the financial ability and policy of the railroad.

Their forms are varied according to the preference of different engineers. We illustrate (Plates XIV and XIX) forms designed by Mr. Alfred Fink, and which have been in general use in past years upon many Southern railroads, especially the Louisville and Nashville Railroad.

This road has been rapidly replacing them of late with wrought-iron bridges of modern type. While excellent bridges can be made of this kind, they should only be considered as an advance from wooden bridges towards the future all-iron bridges.

4. Designing and Proportioning.

The early wooden bridges and some of the early iron bridges were not designed or proportioned upon any very correct theory. They were largely empirical structures, generally of uniform section throughout their length. Practical experience had led to improvements in methods of counterbracing against partial loads, and in providing for better detail, to obviate the crushing of the timber or slipping of the braces and to take up the looseness arising from the shrinkage of the timber.

In the early days of our railroads so able an engineer as Mr. Kirkwood writes:

"The heavy locomotives used in the freight traffic of railroads, and the great speed of express passenger trains, render massive strength in all railroad bridge structures essential. The many bridges which have been strengthened since they were built and the many which have had to be removed and replaced by stronger structures, sufficiently attest the mistaken economy of erecting light and cheap frame bridges. The blind confidence which the best of carpenters are apt to have in the raw material of their trade and in the rude geometry of its usual combinations, has rendered them unsafe guides to engineers, in the planning of bridge trusses involving irregular and shifting strains, with which their general practice cannot make them familiar. Hence it is that, even now, the engineer will always find it not merely a safe but a necessary rule, to make his bridge trusses heavier and stronger than the most intelligent professional carpenter of his acquaintance is likely to consider necessary."

The heavy engines of that day would be very light ones at the present time.

The science of bridge proportioning was yet undeveloped. The best that the engineer could do, was to make the bridges stronger than heretofore, solely on the facts brought out by past experience.

The first attempt to analyze the strains in skeleton bridge trusses appears to be the work of Mr. Squire Whipple, the inventor of the Whipple bridge—the prototype of the present most generally adopted form of truss now in use in America. His book was published at Utica, N. Y., in 1847. It does not seem to have been widely known till a much later date. For Mr. Herman Haupt, who had an extensive experience in building wooden railroad bridges, and whose treatise on bridge construction, published in 1851, was long an authority, states in his preface:

"If any work exists containing an exposition of a theory sufficient to account generally for the various phenomena observed in the mutual action of the parts of trussed combinations of wood or metals, the author has neither seen nor heard of it." "The best works on the subject of construction that have fallen into the hands of the author contain but little that will furnish the means of calculating the strains upon the timbers of a bridge truss or of determining their relative sizes."

While the principal material of construction was wood, its cheapness and the practical reasons for making the principal members of the trusses of uniform section, made the necessity of proper proportioning less apparent. The advent of iron construction, involving the use of a material of a more expensive character, compelled a better consideration of the relation of the principal parts of a truss to the duties to be performed.

Besides the defective knowledge as to the calculation of the strains, there was a like deficiency in regard to the strength of materials in the forms adapted for skeleton structures, and as to the effects of the concentrated engine loads.

The use of a uniform load of one ton per foot of track for designing all parts of a bridge was in general use some years after 1860 and on many bridges up to even a later date. It was not till about 1870 that a heavier uniform load was adopted for the floor system than was used for the main trusses.

The rapid growth of the traffic on our roads and the constantly increasing loads upon our cars and engines were not fully appreciated until very recent times.

Up to about 1874 the designing and the construction of bridges were, almost exclusively, in the hands of the several bridge companies. Each of these companies had its own peculiar style of bridge ; consisting either of a special form of truss, as the Bollman, Fink, Linville, Post, etc., or in the use of patented forms, as the Phœnix, Linville & Piper and American Companies' posts, and other detail parts.

Each company also had its own special geographical field, or lines of railroad, giving it the preference.

Even at points where they did meet as competitors, it was rather as advocates for their special trusses or forms of parts, than as competitors upon any very definite specification.

Though many excellent bridges, considering the state of the art, were built under this system, there were also many very inadequate structures made.

The construction of the St. Louis Bridge marked a most important advance in the development of American bridges. The investigations made, during the building of the St. Louis Bridge, into the strength and other properties of the materials of construction, and especially the testing of full-sized members and their detail connections, not only ad-

vanced very much our knowledge of these matters, but also gave an impulse to such investigations which has continued to the present. The erection of the St. Louis arches, by building out from the piers, was the first practical solution of the cantilever principle on a large scale. The erection of two balanced cantilevers, each over 250 feet, with ease, safety and economy, made clear to the mind of engineers that the cantilever was the economic method of erecting long spans over deep gorges or rivers, where ordinary methods of scaffolding would be too expensive, or subject to great risks, or where navigation forbids the obstruction of the waterway.

The construction of the Cincinnati Southern Railroad also marked an important step in the improvement of bridges and their construction.

The bridges upon this road, including the large span at Cincinnati over the Ohio River, and the crossing of the Kentucky River gorge, comprised the first work of magnitude that was offered for competition upon specifications drawn by an engineer acting exclusively in the interest of the Railroad Company.

Two points in these specifications of Mr. Bouscaren were especially important ones : 1st. The use of an engine and train diagram for the live load, instead of the prevalent method of a fixed uniform load per lineal foot of bridge or floor. 2d. The requirement that full-sized compression members of the different competitors be submitted to test, to determine their ultimate strength and elastic limit.

About 1874, the bridge engineer, acting in the interest of the railroads, began to exert a beneficial influence. New competitors as bridge contractors were also entering the field.

These influences all worked together to make a great improvement in the general character of the structures built after that date.

The failure of the Ashtabula Bridge in December, 1876, not only alarmed the general public, but also shook the blind confidence of the railroad companies in their existing bridges.

Upon the principal roads a general examination was made into the character and condition of their bridges by competent engineers with very beneficial results. Not only were many defective structures rebuilt or strengthened, but the defective methods heretofore in use, both in the proportioning of parts and in constructive detail, became apparent.

Floor systems designed for a uniform rolling load, regardless of the length of the panels, were found to be inadequate for the concentrated

engine loads in use. For the same reason the counterbracing was also deficient. Broken castings emphasized the unreliability of this material for bridge purposes. A careful examination and analysis of the details, upon which the capacity of the structures depended as much as upon the sectional areas of the several members, showed the need of greater attention to such matters.

The Erie specifications drawn by the writer in 1878, embodying the results of his experience in the designing of bridges, their shop construction, the testing of material and the study of existing bridges and their defects, was the first general specification covering the designing, proportioning and detail of construction with that completeness necessary to give the railroad company the full advantage of the competitive method, with a certainty that the resulting structure would in all ways be up to the advanced state of the art.

It was the first paper on bridge construction in which that relic of ignorance, "the factor of safety," was entirely omitted.

It definitely specified the working strains to be allowed on the different parts of the structure according to the service they were to perform.

While aiming to obtain the best results for the railroad company, by clearly specifying for what loads and under what working strains each part should be proportioned, and the perfection of workmanship which must be adhered to, it left to the contractor perfect liberty of selecting his own plan of structure, as to form of truss, relative proportions of depths and panel lengths and methods of detailing.

These specifications were adopted very widely. They have been modified and extended from time to time, by the author and other engineers, as advancing knowledge and experience justified.

The competitive struggle of the bridge contractors has still been allowed free scope in certain directions, but has been modified by the new factor, the bridge engineer acting in the interest of the railroad companies.

The bridge engineer acting for the contractor has for his principal incentive, the selection of a design which, while complying with the specifications in all ways, can be built at the shops for the least money expended in time and material, and which can be erected at the particular location with the least cost and risk.

The bridge engineer acting for the railroad company, while giving

all due weight to the aims of the contractor, is seeking to get a structure which will give the best results after its completion, for operation and maintenance, without undue cost.

As the engineer of the contractor may at some other time be the engineer acting for the railroad and the reverse, these different views are constantly reacting on each other, to the great benefit of our bridge structures.

The competition to-day between the different bridge companies has been largely reduced to the question of shop management, or the relative cost of turning out so many tons of bridge work in a certain limited time.

Practical bridge engineers have given little weight to the theoretical examination of the economical truss. They have recognized as fallacious the deductions usually drawn by theorists from the neglect of most important factors in such investigations. True economy does not necessarily mean minimum weight of material. The relative weight of the various classes of iron in a bridge, the cost of manufacturing each form, rolled, riveted or forged, facility of transportation and erection and other factors must be given their due weight; and even these factors must change from time to time, and do vary even for the same time at different manufactories. While the tendency towards economy of material due to changes in forms and proportions has been fully recognized, the comparison of alternate or competitive plans has led to the proper proportions of true economy with far greater certainty.

The engineer who is constantly employed in preparing competitive designs, can choose almost instinctively the proper proportions to give his design, selecting the panel lengths, depth of truss and skeleton system best suited to give the proper relations between the floor, web and chord systems of the bridge, with due regard to a suitable arrangement for the lateral and sway bracing.

No fixed proportions can be permanently established suitable for all cases; for in addition to the varying elements before mentioned, the question of head room, waterway and skew often necessitate special changes. There must also be a certain undefined harmony in the relation of the several parts, in order that the disproportion or inefficiency of the connection of parts may not render the design a defective one.

A successful bridge engineer, from the American point of view, must be something more than a mere calculator of strains. That is the most elementary part of the duty, and does not come within the province of designing. After the selection of the skeleton form and relative proportions of panels, depths and widths of span, a very moderate knowledge of mechanical mathematics would enable any one to determine the strains in an American bridge. He must, in addition to his knowledge as to the effects of varying forms and proportions, have a full knowledge of the capacity of his forms and their connections, and also of the practical processes of manufacture and erection. He must know how his design can be made and put together, and whether it is so harmonized in all its parts and connections, that each part may do its full duty under all possible conditions of service.

In addition to knowing all the elements that make up a perfect design, he must have the instinct of designing or the power of adapting his knowledge to any individual case, in order to obtain the best or the desired result.

Then experience, observation and a sharp competition with men of like knowledge and instinct will give him his position as a bridge engineer.

The equivalent uniform loads given in the following tables (1 and 2), represent in a general way the live loads used in designing bridges during the past, and the maximum live load now being adopted by certain of our railroads.

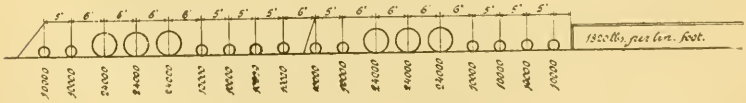

Fig. 1.

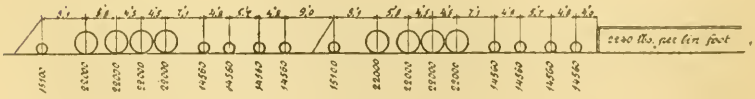

Fig. 2.

TABLE No. 1.

Spans. Feet.	From Moments—Loads for Longitudinal Girders.				
	Uniform Load of One Ton per foot.	Cincinnati Southern Train Load. 1873. Fig. 1.	Erie Train Load. 1878. Fig. 2.	101 Ton Consolidation, followed by 3 000 pounds. 1884. Fig. 3.	Heavy Grade Lehigh, followed by 4 000 pounds. 1889. Fig. 4.
5	2 240	8 000	8 800	12 000	10 000
10	"	4 790	5 290	6 750	9 610
15	"	4 470	5 280	6 670	9 600
20	"	4 320	4 640	6 190	8 560
25	"	3 970	4 400	5 850	7 950
30	"	3 770	4 200	5 470	7 290
35	"	3 540	4 080	5 110	7 040
40	"	3 400	3 850	4 760	6 690
45	"	3 230	3 700	4 550	6 000
50	"	3 140	3 590	4 350	5 870
55	"	3 050	3 450	4 220	5 680
60	"	2 960	3 400	4 110	5 480
70	"	2 920	3 220	3 940	5 110
80	"	2 920	3 260	3 950	4 800
90	"	2 860	3 250	3 970	4 560
100	"	2 860	3 230	3 910	4 520
125	"	2 780	3 150	3 830	4 740
150	"	2 670	3 070	3 740	4 760
175	"	2 590	2 990	3 650	4 670
200	"	2 530	2 910	3 590	4 640
225	"	2 440	2 850	3 520	4 550
250	"	2 390	2 800	3 480	4 470
275	"	2 360	2 750	3 430	4 380
300	"	2 340	2 710	3 390	4 290
350	"	2 240	2 660	3 230	4 250
400	"	2 200	2 610	3 180	4 200
450	"	2 160	2 570	3 160	4 160
500	"	2 120	2 540	3 150	4 120

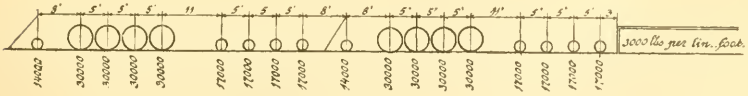

Fig. 3.

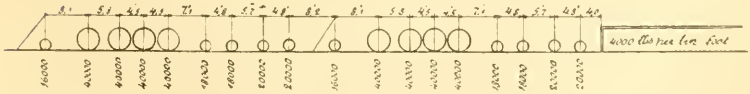

Fig. 4.

TABLE No. 2.

Equivalent Loads for Erie Train Load.

Span—Feet.	From Moments.	From Double Shear.	From Single Shear.
5	8 800	5 280	9 680
10	5 290	4 620	7 200
15	5 280	4 210	6 240
20	4 640	3 830	5 400
25	4 400	3 588	5 040
30	4 200	3 400	4 740
35	4 080	3 270	4 540
40	3 850	3 175	4 300
45	3 700	3 090	4 220
50	3 590	3 030	4 240
55	3 450	3 000	3 930
60	3 400	2 970	3 830
70	3 220	2 960	3 680
80	3 260	2 870	3 620
90	3 250	2 820	3 570
100	3 230	2 750	3 450
125	3 150	2 570	3 280
150	3 070	2 450	3 180
175	2 990	2 400	3 090
200	2 910	2 370	3 000

Table No. 1 gives the approximate equivalent uniform loads for the moments due to several forms of typical live loads upon spans of different lengths. The approximate dates on which these loads were introduced are also given.

These equivalent loads represent in a general way the growth in the required strength of our best bridges, from the earlier days of iron bridge building to the present time.

This table will make clearer than any other method of description the difficulties American bridge builders have had to contend with. In face of the constant progress and bettering of our bridges we have had to meet rapidly increasing demands for greater loads; with too often an increasing reluctance on the part of the authorities to build for anything but the immediate present requirements.

It will be readily seen from this table why our shorter spans are being overtaxed, and why in all our older bridges the floor and counter systems are too weak. The above table represents simply the equivalent loads, which will render the same maximum moments as the corresponding train loads. To provide for the maximum action of any miscellaneous loading, we must also consider the equivalent loads, which rep-

resent the maximum shears. For this purpose we need two additional equivalent loads—one to represent the maximum end-shear upon a single span of girders or trusses, which we will call the "single shear;" and another to represent the maximum shear which can occur at the supports of two adjacent spans or panels of girders or trusses, which we will call the "double shear."

The single shear equivalent loads give us the loads on the end members of the trusses and girders. The double shear equivalent loads give us the loads on the supports, whether they be columns or cross-girders.

Table No. 2 gives for one engine the equivalent loads corresponding to the maximum moments, and maximum double and single shears. They differ from one another for each different span of truss or panel. We must therefore use at least three series of uniform loads to represent even approximately one engine load.

The maximum moment of a miscellaneous loading being generally at some other point than the center of the girders, the equivalent load giving the maximum moment will be different for a girder with uniform flanges and for one with flanges of uniform strength.

To represent the action of a train load properly by this method, we should need four different series of equivalent loads, all varying for each span.

The apparent simplicity, therefore, of using equivalent uniform loads for proportioning our structures, is a fallacious one; when applied to partial loads it becomes far more confusing and untrustworthy.

The old method of computing the strains by use of panel loads and equivalent uniform loads (and too often using only the one derived from the moments) has gradually given way to a system much simpler, more accurate and also as easy of application.

The principles and application of the new method were worked out independently but simultaneously by Mr. Robert Escobar, C. E., of the Union Bridge Company, and the author, some years ago (1880).

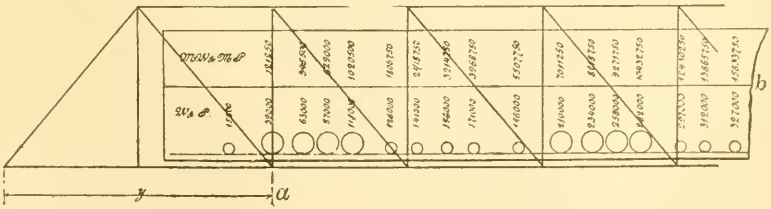

Fig. 5.

Let Fig. 5 represent a skeleton truss of a single system of triangulation and n panels.

The maximum shear at any point a will occur, when one wheel of any miscellaneous series is on the panel point, and when the total weight of train on the bridge, W and n times the loads in front of a (not including the one at a), are the nearest to an equality; or, in other words, calling P the loads in front of a, when $W - n\,P$ is a minimum.

Similarly the maximum moment at any point a will occur when the train is so placed that $W : P :: L : y$; L being length of span.

Application of the method: Lay down the engine and train load on a card b to the same scale as is used for the diagram of the truss. We will assume that the train is headed to the left hand. Commencing at the left hand, write over each wheel the summation of the weights up to that point; on another line above write the moments of all the wheels to the left of the particular wheel about that wheel.

The first line will give the weights P and W for any position of the train, so the placing the diagram to obtain $W - n\,P =$ a minimum, or $W\,y = P\,L$ is readily done.

The second line above will in like manner give the moments $(m\,P)$ or $(m\,W)$ or the moments of the loads about a, or the end of the span. Where the last moment does not correspond to the end of the span, it must be increased by $W \times d$, the distance from the last wheel to the end of the span.

The maximum shear at (a) will then be: $\dfrac{(m\,W) - n\,(m\,P)}{L}$

The maximum moment at (a) (different position of train) will be—

$$\frac{(m\,W)}{L}y - (m\,P)$$

An absolutely mathematically correct result can thus be obtained in a very few minutes, after a little practice. The preparation of a diagram for any engine and train load is only the work of a few minutes' time.

The method has therefore the merit of being no longer than that by uniform loads, and is far more exact, for it gives perfect accuracy for all positions of the loads, without any of those allowances for excess, etc., which are needed in the old methods.

This method has never been published before, but is in general use among the American bridge companies, by gradual transfers. The method cannot be applied to multiple system trusses, but it can be to girders with a continuous web.

The advocates of the method by uniform loads make the claim that from the rapid increase of the loads upon our cars we are fast approaching the time when the loads on the wheels of our cars will equal the loads on our engine wheels, totally forgetting that the distance between the wheels is as important an element as the weights. As long as the engine drags the train by the adhesion of its wheels, its weights and their concentration on a short wheel base must produce effects that will always exceed those produced by the car loads.

The problem before us, therefore, is not the selection of some maximum uniform load which will give us imperfectly proportioned structures; but how far can the designers of engines go in their loads and spacings of the driving wheels? Is there any maximum, or can they go on indefinitely? The want of harmony or rather the independence of the different departments of our railroad organization, has left this important question to be solved by the tentative system, rather than by any scientific study or investigation. That all of the roads in our vast system should adopt one maximum train load for designing their structures, even could such a maximum be decided upon, cannot be expected. The peculiarities of each road as to grade, alignment, traffic and financial strength must lead to variations.

That every road should build the best and strongest bridges that a sound and far sighted financial management will admit, is true. But we cannot expect more than this. Bridges, therefore, will continue to be built of various capacities. Our duty as engineers, is to strive to advance their capacity as far ahead of present needs as the funds available will permit.

We may accomplish this purpose best by bearing in mind certain principles:

First.—By selecting for our live loads typical train loads, where the engine loads will bear a reasonable relation to the following train load. The growth of the traffic and development of the train loads will then be more likely to increase the strains on all parts of our structures proportionately, and they, therefore, can be used till they have reached their maximum justifiable duty; instead of removing them, as is now so frequently the case, because the floors, counters or detail parts are overworked.

A recently constructed bridge is known to the author, which was designed for a very heavy load, so badly arranged that parts of the

structure are only of a capacity equal to the present service of the road, while other parts have double this capacity. The material in this bridge, properly arranged, would have given a structure at least fifty per cent. stronger for future traffic, than it can now do.

Second.—By not increasing our unit strains in proportion to our faith in new forms and new material, where necessity does not compel us.

To-day we are able to get some steel forms at a less price than iron ones, and some others at the same price. Before long we will get all steel cheaper than iron. Why not take advantage, when we can, of this advance towards a stronger material to increase the capacity of our bridges? Why endeavor to push the strains up to the limits of our faith in its capacity for the various forms?

Camber.—The desired camber is obtained by making the length of the top chord panels to exceed those of the lower chord by an amount slightly in excess of the sum of the compression and elongation of the top and bottom chord panels respectively, under the working strains.

For strains in the top chord of 8 000 lbs. per square inch and 10 000 lbs. for the tension of the lower chord and a modulus of elasticity of 24 000 000 lbs., the sum of the changes in the two chords would be

$$\frac{18\,000}{24\,000\,000} = \frac{1}{1\,333}.$$

This amount of increase would, however, only provide for the deflection due to the chords. To provide for the additional deflection due to the web system of the trusses, and to allow some excess, this amount is usually increased one-third, which makes $\frac{1}{1\,000}$, which corresponds very closely to "$\frac{1}{8}$ inch for every 10 feet of panel."

This becomes a very convenient rule for the shops where English measures are employed. It is entirely independent of the depth of the truss and only requires modification when the unit strains differ from the above averages.

Lateral and Transverse Bracings.—The general practice in America is to brace our trusses for a definite amount of lateral force per lineal foot of the structure, instead of assigning so many pounds of wind pressure per square foot of exposed surface.

This has arisen from two causes: 1st. The indefiniteness of the methods of estimating the exposed area, thus giving unfair advantages to the less scrupulous competitor. 2d. The recognition by practical

engineers that there are other forces requiring lateral resistance, even in districts that may be windless.

The rule formulated for the Erie specifications in 1878 and now generally adopted, was as follows:

"To provide for wind strains and vibrations, the top lateral bracing in deck bridges and the bottom lateral bracing in through bridges shall be proportioned to resist a lateral force of 450 pounds for each foot of the span, 300 pounds of this to be treated as a moving load.

"The bottom lateral bracing in deck bridges and the top lateral bracing in through bridges shall be proportional to resist a lateral force of 150 pounds for each foot of the span."

"In no case shall any lateral or diagonal rod have a less area than ¾ of a square inch."

While the above rules correspond approximately to 30 pounds pressure on the projected surface of a train of cars and the trusses, it was only selected after comparing the results with existing bridges up to 200 feet spans, which gave satisfactory lateral action under rapidly moving trains.

5. Strength of Material and Parts of Skeleton Structures.

In the early days of iron bridge building, the knowledge of the strength of materials was very limited. The early experiments of Fairbairn and Hodgkinson comprised about the extent of our knowledge of the strength of iron.

Crude tests made upon grooved specimens of a small size, lead to much misconception of the capabilities of American bar iron. In a pamphlet by Mr. Wendell Bowman on his Harper's Ferry Bridge, built in 1851–52, he states

"the tensile strength of the best American bar iron tables as 80 000 pounds per square inch. Its practical value is generally rated at about one-fourth the nominal value. In the diagram (*his strain sheet*) the highest value of iron is 16 000 pounds, being reduced below any probable rate of fibral separation in any previous data."

Even as late as 1870, Captain Eads was assured by the manufacturers of bar iron that there was no difficulty in furnishing bars of any size, capable of standing 60 000 pounds per square inch, and he made the contract for the St. Louis Bridge with this expectation, only to be disappointed.

In 1878, when the requirements for the strength of bar iron as contained in the Erie specifications, making a reduction in the ultimate strength as the size of the bars increased, was submitted to the most

experienced iron masters for their criticism, they were condemned by all with but one exception, Mr. Andrew Kloman, of Pittsburgh: 1st, as being entirely too low in tensile requirement for good bar iron; and 2d, in making any allowance for increased section of the bars; "there being no reason why the fibers in large bars should not be as strong as in the smaller."

It may be unnecessary to state, that when these specifications were enforced, and the acceptance of the material became dependent upon the test made on either the bars themselves or specimens cut from the same, it was found that only the best and highest priced bar iron could meet the requirements.

The general tendency, since then, has been to relax them somewhat for a broader competition.

The Phœnix Iron Company and the Keystone Bridge Company had crude testing machines at their shops previous to 1870.

They were used to test the strength of eye-bars and wrought-iron columns. In some of the more important structures, all the eye-bars were tested up to a strain of 20 000 pounds per square inch.

While these machines were crude, they did serve to develop the detail proportions of the eye-bar and column used by these companies. The use of steel and new forms of members in the St. Louis Bridge, in connection with the importance of the structure and the yet untried method of erecting as a cantilever, impelled the engineer and the contractors to unusual efforts to determine the capacity of the material and of the forms to be used.

Of the many thousand tests made during the construction of this work, but a limited number have ever been published; the general deductions, however, soon passed into accepted doctrine, and developed a keener desire for a more refined knowledge of the influence of form and proportion upon all our bridge members.

The eye-bar had been well developed under the crude tests previously made by the early bridge companies.

Full sized tests had also been made on a limited number of wrought-iron columns. But no marked progress was made in the forms of compression members, till after the tests of Mr. Bouscaren on the forms then in use. His tests not only showed the inapplicability of Gordon's formula, but also the possibility of much better results by improving the detail of the open and box columns; the only forms of columns

without a proprietary claim and the ones best adapted to the American style of bridges; as they gave ready facilities for connections without the use of cast-iron.

From this time forward rapid progress has been made in the detail and proportion of all our full sized bridge members.

To-day every first-class bridge manufactory has its complements of testing machines, to test with all the refinements either samples of the material or full sized members in compression, tension or transverse strain. Our knowledge of the strength and capabilities of our material and of the usual forms employed in the American style of bridge is such, that no first-class bridge company in America hesitates to accept the clause now general in all specifications, that "full sized members may be tested to destruction," with the sole proviso that the expense of testing and cost of the piece shall be paid for by the purchaser if it satisfies the requirements of the usual specifications.

This positive knowledge of the capacity of our full sized members marks one of the great advantages of our system of bridges over all others. Our "factor of ignorance" has been reduced to this extent; a no mean portion.

We, therefore, have a right to claim, that as our working strains are as low and in many cases lower than those used in Europe, with our more perfect knowledge of the strength of our members, we have in our first-class structures a greater factor of security than prevails in European bridges.

In the appendix will be found tables of the abstracts of the more recent tests on eye-bars and compression members.

6. Manufacture of Bridges.

The great demand for iron bridges in a country as vast as the United States caused the formation of companies for the special purpose of manufacturing and erecting bridge structures. They established special plant and created a corps of engineers, who have made this branch of engineering a special one.

There are to-day in America more than forty bridge building companies manufacturing railroad and highway bridges.

Of these, at least a dozen are capable of constructing bridges of every size in a first-class manner.

The shops, which can be called especially shops capable of construct-

ing the largest class of railroad or highway bridges, are capable of turning out by the year 125 000 tons of bridge work.

It would be safe to estimate that all the shops could turn out 200 000 tons, or approximately 80 miles of 100 feet spans of single track railroad bridges, per year, if their plant were devoted exclusively to this work. Other iron work, as roofs, iron buildings, piers, elevated railroads, etc., however, make a considerable figure in their yearly output. Some few of the bridge shops have been constructed to do riveted work almost exclusively.

The typical American bridge shops are, however, fitted to do any class of bridge, girder or roof work, whether it be exclusively riveted, or combined riveted and pin-connected work. Each company has, therefore, the following arrangements for receiving the iron, and putting it through all the processes to the completion ready for shipment:

First.—Receiving yard, where the iron for each bridge is properly classified and stored.

Second.—Department for straightening, where pieces can be straightened with more accuracy than can be obtained directly from the rolling mills.

Third.—Template and pattern shop, for preparing the templates for the rivet and pin holes and crude shapes and dimensions of all pieces, with the proper allowance for final tool finish.

Fourth.—Laying out shops, where each individual piece of iron is carefully marked in accordance with the templates.

Fifth.—Punch and shear shop, where all the iron is punched and sheared.

Sixth.—Fitting up shop, where all the iron for riveted members is assembled and bolted together ready for the riveting machines.

Seventh.—Riveting shop, with its proper complement of air, steam and hydraulic riveting machines.

Eighth.—Machine shop for planing, boring, turning, etc., to complete the finished bearing surfaces of all the members.

Ninth.—The upsetting and forge shops for making eye-bars and all forged parts required.

Tenth.—Painting and shipping sheds and yard.

The aim in the construction is to pass the material from the time of its receipt from the rolling mill to its final shipment through the necessary steps with as little waste labor in handling as possible and to perform all work by machinery in preference to hand labor.

While the laying out and riveting are done with all care and accuracy, the lengths of the members and the fitting of the same together do not depend upon the accuracy or neglect of these processes.

The length of all abutting members and distances between centers of pin holes are determined finally at the machines for planing the abutting ends or for boring the pin holes.

Machines for operating on each end of such members are provided with iron beds and extremely accurate methods of setting the same to any required distances.

Such machines once set and operated with proper precautions in regard to uniform temperature guarantee great accuracy of duplication of all similar parts.

It is sometimes claimed that there is an error in lengths of parts due to the usual allowances for play of the pins in the pin holes, varying from $\frac{1}{64}$ to $\frac{1}{32}$ of an inch, according to the size of the pins. This is an error; this allowance is provided for by either measuring from out to out of the pin holes for tension members and from the inside of the pin holes for compression members, or by taking it into consideration where the lengths are given from center to center.

The surety of the fitting of the members of even the largest structures after they have passed through a properly organized bridge shop is such that no assembling of the finished members of a structure is ever made at the shops, except in extremely crooked and complicated structures, and even then not as a whole, but only sufficient to test the fitting of specially intricate connections.

The rapidity of the erection of our structures and the satisfactory manner in which they come together in the field without any tool work prove the certainty of the American methods.

A pin connected truss is composed of the following class of members:

1. Top chord sections.
2. End posts.
3. Intermediate posts.
4. Pedestals.
5. Lower chord bars.
6. Diagonal web bars.
7. Pins.

The top chord sections and end posts are usually similar in form. They are usually formed of two or more vertical channels, either of rolled forms, or built of angles and plates, connected on top with a cover plate and on the bottom with lattice bars.

In the best practice most of the material for these members is concentrated in the vertical channels; the top plate besides acting as a cover and stiffener during transportation, gives an unbalanced section sufficient to counteract the tendency to deflect due to the weight of the piece. The pin holes are reinforced by additional plates to reduce the bearing pressure to the allowed limits. The amount of rivets in these pin plates and at the ends of the sections is made large enough to distribute the localized pin pressure over the section of the whole chord.

The chords are spliced at one side of the pin hole to insure a full pin hole for more convenient boring and to enable the rivets in the splice to be driven after the parts are assembled.

After these chords are riveted together in the shop the abutting ends are carefully tooled to square surfaces and exact lengths and the pin holes bored. The accuracy of the finished lengths and diameter of pin holes and the character of the tool work having been passed upon by the inspector, the piece is marked, painted and stored for shipment.

All posts are made with pin bearings only. The intermediate posts are usually made of the open form, consisting of two channels, either rolled or built up of plates and angles, latticed on both sides. The sizes of the end or batten plates and the lattice bars have been evolved from experience in transportation and from the study of the results of full sized tests.

The ends of the posts are usually forked (see Fig. R, Plate XXVII), in order to pack between them the tension bars to produce a more compact joint.

These ends are stiffened by thickening with plates of sufficient length to make the compressive strength of these ends fully up to the capacity of other parts of the same post.

The pedestals are riveted members formed of plates and angles. They are proportioned for the proper bearing pressure and with rivets sufficient to meet all the strains. Their base is determined by the allowed pressure upon the masonry.

Under the pedestals of one end of the span, nests of friction rollers proportioned according to an accepted rule are placed, to provide for the expansion and contraction of the trusses.

Lower chord bars.—These are flat bars with forged eyes at each end. These bars have, in the past, been made by various methods. But economy in their manufacture has developed special upsetting

and die forging machines of great power, using either steam or hydraulic pressure, by which they can be made with great certainty and at a very moderate cost. Only by such perfected machinery and great experience in the working could such accurate results as are demanded by our ordinary specifications be attained. The center of the heads must be in the center line of the bar and the heads symmetrical to this line. The heads must be clean, smooth and of proper thickness. The necks must be of proper shape and upset so as to be full in all dimensions. The lengths between centers of the heads must be very close to correct length or when bored there will not be the proper excess for the eye. To produce such bars economically all reheating and reworking to correct errors must be avoided.

The process, of course, requires the best material, whether of iron or steel.

All steel bars are annealed after they have been forged.

Our summary of some recent tests made in the ordinary course of the execution of contracts shows how perfectly these bars answer to the full capacity of the material.

Web-bars.—These are similar to the lower chord bars, except the counter-rods, which are made in two pieces and connected by sleeve nuts or turnbuckles. It is the best practice to secure these turnbuckles or sleeve nuts firmly in their position after these rods have been properly adjusted.

Pins.—These are made of round iron or steel, turned perfectly smooth, and to accurate diameters, with an allowance of $\frac{1}{50}$ inch for pins under $4\frac{1}{2}$ inches diameter, and $\frac{1}{32}$ inch for larger pins, less diameter than the bore of the pin holes. These pins have wrought-iron nuts at each end, which are tightly screwed up and secured after the bridge is connected together.

The Iron Floor System of our bridges consists of cross floor beams at each panel point, with a pair of stringers under each track. These floor beams and stringers are usually plate girders. This system is connected to the trusses usually in one of two ways ; either by suspending from the pins or by riveting to the vertical posts for through bridges ; or for Deck Bridges, either resting on the top chords at the panel points, or by riveting to the posts. Each particular case must be worked out to accomplish the purpose best, considering the local requirements of clearance for floods or navigation, head room, etc.

Lateral and Sway Bracing. — The horizontal and vertical bracing between the trusses have generally in the past been similar to the other characteristics of our type of bridge, rectangular systems of tension rods and compression struts connecting to the pins of the main trusses. Of late years the preference for more rigid forms for the parts of these comparatively light trusses has led to the use of angle iron bracing instead of tension rods.

7. Erection of Bridges.

The great risk involved in erecting important structures over rivers subject to sudden floods, ice jams and similar dangers, emphasizes the importance of having a style of structure which can be rapidly and surely assembled without detriment to the perfection of the workmanship or accuracy of the connections.

The American pin-connected bridge is especially suited for this purpose, the connection of the comparatively few compact members being readily completed after they have been hoisted into position by the driving into place the connecting pin and securing the same by setting up the nuts.

The only rivets to be driven in the main trusses are usually those in the plates connecting the top chord and end post sections. But these do not in any manner affect the perfect action of these members, which depends entirely upon the machine surfaces of the abutting ends and the turned pins. They serve solely to insure the bearing surfaces from side displacements through any accidental blows. They can be connected by bolts, or the rivets may be driven at any time, even after the bridge is in use for trains.

The rivets in the floor system can likewise be driven after the bridge is free from the false work.

The erection, therefore, of a pin-connected bridge is very readily performed, and with men experienced in this class of work with marvelous rapidity.

The importance of the erection with rapidity compels a careful design for the detail of all connections, and also often gives weight in favor of a special style over all others.

The additional security of the cantilever method of erection favors that form of bridge for long spans over deep gorges or rivers where the risks are unusually great.

The selection of a cantilever style of bridge for spans less than 500 feet is entirely unjustified, except where the special merit of this method of erection outweighs the objectionable features of the system—excessive deflection and reversal of strains.

The rapidity with which our pin-connected trusses can be assembled and swung clear of the supporting false works has been so often demonstrated, it would be useless to enumerate examples.

A 250-foot span railroad bridge has been erected in sixteen working hours, taking the material from the storage yard 1 000 feet from the bridge, without any emergency demanding unusual exertions.

It would be no exaggeration to assert that any span up to 250 feet could be erected within the limits of one day-light, so as to be self-sustaining and independent of all risks from loss of the false works.

The erection of the two channel spans of the Cairo bridge is an example of rapid erection, which illustrates the possibilities of the American system of construction.

These spans were each 518 feet 6 inches center to center of end pins; 61 feet deep center to center; 25 feet wide center to center of trusses; panel lengths 30 feet 5¾ inches. Total weight of one span, 2 055 200 pounds.

The first span was erected in six days. After this span was erected, the false works were taken down, the supporting piles drawn and redriven for the second span, the false works again put up and the second span erected.

The whole time covering the erection of the two spans and moving the false works was one month and three days, including five days lost time, waiting for the completion of the masonry.

The false works and traveler were in position ready to commence the erection of the second span by 2.30 P. M., October 30th, 1888. At 2.50 P.M., November 3d, the trusses of this span and the top bracing were all connected. No work was done at night. The material was run on trucks about 1 025 feet from the storage yard to the nearest end of the span being erected. About twenty-four men were employed in delivering the material and fifty in erecting and connecting together.

The false works stand about 104 above low water. The bents being 72 feet high above the capping of the piles. The depth of water at its low stage is about 20 feet. The piles were from 50 to 75 feet long.

The erection of these spans was done by William Baird & Co., sub-

contractors, under the Union Bridge Company, for this part of the work. Plates XX to XXIV are reproductions of photographs taken every twenty-four hours to show the daily progress of the erection of this second span.

8. Typical American Railroad Bridges and their Relative Merits.

While the pin-connected bridge is recognized as the one which can be especially called the American type, American bridge engineers have not failed to "search all things and hold fast that which is good," not only within our own experience but within that of European practice. We have had one great advantage over our European brothers, that their works and practice are so much prompter and fuller given in the technical literature of the day. We have had another advantage from our allying the several branches of designing, manufacturing and erecting more intimately than is usual in European countries. We have been less liable, therefore, to give undue weight to the claims of theorists or the riders of hobbies. Nearly every imaginable form of structure has been tried in the past by some enthusiast.

After an experience unequaled for its variety and extent, we are arriving at a very general uniformity in the styles of bridges to be adopted for different spans.

Ten years ago, of a thousand bridges of which no two would be alike, a practical bridge engineer could almost invariably determine the designer and manufacturer, either by the general style of the bridge or by the use of some peculiar forms of member or detail.

To-day the use of a peculiar form of truss, member or detail is due more to the special necessities of the case than to the idiosyncrasies of any particular individuals.

While no two bridges to-day are exactly alike, there is a general approximation to certain broad types in styles and detail.

Plate Girders.—These are very generally used for spans up to 65 feet, or lengths which do not require more than two ordinary flat cars 33 feet long to transport from the shops to the bridge site.

Some railroad companies have extended their maximum length for plate girders to three car lengths, or about 100 feet.

These girders are riveted completely at the shops. Plate girders completed at the shops in this manner give excellent satisfaction. For

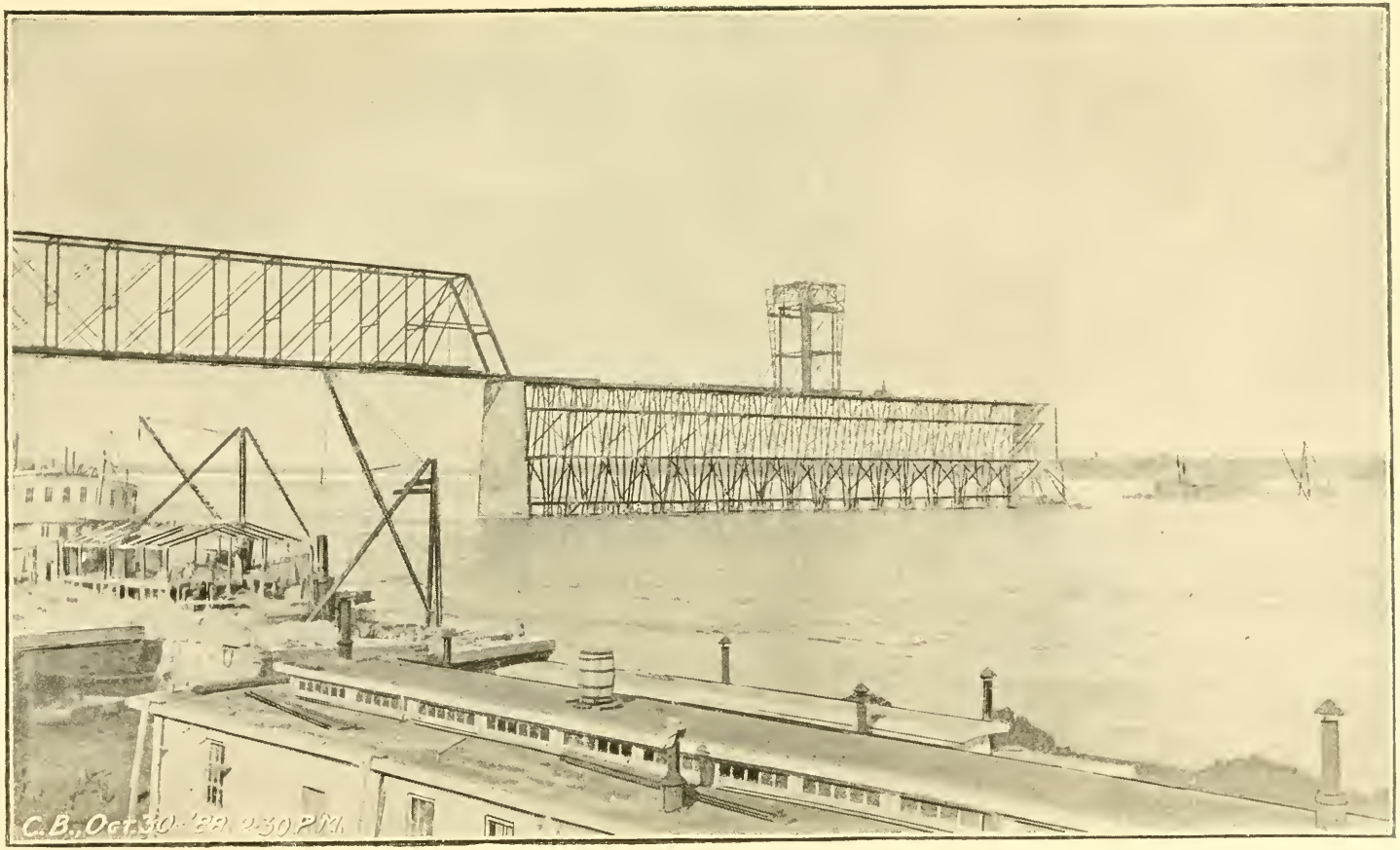

PLATE XX. CAIRO BRIDGE. Vol. XXI, p. 40.

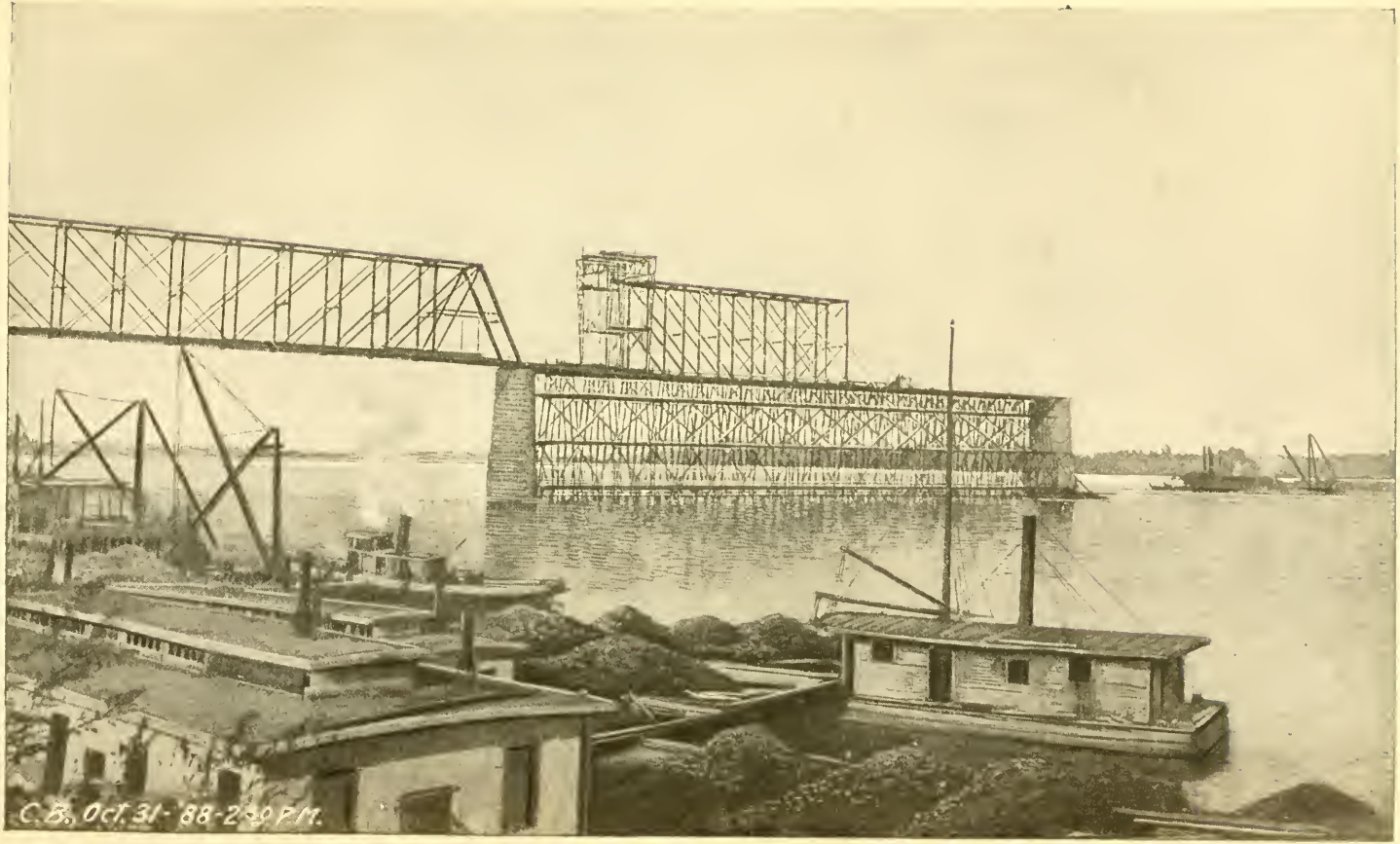

PLATE XXI. CAIRO BRIDGE. Vol. XXI, p. 40.

PLATE XXII. CAIRO BRIDGE. Vol. XXI, p. 40.

PLATE XXIV. CAIRO BRIDGE. Vol. XXI, p. 40.

equivalent strengths, they are cheaper up to at least 65 feet spans than lattice girders. Their maintenance is also less. Their relative security is far greater ; one per cent. of faulty rivets will make a much greater reduction of strength in a lattice girder than in a plate girder to do the same duty. Plate girders are cleaner than lattice girders, being free from the numerous recesses and corners peculiar to this latter type, and hence less exposed to the accumulation of dirt, moisture and their accompanying oxidation.

Lattice Girders.—Riveted lattice girders have been used quite generally on all our railroads for short spans and on certain lines of railroad for all spans. For the shorter spans, as before stated, preference is now strongly in favor of plate girders. For the larger spans the necessity of performing so much riveting of important connections at the bridge site, where that care and accuracy attainable at the shops cannot be depended upon, does not render them acceptable to many engineers. And the necessarily increased risks and cost of the erection lead most manufacturers to prefer some other style of bridge, except for special cases.

While lattice bridges have given good satisfaction where they have been well proportioned, they have no merits which cannot be also had from our pin-connected types. They have not, therefore, made any progress for the longer spans.

On most of our roads they are now generally limited to the spans between the longest plate girders and the minimum pin-connected spans; a limit not definitely fixed, the lower limit being determined by the relative cost of plate and lattice girders and the ability of the roads to transport long plate girders ; and the upper limit by the personal preference of the engineer. These limits may be broadly stated as somewhere between 60 and 120 feet. The riveted form of connection has an advantage over the pin form for the lower spans, to be determined by the circumstances of each case, from the additional stiffness of the connections and the form of the members. This is partially attained by some designers by using stiff members for both tension and compression in their short pin-connected trusses. It would be possible to make an equally stiff pin-connected bridge for these lower spans, and at the same time attain that perfection of fitting and lengths of parts peculiar to this form of truss, if those connections, at which reversal of strains occur, were also rigidly bolted or riveted together. This

might be objectionable to the theorists who object to secondary strains. The author does not consider these, if restrained within proper bounds, as so objectionable; pin-connected trusses cannot be considered as free from them, as long as there exists such a thing as friction.

Pin-connected Spans.—With the exception of not over a few hundred spans, the longest of which are 260 feet (lattice bridges), the iron railroad bridges of the United States for spans over 100 feet, amounting to about 7 000 spans, aggregating 210 miles in length, are of the pin-connecting type.

The American people are of too practical a character to have built this quantity of bridges of a peculiar type, with the knowledge of other forms in use, both here and in European countries, and after thirty years' experience in their use, to still adhere to them, if their merits were not strong and positive ones.

Through all the changes of styles of trusses and forms of parts, the connection of the members by means of pins, and the attaining accuracy of lengths and fittings by machining the joints, has been persistent.

Forms of trusses with more than a single system of triangulation have gradually been rejected, and have now disappeared from first-class bridge designing.

Such subdivisions of the trusses as will give the greatest concentration of parts, with long panels, and without any theoretical or practical ambiguity of strain under a miscellaneous series of wheel loads, are now only adopted.

Forms of members requiring cast-iron joint boxes or other uses of this material have become obsolete.

The old forms, like the Bollman, Fink, Lowthorp and Post trusses (see Plates XII, XIII, XIV, XV, XXV, XXVI), have disappeared from American practice. The double intersection Whipple or Linville is rapidly following them. Generally bridges are made with parallel chords and equal panels; this gives the minimum number of different lengths of parts, which leads to greater economy and accuracy of manufacturing. For the longer spans, the depth may be reduced with advantage at the end panels.

The longest pin-connected truss span in existence is the recently completed channel span of the Ohio River Bridge at Cincinnati, 545 feet from center to center of end pins, 84 feet deep at the center and 60 feet at the end posts; panels, 27 feet 1½ inches; trusses, 30

feet apart, center to center. It carries a double-track railroad between the trusses, and has on each side a wagon-way and foot-walk, 16 feet wide.

The persistence of the pin-connected type of structure, now that the bridge engineer acting for the railroad companies has become an important factor in the problem of bridge construction, shows that it is not solely due to the preferences of the manufacturers; but that the operation and maintenance of such structures are also in favor of this type.

The typical American railroad bridge is a skeleton structure, pin-connected at all the principal articulations. Its essential characteristics in addition to being connected by pins are:

First.—So formed as to reduce all ambiguity of strains to a minimum;

Second.—Concentration of parts;

Third.—Facility of manufacture;

Fourth.—Perfection of lengths and fitting of all the members, so as to reduce to a minimum all riveting or mechanical work in the field;

Fifth.—Readiness with which the individual members can be assembled during erection.

9. Amount and Kind of Bridges on the Railroads of the United States.

The author attempted to collate from the official reports of the State Railroad Commissioners the data from which to form an estimate of the amount and kinds of bridges on our railroads.

He found that many States had no bureaus which collected this information. Also, that many reports were indefinite and evidently incorrect.

In order, therefore, to obtain more definite and reliable information, and also to get data which could be considered as truly representative of all the geographical divisions of the country, he sought it directly from the principal large systems of railroads. Many of these systems promptly placed at his disposal very full and reliable information as to their bridges.

Taking from the Railroad Commissioners' reports such data as appeared correct and complete, and adding thereto this additional information, avoiding as far as possible any duplication of the same roads, data covering nearly 60 000 miles of railroad, fairly distributed over the whole of the country, were obtained.

In order to make these data as nearly comparable as possible, the length of all double-track bridges was reduced to the corresponding length in single-track bridges.

In like manner, the length of all main tracks, omitting sidings and turnouts, was put into terms of single track.

Elevated railroads, which are composed mostly of bridges or trestles, have been omitted entirely.

Table No. 3 gives the general data as to quantity of bridges and trestles and the average rate per mile of track.

It shows that the relative amount of bridges and trestles varies in different districts from 58 feet per mile to 231 feet per mile. This last, however, is excessive, from including the crossing of Lake Pontchartrain, near New Orleans, on a trestle 22 miles long. Omitting this, we would get only 162 feet per mile as the maximum.

These variations are not entirely due to geographical location, as might appear at first thought. They are also affected by principles governing the original location of each road or division of a system. The alignment and grade may have been sacrificed to the avoidance of bridges or trestles, or the contrary.

From the large mileage covered by our table, we can rely with considerable confidence upon our average.

TABLE No. 3.

System of Railroad, or State.	Miles of Road.	Total length of Bridges and Trestles in feet.	Lineal feet of Bridges and Trestles per Mile of Road.
New York Central and West Shore Railroads	2 894	364 722	126
New York, Lake Erie and Western Railroad	1 514	95 509	63
Other Roads in New York	3 586	445 900	130
Roads in Pennsylvania	4 352	336 957	77
Roads in New England	2 199	176 700	80
Wabash System	1 636	160 025	98
Missouri Pacific System	4 707	566 953	120
Chicago, Milwaukee and St. Paul Railroad	5 727	614 736	107
St. Louis and San Francisco Railway	1 441	130 075	90
Denver and Rio Grande Railroad	1 458	102 195	70
Union Pacific Railroad	4 754	276 032	58
Louisville and Nashville Railroad	2 495	322 679	123
Queen and Crescent System	1 139	299 222*	231*
Roads in Illinois	8 539	707 535	83
Roads in Michigan	4 151	249 345	60
Roads in Iowa	7 778	1 049 386	135
Central Railroad and Banking Company of Georgia	1 487	173 975	117
Totals	59 857	6 071 946	101

* Included the crossing of Lake Pontchartrain, a trestle 22 miles long.

Taking, therefore, 100 feet per mile as our basis of estimate, we have for the 160 000 miles of railroad in the United States, 16 000 000 feet or 3 030 miles of bridges and trestles.

TABLE No. 4.

Miles of Railroad.	Trestles and Spans under 20 feet.	Spans from 20–50 feet.	Spans from 50–100 feet.	Spans from 100–150 feet.	Spans from 150–200 feet.	Spans from 200–300 feet.	Spans from 300–400 feet.	Spans from 400–500 feet.	Spans over 500 feet.	Total.	Average per Mile of Road.
26 228	2 299 758	85 181	94 165	149 121	80 551	29 542	5 677	1 211	1 040	2 746 246	104.7 feet.

Table No. 4 gives the distribution of the bridges upon 26 000 miles of railroad into spans of different lengths.

Using this as a basis of estimate, the 3 030 miles of bridges and trestles in the United States should be distributed as follows:

Trestles and spans under 20 feet.....	2 424 miles	=	727 200	spans.
Spans from 20–50 feet.....	121 "	=	18 150	"
" 50–100 "	130 "	=	9 100	"
" 100–150 "	190 "	=	8 000	"
" 150–200 "	109 "	=	3 300	"
" over 200 feet.....	56 "	=	1 150	"
	3 030 "	=	766 900	"

The above includes all bridges of either wood or iron.

Using the detailed information in the author's possession as to the bridges on the above 26 000 miles of railroad, which are of wood, combination and iron, we obtain the following estimate for the amount of iron bridges on our railroad system:

Iron spans under 20 feet.....	17 miles	=	5 100	spans.
" 20–50 feet.....	86 "	=	12 900	"
" 50–100 "	66 "	=	4 600	"
" 100–150 "	93 "	=	3 900	"
" 150–200 "	69 "	=	2 100	"
" over 200 feet.....	49 "	=	950	"
Total.....	380 "	=	29 550	"

The author's own office records contain the following iron bridges over 200-foot spans, and they are very incomplete for spans less than 300 feet:

Spans over 500 feet	2.5	miles.
" 400–500 "	3.9	"
" 300–400 "	5.0	"
" 200–300 "	15.0	"
Total	26.4	"

From the previous figures it would appear that there are now in existence on our railroads the following wooden and combination trestles and bridges:

Trestles and spans under 20 feet	2 407	miles.
" 20–50 "	35	"
" 50–100 "	64	"
" 100–150 "	97	"
" 150–200 "	40	"
" over 200 "	7	"
Total	2 650	"

Of the 2 400 miles of wooden trestle, we can consider one-quarter as only temporary, to be filled in as embankment. Of the remaining 1 800 miles at least 800 miles will be maintained in wood. This leaves 1 000 miles to be replaced gradually with iron bridges and trestles of such spans as may be most suitable for each location, probably from 50 to 200 feet.

This would ultimately make 1 600 miles of bridges for the 160 000 miles of railroad now in existence, of which only 380 miles are in iron at the present time.

The substitution of iron for the existing wooden bridges will, of course, be a gradual one. The amount of work involved in this, with that required in building iron bridges for new roads, additional crossings of our large rivers to connect present and future systems of railroad, and the demands for city and highway bridges, will furnish plenty of employment for our bridge building firms.

The author has no reliable basis for estimating the amount of highway bridges demanded by the present or the future. A comparison of the relative crossings of streams by railroads and highways in the more developed parts of our country, leads him, however, to the belief, that the lineal feet of highway bridges will be four to five times that required for railroads only.

10. Failure of Bridges.

In consideration of the facts already presented, it need not be a matter of surprise, that bridges have failed under train service in America.

The data in regard to the number of such failures have been magnified and distorted.

That such distorted evidence should have been submitted to a foreign society, without any opportunity for those acquainted with the truth to counteract the false impressions and conclusions, is to be regretted.

In the paper referred to, 251 bridges are given as the number of failures in the last ten years, classified as follows, by the author of the paper: 57 knocked down by derailed trains; 30 "square falls"; 96 uncertain; 5 occurred during replacement or repairs, and 63 unclassified.

That the great majority of these bridges were old and defective wooden bridges is not mentioned.

In addition to this generalization, there are given a number of photographs with incomplete statements of the failures they represent.

Of the nine railroad bridges, in regard to which their identity or any reliable statements are given, we are able to summarize as follows, with the aid of other facts in our possession:

Three knocked down through derailment of the train, one from a broken axle, one by a cow on the track, and one from neglect to spike the newly laid rails.

One split open by two trains colliding within the trusses; this was an old-fashioned structure with cast iron top chords.

One knocked down by a steam derrick too large to pass through the bridge.

One wood and iron combination bridge, seven years old, which failed in its wooden members under a load far in excess of its designed load.

One, the Bussey Bridge, an abortion in design and construction, and a bridge in which no engineer had any part.

One which is stated to have been culpably overloaded, and to have withstood this excessive overloading for a long period without showing any apparent signs of distress.

One showing a lattice bridge distorted by a train collision and which still carried the train.

The above is a summary of the evidence presented to show that

American pin-connected bridges are not so safe and reliable as those of a lattice form. It is fair to presume that this is the strongest evidence that the facts would admit.

The same authority, quoted by the author of the paper referred to, gives more recently a fuller account of the bridge failures for the ten years ending January 1st, 1889; total number, 265, of which only 38 are known to be of iron.

In his total he includes 27 wooden bridges burned, 39 carried away by floods, 8 failing during repairs, 60 knocked down, 34 square falls, 97 unknown.

Of the 34 square falls, 10 only are of iron.

The one iron bridge recorded during 1888 as a square fall was a through plate girder, failing from the breaking of a plate carrying the cross floor beams. More detailed information of the other nine is not given, so we are unable to determine the kinds of iron bridges and the causes of the failures.

It may be necessary to state that these reports of accidents are not based entirely on official reports; but are collated from the newspapers, and are undoubtedly full of sensation and inaccuracies.

Does any one advocate the designing and building of bridges to withstand the impact of a railroad train, or the bursting effect of piling two trains on one another inside of the trusses? Are such accidents to be classed as bridge failures or as failures of management?

That bridges of the lattice type have given good results in many cases of derailment, etc., proves nothing. Scores of similar cases, showing that pin-connected bridges do stand much abuse, and are as capable of resisting occasionally these extraordinary strains as any other kind of bridge, could be given by every practical bridge engineer in America.

They prove nothing in regard to the merit of any particular form of truss. They simply point to the need of better means to prevent and remedy cases of derailment.

It is a very gratuitous claim to assume that the bridge accidents on railroads in America are in any manner due to the accepted forms of our bridges, or that other forms of bridges would have given different results under the same circumstances.

The number of bridge accidents on American railroads, due to causes or forces which a bridge should be designed to meet or resist, is an exceedingly small one, when compared, as it should be, with the total

number of bridges on our railroads, and when the circumstances under which many of them have been built be given proper consideration.

The total number of spans of railroad bridges in the United States over 50 feet span, is 21 550, of which number 11 550 are of iron.

Due weight must be given to the rapid development of our railroad system, and the want of appreciation in the past of the demands for heavier rolling stock and correspondingly heavier bridge structures.

The American idea of building cheap railroads far in advance of the immediate demands of the regions through which they run, to settle those districts and build up a future paying traffic, has compelled the use of cheap and light bridge structures.

Even on the roads in the older portions of our country, the earlier bridges were built for loads far below present necessities.

Economy has also, in many cases, favored the lowest bidder, regardless of the incapacity or unscrupulousness of the bidder. There have been and still are many bridges of an inadequate strength in use on American railroads.

They are disappearing very rapidly, especially upon our better managed and first-class railroads.

Under these circumstances, it is a great credit to our style of bridge construction, that so few, comparatively, of our bridges have failed.

The writer does not know of a single case of the failure of a modern all-wrought-iron American railroad bridge, which has failed under legitimate train service. He has had knowledge of many bridges which have for years done far beyond their intended duty with satisfaction.

That the bridges built in the past have been lighter than the demands of the future must not be entirely credited to the engineer. As a rule he has done fully as well as the means given him would admit.

The American engineer has reaped one great advantage from the defective and inadequate structures of the past, not only from those that have actually failed, but also those which have been worked beyond their designed capacity—a knowledge of the relative merits of the various forms and proportions of the members and their details.

These structures may be said to be full sized tests, from which much that is valuable has been learned.

We can feel that our knowledge of bridges has passed far beyond the study of strains and tests of specimens. We have not remained content

with this limited field of knowledge, covering our ignorance under a so-called "factor of safety." We have determined the capacity and reliability of the full sized parts of our structures. We have also been enabled to study the working under excessive loadings, even up to rupture, of structures and their details which had outlived their intended purpose or which were inadequately designed from the beginning.

American bridge engineers and manufacturers have therefore legitimate grounds upon which to base their claims as to the great merits of our system of construction and proportioning.

The author desires to emphasize the statement that failures of bridges in the United States have no more bearing upon the relative merit of different systems of their construction, than the failures in other kinds of machines. The old-timed boiler explosions on our Western rivers did not convince any sensible engineer that cylindrical boilers were not suitable for high pressures. That machines inadequately proportioned for the demands which will be put upon them break down ultimately, especially when they fail to receive intelligent care and attention, is to be expected.

The intelligent investigator does not decide upon the merits of any developed system by the failures which are necessary steps in its development. Without variations and failures there would be no evolution or the survival of the fittest.

These variations and failures are interesting and instructive, but should not blind us to the merits of the final result.

The following remarks by Professor Unwin are worth careful consideration in this connection :

"If an engineer builds a structure which breaks, that is a mischief, but one of a limited and isolated kind, and the accident itself forces him to avoid a repetition of the blunder. But an engineer who from deficiency of scientific knowledge builds structures which don't break down, but which stand, and in which the material is clumsily wasted, commits blunders of a most insidious kind."

The author desires to draw attention to a series of articles on American and English bridges, by Mr. T. Claxton Fidler, in "Engineering," November 9th to December 28th, 1888. They are a valuable contribution, and are exceedingly fair in their treatment of the subject, considering that their author on some points has not the real facts of our best practice before him, and that he has mistakenly accepted a particular bridge company's specification as representing the general practice in our country.

He summarizes the following results as derived from the American system of competitive bridge construction :

"*First.*—It has developed much ingenuity of design, which has been directed, however, to the improvement of trusses in detail rather than to such broader features of design as are to be seen in the relative outlines of the Britannia Bridge, the Saltash Bridge and the Forth Bridge.

"*Second.*—It has attained to a great economy in details and the advantage obtained by the use of pin-connections, together with the attendant facility of erection has sometimes enabled the American maker to obtain the lead in the market.

"*Third.*—The system has necessitated and has produced an earnest inquiry into the theoretical principles of bridge construction, whose results have been embodied in certain specifications comprising a series of rules and regulations which are intended to insure the safety of the bridge.

"*Fourth.*—The defects and possible abuses of the competitive system are, however, acknowledged; and have been attended with numerous bridge failures.

"*Fifth.*—In quite recent times there has been a strenuous endeavor to repair the defective features of the system, and to obtain more ample guarantees for the safety of the bridge in the shape of more stringent rules; but so long as the rules are drawn with reference only to the real stresses, they must always be insufficient for the purpose in view, unless they are supplemented by the independent judgment of the engineer proceeding from merely arbitrary methods.

"*Sixth.*—In order that the rules may be effective for the purpose in view, they must be based upon a complete theory of safe construction, and not merely upon a theory of stresses; and must provide for all those requirements of stability and solidity which are instinctively recognized by the practical engineer, and which cannot be complied with by merely using a large factor of safety.

"*Seventh.*—If these requirements are to be formulated in mathematical shape, it would be necessary to enter upon that wider inquiry which has been referred to in these articles, and to specify a certain series of empirical and unreal distorting forces, which are to be assumed as acting at different points in the structure, and to be resisted by members possessing the strength which may be requisite for that purpose in addition to the strength which may be demanded by the real stresses. Such

a theory of unreal stresses may perhaps be dispensed with in English practice as it has been hitherto; but there is no reason why it should not be framed upon common sense principles, and adapted to the requirements of the American competitive system, for whose purposes it seems to be urgently required."

It is true that our system of bridge building has not advanced in the direction of such designs as the Britannia and Saltash Bridges during the past. Whether it will be towards designs like the Forth Bridge in the future is very doubtful. The author believes that problems of this magnitude will be solved by the American bridge builder, when the opportunity occurs upon the broad principles of our past experience.

We are always ready to admire "big things," as we have each of these structures in its day, and to reap benefit from the valuable contributions which have been given us by their able engineers in the line of new investigations and experience.

We are ready to acknowledge the evils of the old competitive system, where "the independent judgment of the engineer," or "those requirements of stability and solidity which are instinctively recognized by the practical engineer," do not enter sufficiently to obtain a safe system of construction.

The most perfect system of rules to insure success must be interpreted upon the broad grounds of professional intelligence and common sense.

While our competitive system, guided by a general specification, is more apt to produce good bridges than dependence upon designs prepared by any one with narrow and limited practical knowledge, its best results follow when supplemented by the independent judgment of the practical bridge engineer.

APPENDIX.

ABSTRACT OF RECENT TESTS ON FULL SIZE BRIDGE MEMBERS.

TABLE No. 5.

ABSTRACT OF TESTS ON WROUGHT-IRON COLUMNS, WATERTOWN ARSENAL.—SYMMETRICAL SECTIONS—PIN CONNECTED.

Set. *	Test Number.	Ratios.		Area of Section.	Pin Bearing.	Diameter of Pin.	Ultimate Strength per Square Inch.	Mode of Failure.
		$\frac{l}{d}$	$\frac{l}{r}$	Square Inches.	Square Inches.	Inches.	Pounds.	
N	17	10	27	12.	9.1	3½	35 070	Channels buckled near end of column.
N	1109	10	27	12.2	9.1	"	33 420	Channels buckled near end of column.
N	15	15	40	12.1	9.1	"	33 830	Channels buckled near end of column.
N	16	15	40	12.5	9.1	"	35 490	Channels buckled near end of column.
L	13	15	41	9.7	5.6	"	35 550	Filled by direct compression.
L	14	15	41	9.6	5.6	"	35 350	Channels buckled near end of column.
C	372	20	49	10.	8.8	"	31 698	Plates buckled between rivets.
C	371	20	49	9.1	8.5	"	31 302	Plates buckled between rivets.
L	1640	20	52	12.	13.	"	33 740	Deflected perpendicular to pins.
L	1641	20	52	12.3	13.	"	34 670	Deflected diagonally.
M	1638	20	52	12.4	13.	"	31 130	Deflected perpendicular to pins.
M	1639	20	52	12.7	13.	"	31 990	Deflected perpendicular to pins.
L	469	20	52	13.	8.1	3	28 970	By deflection in line of pins; column had an initial bend of ¼ inch in that direction.
I	740	20	53	7.6	7.	3½	34 340	Deflected perpendicular to pins.
I	741	20	53	7.6	7.	"	33 530	Deflected perpendicular to pins.
H	746	20	53	7.6	7.	"	33 910	Deflected perpendicular to pins.
H	1111	20	53	7.5	8.2	"	34 810	Channels buckled.
H	1112	20	53	7.5	8.2	"	35 240	Deflected perpendicular to pins.
L	11	20	55	9.6	5.6	"	33 840	Channels buckled.
L	12	20	55	9.6	5.6	"	34 000	Channels buckled.
N	9	20	56	12.	5.6	"	34 360	Channels buckled.
N	10	20	56	12.3	5.6	"	36 610	Channels buckled.
N	1107	24	61	4.6	5.2	"	34 740	Deflected perpendicular to pins.
N	1108	24	61	4.6	5.2	"	34 160	Deflected perpendicular to pins.
N	1	25	64	4.6	3.5	"	35 880	Deflected perpendicular to pins.
N	2	25	64	4.7	3.5	"	32 380	Deflected perpendicular to pins.
A	752	20	64	9.8	8.7	"	30 220	Deflected perpendicular to pins.
D	360	20	65	15.4	12.7	"	30 965	Deflected in plane of pins.

D	361	20	65	15.4	12.7	3½	31 494	Deflected perpendicular to pins.
A	757	20	65	10.2	8.75	"	31 380	Sheared rivets in bearing plates.
B	367	27.5	65	11.4	5.7	"	32 329	Deflected perpendicular to pins.
B	368	27.5	65	11.4	5.7	"	33 205	Deflected perpendicular to pins.
X	470	25	65	13.	8.1	3	30 000	Deflected perpendicular to pins; column had an initial bend in that direction.
L	467	24	66	9.2	10.8	"	29 980	Deflected perpendicular to pins.
L	1113	25	67	7.5	8.2	3½	33 970	Deflected in line of pins.
L	1114	25	67	7.5	8.2	"	33 610	Deflected perpendicular to pins.
X	7	25	67	12.1	9 1	"	32 940	Deflected perpendicular to pins.
X	8	25	67	12.0	9.1	"	34 240	Channels buckled near end.
E	358	28	67	17.8	9.3	"	32 074	Channels buckled near end.
E	359	28	67	17.2	9.3	"	32 253	Deflected perpendicular to pins.
L	3	25	69	9.7	9.1	"	33 880	Deflected diagonally.
L	4	25	69	9.8	9.1	"	33 660	Deflected perpendicular to pins.
M	1636	30	70	12.	13.	"	32 830	Deflected diagonally.
M	1637	30	70	12.	13.	"	32 740	Deflected diagonally.
C	370	30	74	9.2	8.6	"	31 650	Deflected perpendicular to pins.
C	369	30	74	9.5	8.6	"	30 720	Deflected perpendicular to pins.
X	1231	30	77	4.5	5.2	"	33 820	Deflected perpendicular to pins.
X	1232	30	77	4.6	5 2	"	32 380	Deflected perpendicular to pins.
L	1634	30	78	12.1	13.	"	33 630	Deflected perpendicular to pins.
L	1635	30	78	12.2	13.	"	32 440	Deflected perpendicular to pins.
H	747	30	78	8.	7.	"	36 580	Deflected perpendicular to pins.
H	748	31	79	7.6	7.	"	34 120	Deflected perpendicular to pins.
H	749	30	79	7.6	7.	"	33 410	Deflected diagonally.
I	739	30	79	7.5	7.	"	33 390	Deflected in plane of pins.
I	750	30	79	7.5	7.	"	34 390	Deflected perpendicular to pins.
H	1115	30	80	7.6	8.2	"	32 160	Deflected perpendicular to pins.
H	1116	30	80	7.5	8.2	"	32 140	Deflected perpendicular to pins.
X	28	30	80	12.2	5.6	"	31 610	Channels buckled near end.
X	29	30	80	12.5	5.6	"	31 340	Deflected perpendicular to pins.
H	465	30	80	7.75	7.8	3	27 750	Deflected perpendicular to pins.
L	5	30	83	10.	5.6	3½	34 130	Deflected perpendicular to pins.
L	6	30	83	10.	5.6	"	31 930	Deflected perpendicular to pins.
L	468	30	83	9.2	10.8	3	32 000	Deflected perpendicular to pins; column had initial deflection of 1 inch in that direction.
B	357	37	87	11.3	5.7	3½	29 186	Deflected perpendicular to pins.
B	356	37	87	11.4	5.7	"	29 947	Deflected perpendicular to pins.
X	1229	35	89	4.7	5.2	"	32 750	Deflected perpendicular to pins.
X	1230	35	89	4.7	5.2	"	31 120	Deflected perpendicular to pins.
E	1653	37	90	17.9	9.4	"	26 480	Deflected perpendicular to pins.
E	1654	37	90	19.5	9.8	"	25 290	Deflected perpendicular to pins.
H	1123	35	93	8.0	3.8	"	32 230	Deflected perpendicular to pins.
H	1124	35	93	7.7	3.8	"	31 370	Deflected perpendicular to pins.

* See Plate XXVII.

TABLE No. 5.—(*Continued.*)

Set. *	Test Number.	Ratios. $\frac{l}{d}$	Ratios. $\frac{l}{r}$	Area of Section. Square Inches.	Pin Bearing. Square Inches.	Diameter of Pin. Inches.	Ultimate Strength per Square Inch. Pounds.	Mode of Failure.
F	1645	40	96	16.2	10.	3½	28 020	Deflected in plane of pins.
F	1650	40	96	16.2	10.	"	27 910	Deflected perpendicular to pins.
A	755	30	96	10.	8.6	"	25 160	Deflected perpendicular to pins.
A	756	30	96	10.	8.6	"	21 105	Deflected perpendicular to pins; strength reduced by bending of pin.
L	26	35	96	9.3	9.2	"	32 180	Deflected perpendicular to pins.
L	27	35	96	9.6	9.2	"	29 380	Deflected perpendicular to pins.
D	751	30	98	16.	13.	"	26 430	Deflected perpendicular to pins.
D	1642	30	98	16.3	13.	"	22 540	Deflected perpendicular to pins.
G	1651	40	98	21.	12.	"	25 770	Deflected diagonally.
G	1652	40	98	20.6	12.	"	25 950	Sheared rivets in bearing plates.
C	354	40	98	9.2	8.75	"	28 950	Triple flexure.
C	355	40	98	9.4	8.75	"	29 879	Deflected perpendicular to pins.
H	466	37.5	100	7.75	7.8	3	26 000	Deflected perpendicular to pins.
N	463	40	102	4.7	4.2	"	25 000	Deflected perpendicular to pins.
N	1117	40	102	4.7	5.2	3½	29 180	Deflected perpendicular to pins; weakened by separators.
N	1118	40	102	4.6	5.2	"	30 990	Deflected perpendicular to pins.
H	24	40	107	7.8	9.4	"	31 350	Deflected perpendicular to pins.
H	25	40	107	7.8	9.4	"	27 850	Deflected perpendicular to pins.
I	1643	40	107	7.7	7.	"	30 840	Deflected in plane of pins.
I	1644	40	107	7.7	7.	"	30 770	Deflected diagonally.
H	1648	40	107	7.7	7.	"	31 610	Deflected in plane of pins.
H	1649	40	107	7.7	7.	"	29 870	Deflected diagonally.
N	1119	45	115	4.6	5.2	"	30 590	Deflected perpendicular to pins.
N	1120	45	115	4.7	5.2	"	31 050	Deflected perpendicular to pins.
H	22	45	120	7.8	9.4	"	24 850	Deflected perpendicular to pins.
H	23	45	120	7.8	9.4	"	26 920	Deflected perpendicular to pins.
N	1121	50	128	4.7	3.5	"	23 350	Deflected perpendicular to pins.
N	1122	50	128	4.6	3.5	"	25 360	Deflected perpendicular to pins.
N	464	50	128	4.7	4.2	3	15 260	Deflected perpendicular to pins; initial bend of 0.3 inch.
A	753	40	128	9.7	8.6	3½	19 380	Deflected perpendicular to pins.

A	754	40	128	9.7	8.6	3½	16 220	Deflected perpendicular to pins.
D	1646	40	131	16.2	13.	"	19 700	Deflected perpendicular to pins.
D	1647	40	131	16.1	13.	"	17 570	Deflected perpendicular to pins.
X	20	55	141	4.7	5.2	"	21 850	Deflected perpendicular to pins.
X	21	55	141	4.7	5.2	"	20 810	Deflected perpendicular to pins.
X	18	60	154	4.7	7.	"	14 740	Deflected perpendicular to pins.
X	19	60	154	4 7	7.	"	15 900	Deflected perpendicular to pins.
				Unsymmetrical Sections.—Pin Connected.				
N	1630	29	75	17.6	9.3	3½	26 190	Deflected perpendicular to pins. Pin in center of gravity of section.
N	1631	29	76	17.2	9.3	"	28 150	Deflected perpendicular to pins. Pin in center of gravity of section.
K	352	29	77	12.6	9.1	"	27 668	Deflected perpendicular to pins. Pin in center of gravity of section.
K	353	29	77	12.8	9.1	"	30 596	Deflected perpendicular to pins. Pin in center of gravity of section.
N	1632	29	78	17.6	9.3	"	17 420	Deflected perpendicular to pins. Pin in center of channels.
N	1633	29	78	17.7	9.3	"	17 240	Deflected perpendicular to pins. Pin in center of channels.
K	350	29	79	12.5	9.	"	16 242	Deflected perpendicular to pins. Pin in center of channels.
K	351	29	79	10.8	9.	"	19 207	Deflected perpendicular to pins. Pin in center of channels.
				Square Ended Columns.				
E	379	20	47	17.	.....	.....	34 950	Plates buckled between rivets.
E	380	20	47	17.8	.....	.....	35 595	Triple flexure.
B	377	20	47	12.	.....	.....	31 722	Plates buckled between rivets.
B	378	20	47	11.1	.....	.....	33 564	Plates buckled between rivets.
	1059	20	51	4.8	.....	.....	36 720	Channels buckled.
	1060	20	51	4.7	.....	.....	35 330	Channels buckled.
F	346	21	51	15.7	.....	.....	32 846	Plates buckled.
F	347	21	51	15.8	.....	.....	35 050	Plates buckled.
G	348	21	51	21.	.....	.....	33 682	Plates buckled.

* See Plate XXVII.

TABLE No. 5.—(*Continued.*)

Set. *	Test Number.	Ratios. $\frac{l}{d}$	Ratios. $\frac{l}{r}$	Area of Section. Square Inches.	Pin Bearing. Square Inches.	Diameter of Pin. Inches.	Ultimate Strength per Square Inch. Pounds.	Mode of Failure.
G	349	21	51	21.5	.....	.....	33 061	Triple flexure.
F	342	30	75	15.7	.....	.....	33 000	Deflected in plane A B.
F	344	30	75	15.6	.....	.....	34 505	Plates buckled.
G	341	30	75	21.2	.....	.....	33 019	Deflected in plane A B.
G	343	30	76	21.5	.....	.....	33 943	Deflected in plane A B.
N	337	30	76	17.	.....	.....	34 279	Deflected diagonally.
N	338	30	77	17.4	.....	.....	33 333	Deflected diagonally.
K	339	29	79	12.6	.....	.....	32 666	Deflected upwards.
K	340	29	79	12.7	.....	.....	33 862	Deflected upwards.

FORKED ENDS OF OPEN POSTS.—PIN CONNECTED AT ONE END.

							Total Ultimate Strength.	
P	391	.....	.....	11.5	6.4	$3\frac{1}{2}$	393 000	Pin ends bent inwards.
P	392	.....	.....	11.5	6.4	"	415 000	Pin ends bent inwards.
R	393	.....	.....	11.8	4.9	"	302 600	Ends buckled.
R	394	.....	.....	11.8	4.9	"	302 200	Ends buckled.

* See Plate XXVII.

TABLE No. 6.

Tests of Eye-Bars.

Date.	Makers.	Kind of Steel.	Size of Bars. Inches.	Length of Bar Tested. Feet and Inches.	Excess of Material in Eyes. Per Cent.	Elastic Limit in Pounds per Square Inch on Original Section.	Ultimate Strength in Pounds per Square Inch on Original Section.	Percentage of Elongation. At Fracture.	Percentage of Elongation. Over Whole Bar.	Percentage of Reduction of Area at Fracture.	Size of Pin Holes. Inches.	Elongation of Pin Holes. Inches.	Where Fractured.	Character of Fracture.
May 10th, 1887	Union Bridge Company	Bessemer	8 x 1 1/4	32 4.8	41.7	36 900	65 800	38.	16.	49.3	7	1.2	Body of bar.	Silky cup.
May 16th, 1887	"	"	8 x 2 3/8	14 3	49.	37 200	63 700	39.	16.8	43.9	7	1.125	"	100 per cent. silky.
May 17th, 1887	"	"	8 x 1 5/8	30 10.8	48.	38 600	64 680	39.	14.2	45.	7 1/4	1.0	"	Silky cup.
June 1st, 1887	"	"	8 x 2 1/2	9 10	40.6	31 160	62 790	28.3	13.6	63.6	8	1.48	"	50 per cent. silky and 50 per cent. granular.
July 16th, 1887	"	"	6 x 1 13/16	16 3	34.	36 460	61 340	37.5	17.3	44.6	6	1.17	"	Silky.
August 3d, 1887	"	"	8 x 1 3/4	23 3	32.	37 100	60 770	30.8	15.8	40.5	8	1.2	"	Ragged, partly cupped, fine crystal on edge.
November 15th, 1887	"	"	6 x 2	20 5	45.	40 560	66 930	35.4	17.1	47.3	6	1.04	"	90 per cent. silky, 10 per cent. crystalline.
November 21st, 1887	"	"	5 x 1 1/4	28 8	43.3	38 849	62 678	35.8	17.7	47.	4 1/4	1.0	"	Ragged, silky, few crystals.
December 14th, 1887	"	"	5 x 1 1/2	36 11	47.6	38 560	62 144	30 8	10.0	43.6	4	0.51	"	55 per cent silky, 45 per cent crystalline.
March 1st, 1888	"	"	6 x 1 9/16	29 8	52.3	40 063	63 597	38.8	15.2	55.9	5	1.08	"	Silky cup.
June 29th, 1888	"	"	8 x 2	31 0	41.9	38 048	63 416	28.3	12.4	41.7	7	1.35	"	85 per cent. silky, 15 per cent. fine granular.
July 24th, 1888	"	"	6 x 1 1/4	31 0	62.2	37 170	62 880	34.2	15.9	48.4	7	0.62	"	Silky cup.
October 25th, 1888	"	Open Hearth	6 x 1	27 2	43.7	31 863	58 770	38.8	13.6	47.7	6	0.99	"	Silky.
December 7th, 1888	"	"	6 x 1 7/16	19 9	74.6	32 440	68 100	35.	15.7	48.8	6 1/2	0.60	"	Silky cup.
December 10th, 1888	"	"	4 1/4 x 1	22 0	60.	35 300	59 600	30.	16.1	45.2	4 3/4	0.49	"	90 per cent. silky, 5 per cent. granular.
December 13th, 1888	"	"	7 x 1 1/4	17 5	43.5	34 600	55 370	32.5	18.8	48.2	5 1/4	1.38	"	Silky cup.
December 13th, 1888	"	"	6 x 1	31 5	52.	35 870	59 480	30.	18.6	42.8	5 1/4	0.96	"	Silky cup.
December 20th, 1888	"	"	6 x 1 1/4	25 0	51.1	29 530	56 000	36.6	16.6	56.6	4 15/16	0.80	"	80 per cent. silky, 20 per cent. granular on edges.
January 30th, 1889	"	"	5 x 1 9/16	10 5	55.5	34 170	59 250	35.8	21.2	48.4	5	0.67	"	Silky cup.
February 21st, 1889	"	"	5 x 1 1/2	22 9	43.8	30 880	55 650	44.2	14.0	50.6	7	1.26	"	Silky.
February 26th, 1889	"	"	7 x 1 1/2	22 3	41.9	31 000	57 350	41.6	14.1	49.1	6 3/8	1.02	"	Silky.
February 26th, 1889	"	"	5 x 1 1/2	31 1	46.6	31 500	55 600	40.	18.1	49.6	6 15/16	1.04	"	Silky.
February 26th, 1889	"	"	5 x 1 1/2	31 1	49.5	31 280	55 340	41.6	17.7	50.1	"	1.17	"	Silky cup.
January 10th, 1888	Edgemoor Iron Company	Bessemer	5 x 1 7/16	30 2	41.2	44 330	68 230	38.7	13.3	54.4	3 15/16	1.15	"	Silky cup.
January 10th, 1888	"	"	5 x 1 1/2	30 2	42.2	41 470	64 500	43.4	14.9	53.8	"	1.08	"	Silky cup.
January 10th, 1888	"	"	5 x 1 9/16	29 2	41 2	41 240	68 230	40.3	14.7	56.0	"	1.24	"	Silky cup.
April 26th, 1888	"	"	4 x 1 7/16	20 0	28.7	41 000	62 800	39.7	13.8	57.	"	0.54	"	Silky cup.
April 26th, 1888	"	"	3 x 1 1/4	20 0	36.4	42 620	65 400	36.5	16.8	54.4	"	0.48	"	Silky cup, slightly granular.
April 26th, 1888	"	"	5 x 1	30 0	53.2	43 860	63 600	37.1	14.8	45.2	4 5/8	0.44	"	Silky cup, slightly granular.
March 5th, 1888	"	"	3 x 1	15 0	39.3	41 370	65 530	41.	19.8	41.3	3 15/16	0.46	"	Silky cup, slightly granular.
March 29th, 1888	"	"	4 x 1	15 0	28.9	42 890	66 090	40.3	11.6	50.9	"	0.54	"	Silky cup.
April 3d, 1888	"	"	4 x 3/4	18 4	31.6	44 810	67 210	36.3	12.9	51.1	3 7/8	0.36	"	Silky cup.
April 3d, 1888	"	"	4 x 1 1/4	18 4	32.9	43 750	64 390	42.0	15.1	55.	"	0.81	"	Silky, ragged
April 7th, 1888	"	"	6 x 3/4	29 2	43.7	41 810	65 370	42.5	12.0	43.6	6 1/4	0.55	"	Silky, cupped.
April 7th, 1888	"	"	6 x 1 1/4	29 2	41.5	39 180	65 490	41.8	11.0	48.8	"	1.25	"	Silky.
March 12th, 1888	"	Open Hearth	5 x 1 7/16	24 0	36.6	32 670	63 310	43.8	15.	48.3	4 5/8	0.77	"	Silky, with granular edges
March 12th, 1888	"	"	5 x 1 9/16	24 0	41.7	34 650	66 600	40.	16.4	47.4	"	0.98	"	Silky, with granular edges.
March 13th, 1888	"	"	5 x 1	24 0	40.0	32 960	64 460	45.	17.2	47.2	"	0.74	"	Silky, cupped.
March 13th, 1888	"	"	5 x 1	24 0	40.0	37 650	68 670	45.6	17.7	45.6	"	0.84	"	45 per cent. silky.
March 14th, 1888	"	"	5 x 1 7/16	20 0	35.4	33 180	66 360	38.5	12.	44.3	"	0.93	"	Silky, with granular edges.
March 13th, 1888	"	Bessemer	6 x 1 9/16	22 6	54.6	35 190	61 000	47.5	11.8	50.	5	0.72	"	45 per cent. silky.
March 13th, 1888	"	"	5 x 1 9/16	22 6	54.8	36 840	65 320	48.1	17.	51.8	5	0.48	"	Silky.
July 10th, 1888	"	Open Hearth	5 x 1 1/2	17 2	35.	31 840	59 400	43.7	14.9	50.5	4 7/16	0.76	"	Silky cup.
July 10th, 1888	"	"	5 x 1 1/2	17 2	37.	31 000	58 800	42.6	16.1	53.2	"	1.05	"	45 per cent. silky
July 10th, 1888	"	"	5 x 1	17 2	37.2	32 000	58 200	45.0	16.1	50.0	"	1.07	"	Silky.
July 10th, 1888	"	"	5 x 1	17 2	40.	33 000	59 300	42.0	14.6	50.6	"	1.06	"	Silky.
July 10th, 1888	"	"	4 x 1 1/2	31 2	65.1	32 100	59 600	44.0	16.4	55.3	"	0.64	"	Silky cup.
July 10th, 1888	"	"	4 x 1 1/2	31 2	48.	32 470	61 600	39 5	15.5	50.0	"	0.67	"	45 per cent. silky
August 30th, 1888	"	Bessemer	2 1/4 x 3/4	17 0	41.3	39 000	63 000	34.5	16.3	60.6	2 1/4	0.38	"	Silky, cupped.
August 30th, 1888	"	"	2 1/4 x 3/4	17 0	44.2	39 430	63 090	28.5	14.5	52.6	"	0.56	"	45 per cent. silky.
August 31st, 1888	"	"	2 1/4 x 3/4	17 0	42.4	39 430	61 120	33.0	12.8	55.3	"	0.54	"	45 per cent. silky.
August 31st, 1888	"	"	2 1/4 x 3/4	17 0	41.8	39 710	64 370	32.0	13.6	59.1	"	0.42	"	Silky, cupped.
August 31st, 1888	"	"	7 x 1 1/4	22 2	44.	38 860	60 860	35.	18.	43.4	6 13/16	1.21	"	Silky, 35 per cent. granular.
August 31st, 1888	"	"	7 x 1 1/2	27 10	46.	38 080	60 560	35.	13.9	39.6	"	1.39	"	Silky, 45 per cent. granular.
August 31st, 1888	"	"	7 x 1 13/16	27 10	44.4	39 540	61 800	39 2	13.5	47.2	"	1.39	"	Silky.
August 31st, 1888	"	"	7 x 2	27 10	48.6	37 360	59 160	36.6	14.7	50.3	"	1.39	"	Silky, cupped.
August 31st, 1888	"	"	7 x 2 1/4	27 10	46.7	36 800	60 400	38.3	14.8	50.8	"	1.29	"	Silky, cupped.
August 31st, 1888	"	"	7 x 1 13/16	29 0	45.4	38 710	60 000	40.	14.5	51.0	"	1.50	"	Silky, 15 per cent. granular.
September 1st, 1888	"	"	7 x 1 13/16	29 0	43.1	38 900	62 300	26.	15.	46.8	"	1 04	"	Silky, granular edges.
September 15th, 1888	"	"	4 x 1 1/2	18 0	31.5	40 680	68 940	.....	.....	.....	3 15/16	.....	In eye.	Granular, not properly annealed
September 15th, 1888	"	"	4 x 1 1/2	22 0	34.0	36 630	63 820	39.2	17.4	56.4	"	0.67	Body of bar.	Silky, cupped.
September 17th, 1888	"	"	4 x 3/4	22 0	27.7	41 630	67 630	37.5	13.9	51.9	"	0.43	"	Silky, cupped.
September 17th, 1888	"	"	4 x 3/4	22 0	34.6	41 370	66 840	36.4	14.	42.6	"	0.88	"	45 per cent silky.
September 17th, 1888	"	"	4 x 3/4	22 0	27.3	42 270	66 240	37.2	14.	47.2	"	0.38	"	45 per cent. silky.
September 17th, 1888	"	"	4 x 1 7/16	28 6	28.	39 950	66 650	40.3	13.2	57.5	"	0.79	"	Silky, cupped.
September 18th, 1888	"	"	4 x 1 1/2	28 6	30.8	40 230	64 610	33.	11.6	48.8	"	0.23	"	45 per cent. silky.
September 18th, 1888	"	"	4 x 3/4	28 6	34.4	40 000	63 760	36.7	14.	50.0	"	0.45	"	45 per cent. silky.
September 18th, 1888	"	"	4 x 1 1/2	32 2	30.3	37 070	59 510	.....	.....	.....	"	.....	In eye.	Granular.
September 18th, 1888	"	"	4 x 1 1/2	32 2	33.8	41 440	65 610	32.7	10.4	45.	"	0.69	Body of bar.	45 per cent. silky.
September 22d, 1888	"	"	6 x 1	34 8	47.2	36 000	64 300	38.7	13.8	44.2	4 7/16	0.98	"	Silky, with granular edges.
September 22d, 1888	"	"	6 x 1 1/4	24 8	47.5	37 400	62 680	46.6	14.0	50.9	"	0.90	"	Silky, cupped.
September 24th, 1888	"	"	6 x 1 1/4	24 8	47.4	32 890	60 760	41.2	13.4	48.7	"	0.81	"	Silky, cupped.
October 25th, 1888	"	"	6 x 1 9/16	22 6	44.0	38 600	65 400	43.7	13.7	47.5	"	1.01	"	45 per cent. silky.

PLATE X.
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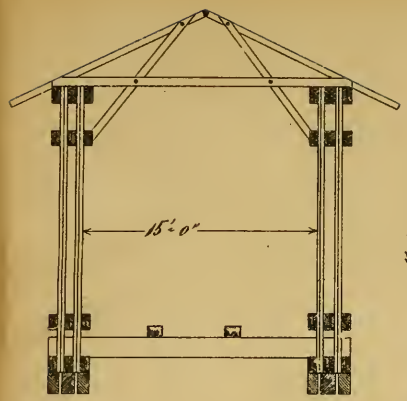
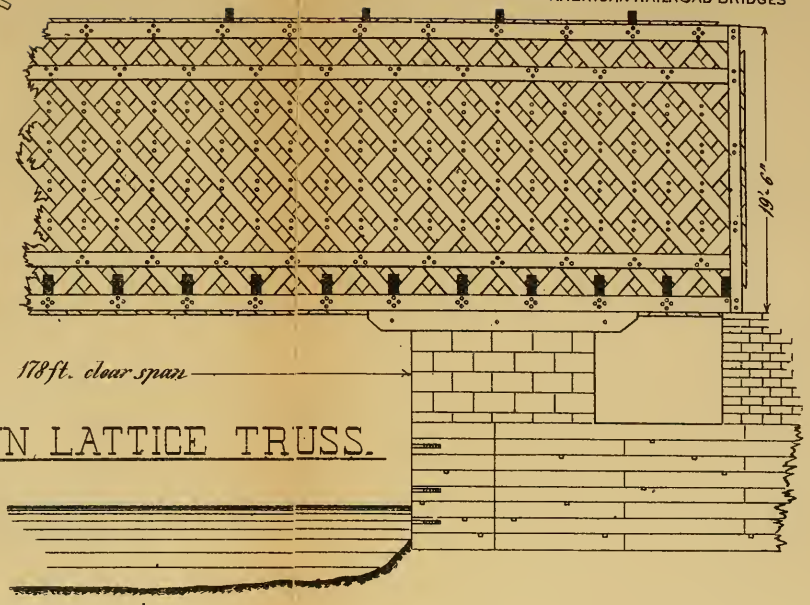
COOPER ON
AMERICAN RAILROAD BRIDGES





TOWN LATTICE TRUSS.

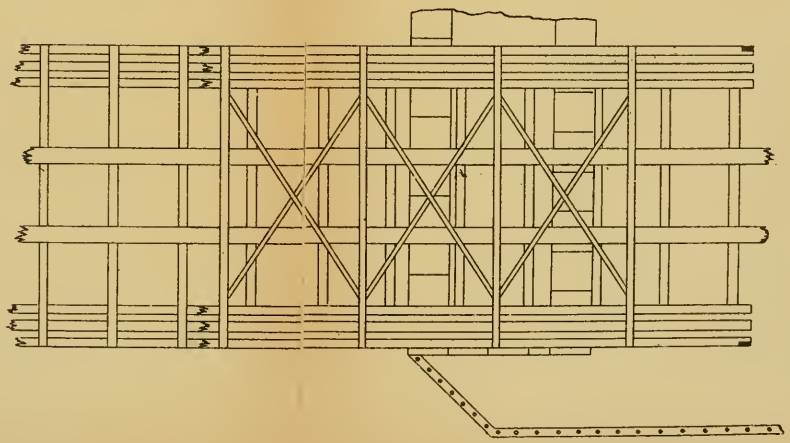


PLATE XI
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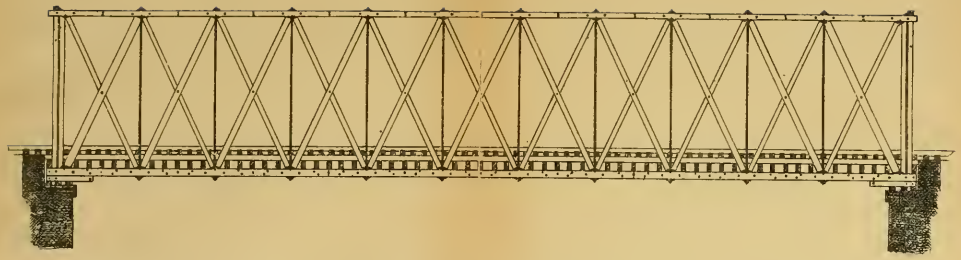
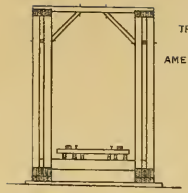
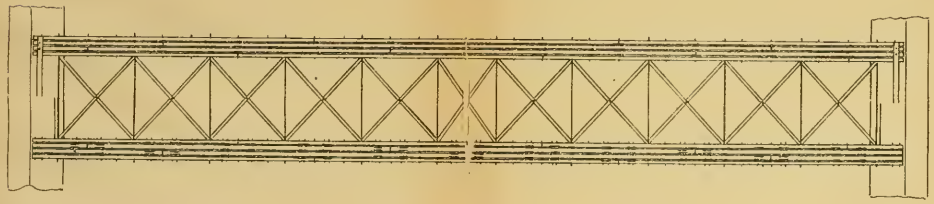
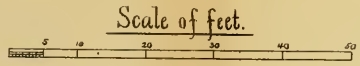
AMERICAN RAILROAD BRIDGES





Howe Truss Bridge.





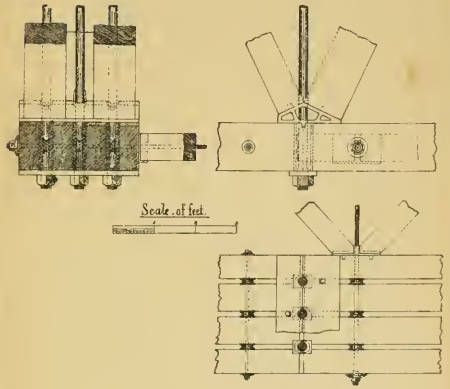


PLATE XII
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COOPER ON
AMERICAN RAILROAD BRIDGES

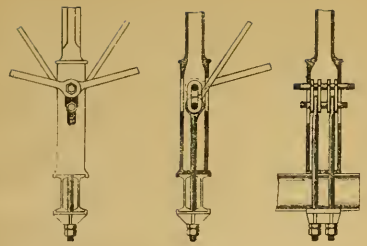
THE BOLLMAN PATENT

IRON SUSPENSION TRUSS BRIDGE

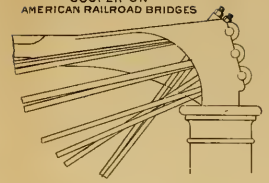
BUILT BY

WENDELL BOLLMAN

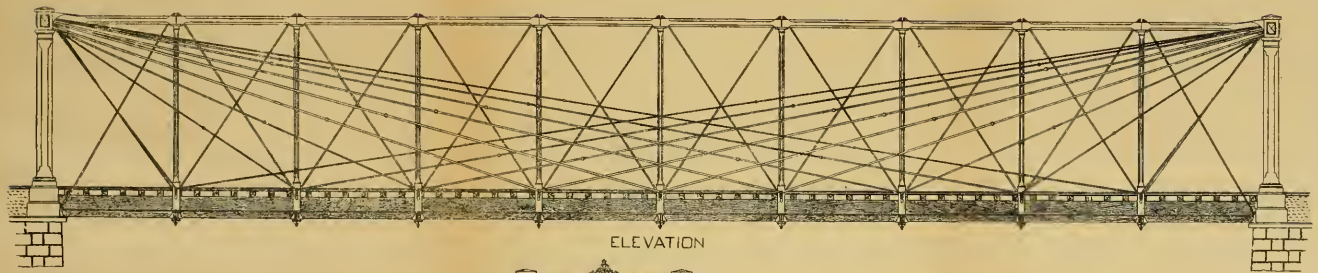
BALTIMORE.



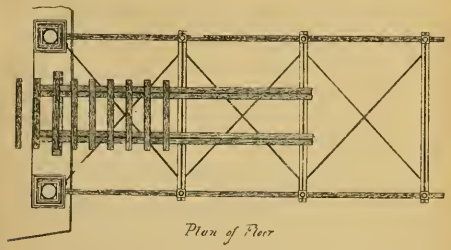
Details of Post



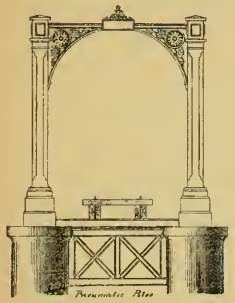
Details of Buttress.



ELEVATION



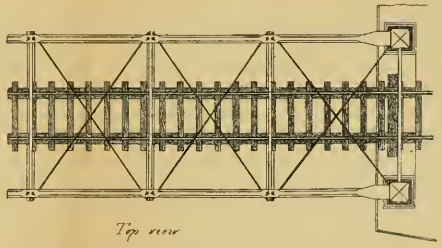
Plan of Floor



Section of chord

Section of post

Section of tower



Top view

IRON BRIDGE OVER MONONGAHELA RIVER ON THE BALTIMORE & OHIO R.R.

Constructed under B.H.Latrobe Chief Engineer in 1851-2

Designed & patented by Albert Fink *Assistant Engineer.*

PLATE XIII.
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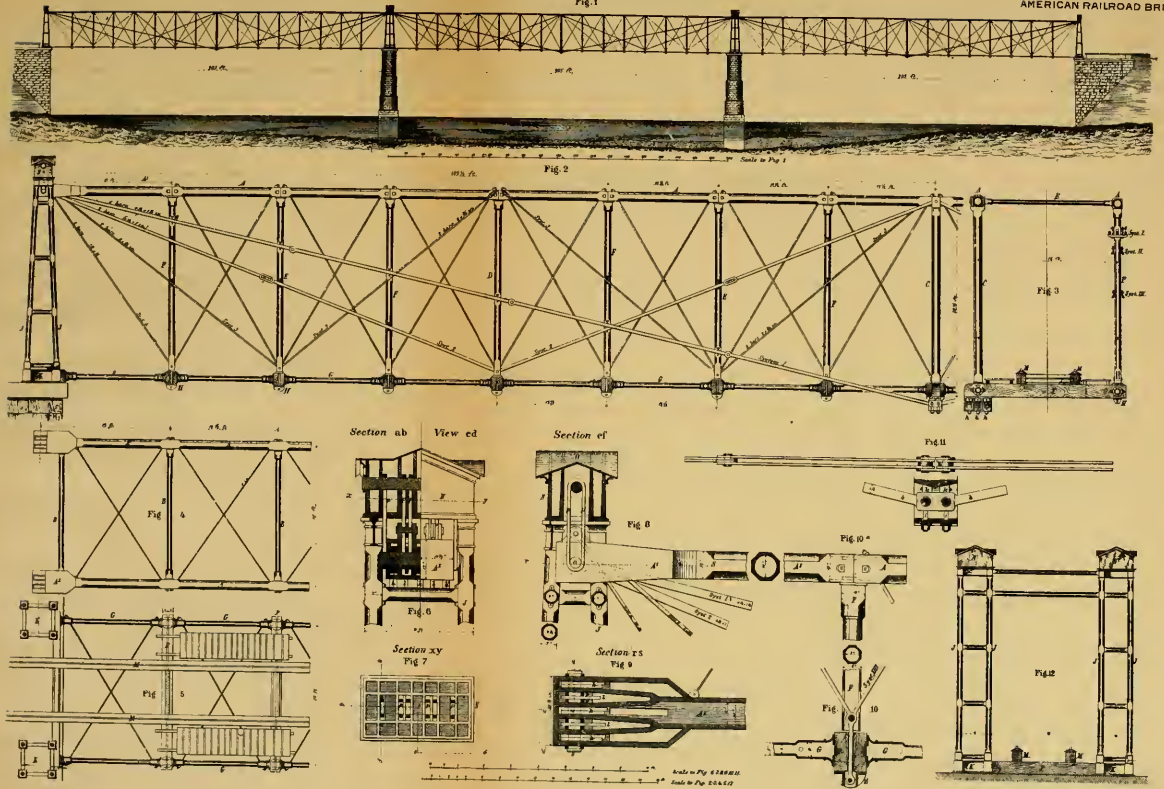
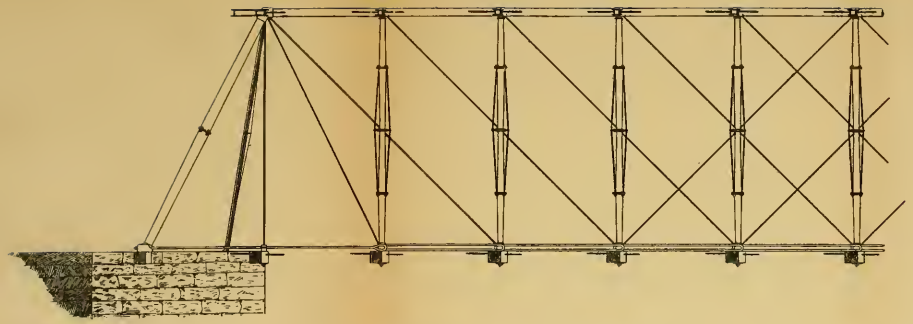
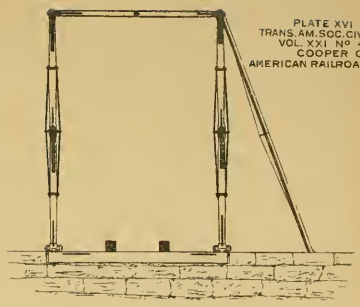
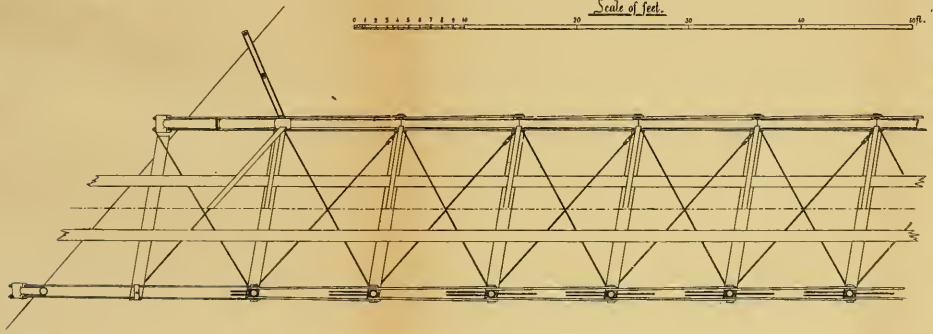


PLATE XVI
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXI No 418.
COOPER ON
AMERICAN RAILROAD BRIDGES

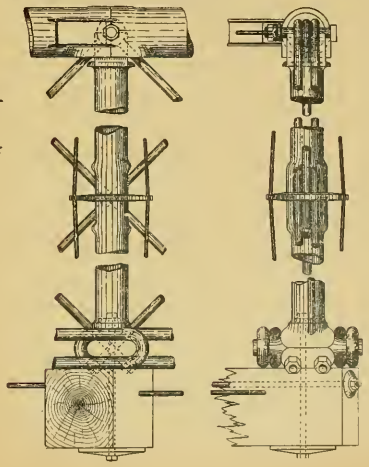




Scale of feet.



Scale of feet.



THE WHIPPLE TRAPEZOIDAL TRUSS.

Built 1852-53 on the Rensselaer & Saratoga R.R., 7 Miles North of Troy, N.Y.

by Squire Whipple.

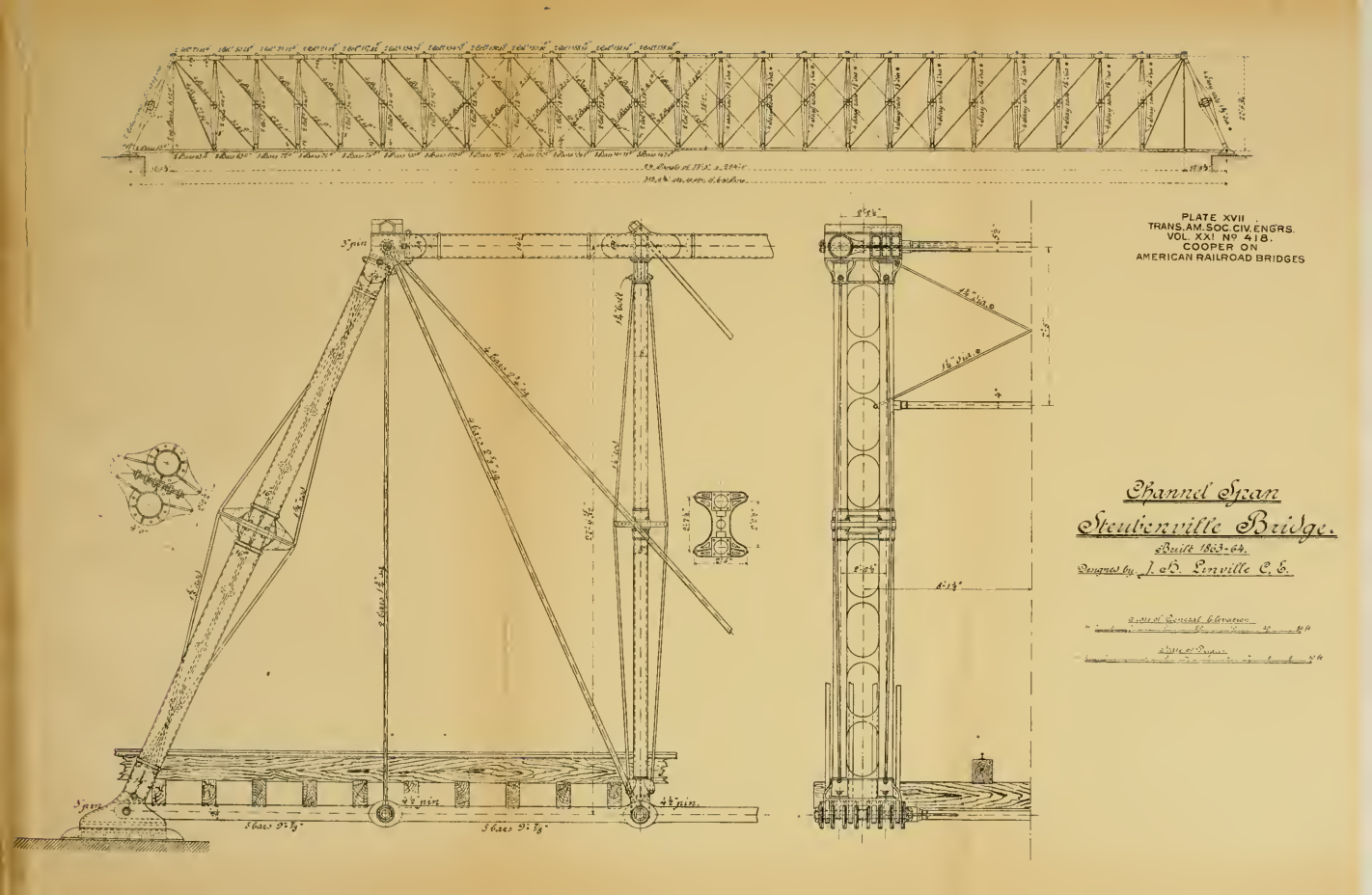


PLATE XVII
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COOPER ON
AMERICAN RAILROAD BRIDGES
Channel Span
Steubenville Bridge.
Built 1863-64.
Designed by J. H. Linville C. E.

PLATE XIX
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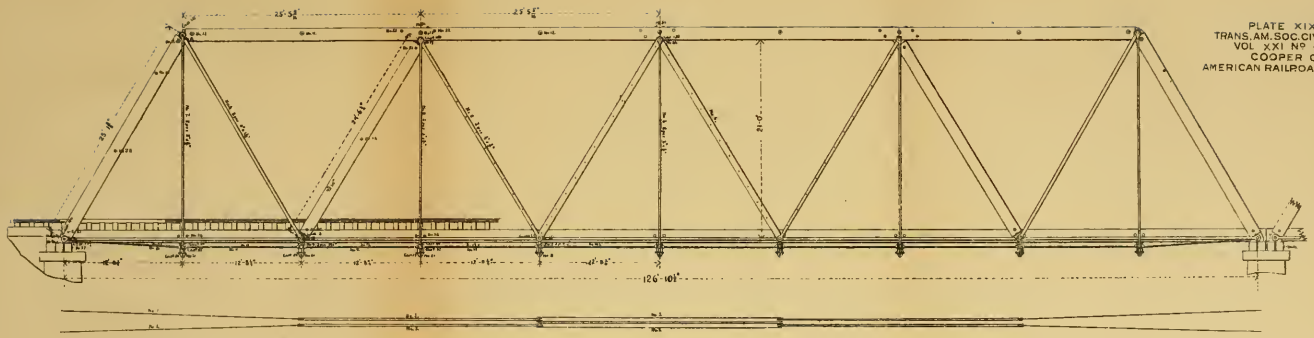
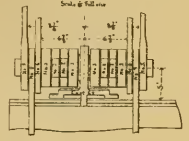
AMERICAN RAILROAD BRIDGES



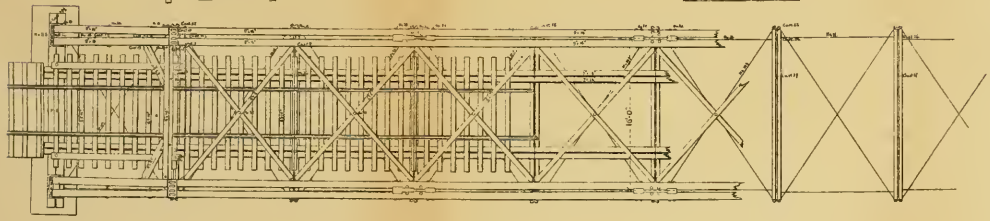
JOINT AT FOOT OF TRUSSED BRACES.
Scale of full size



GENERAL PLAN
OF
BLUE RIVER BRIDGE.

J. M. & I. R.R.

Louisville Bridge & Iron Co.
March 17 1876



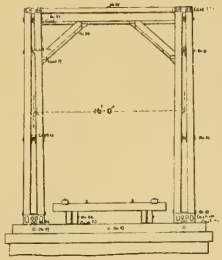


PLATE XXV
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AMERICAN RAILROAD BRIDGES

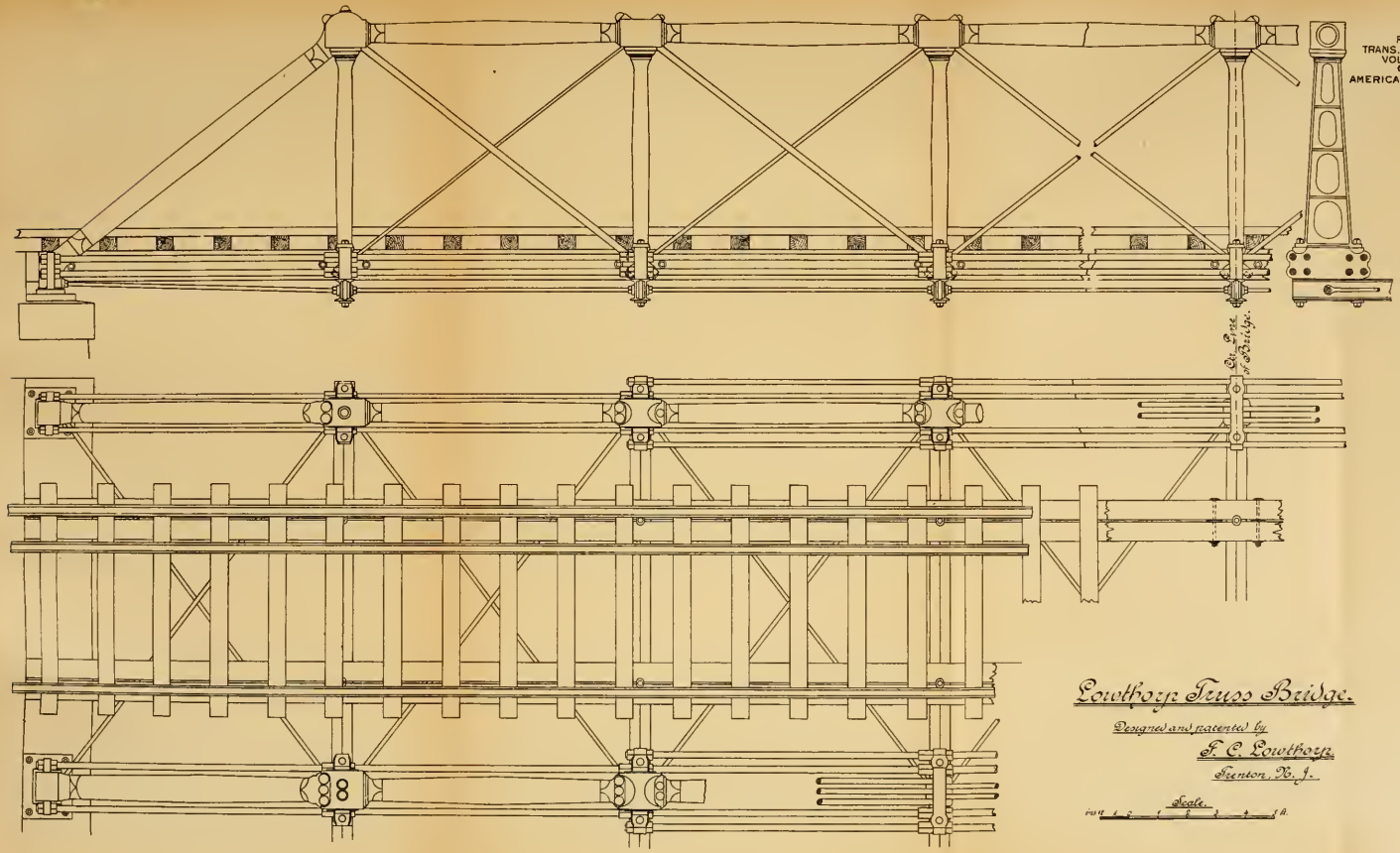
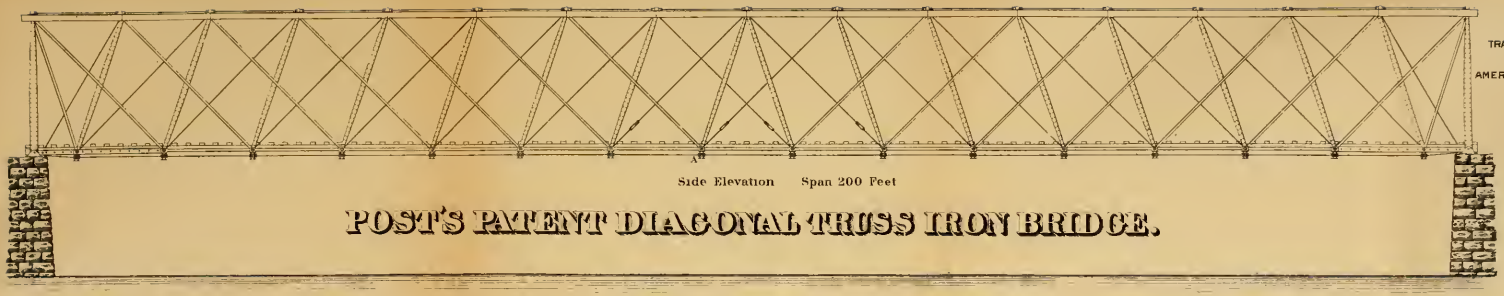


PLATE XXVI.
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXII. No. 418.
COOPER ON
AMERICAN RAILROAD BRIDGES

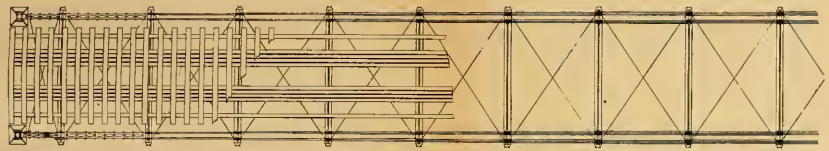


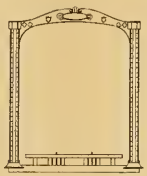
Side Elevation Span 200 Feet

POST'S PATENT DIAGONAL TRUSS IRON BRIDGE.

Scale of Elevation $\frac{1}{48}$ of an inch to one Foot

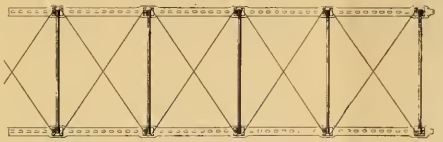
Plan of Lower Chord and Lateral Bracing.

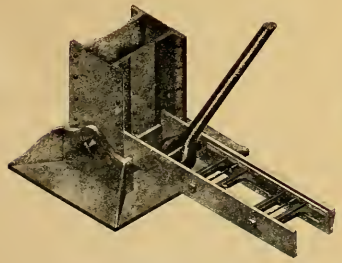




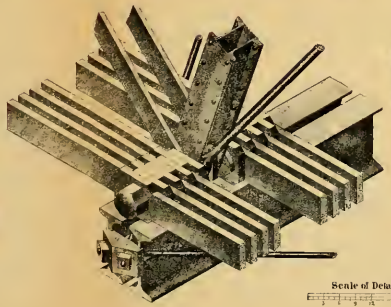
End Elevation.

Upper Chord and Top Lateral Bracing



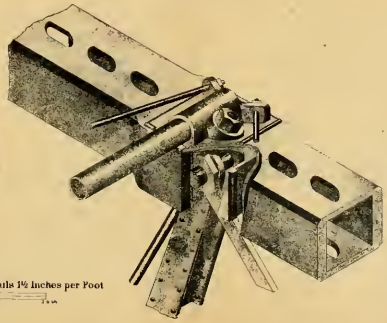


Square End Post.

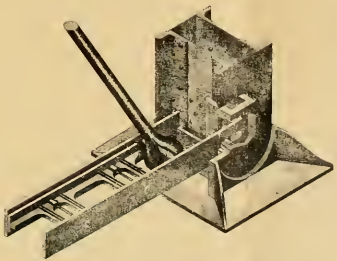


Connections of Lower Chord at A.

Scale of Details 1½ Inches per Foot



Connections of Top Chord



Round End Post and Compensation for Expansion

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

419.

Vol. XXI.—August, 1889.

SHIP CANALS IN 1889.

By R. E. PEARY, M. Am. Soc. C. E.

When the idea of presenting something upon the subject of ship canals first occurred to me, it was my intention to give a brief description of every work of that kind, completed, in progress, and projected, giving its history, principal features, cost actual or estimated, traffic actual or expected, cost of maintenance, and development, without, however, entering into any discussion of details, of merits of different routes, or of the question of canals *versus* railroads—something that would be of general interest and of a sufficiently non-technical character to be easily assimilated even in hot weather. But limited time and the unexpected extent of the task I had set before myself, has hampered me until I feel that the result is hardly more than a ship canal directory. Still I have felt that even that might not be entirely without interest to many, and I trust that omissions and incompleteness will be regarded leniently.

Naturally the paper is simply a compilation and condensation.

For the material for condensation I have been indebted to "Harcourt's Rivers and Canals," to the Annual Reports of the Chief of Engi-

neers, to various scientific and technical publications, the columns of the press, and, more perhaps than to any other one source, especially for the continuous record of the progress of the various projects, to the columns of *Engineering News.*

Completed Canals.—Abroad.

Languedoc Canal.—This canal, known also as the "Canal du Midi," is often spoken of as a ship canal, though its dimensions do not in any degree justify such a classification.

Its claim to the name seems to be in the fact that it was built for the use of the "ships" of that time (17th century) engaged in the coasting trade between the Atlantic and Mediterranean coasts of France, and that for its time it was undoubtedly as great an undertaking as the Suez Canal in recent years.

It forms a communication between Bordeaux, on the Bay of Biscay, and Cette, on the Mediterranean. Its length is 140 miles, summit level 610 feet above the Mediterranean, depth 6 feet 7 inches, and it has 119 locks.

The canal was completed in 1681 at a cost of $7 200 000.

Caledonian Canal.—This canal is located through a remarkable valley called the Great Glen, stretching across the Highlands of Scotland, between Inverness Firth and Loch Eil, a distance of about 60 miles; 38 miles of this distance, however, are occupied by a chain of lakes.

In 1773 James Watt reported favorably upon the project, but nothing was done.

In the beginning of this century, Telford, at the request of the Government, reported upon the project, and was intrusted with the execution of the work, which was commenced in 1804 and completed in 1823.

The Government was led to build this canal by the expectation that it would save vessels a long and dangerous circuit by the Pentland Firth, where previous to the introduction of steam they were liable to be detained for weeks by contrary winds; and, also, that in time of war the canal would afford a convenient refuge for merchant vessels from privateers, and a means by which war vessels might pass rapidly from one sea to the other.

The canal was designed for vessels of 20 feet draught, and it has 28 locks 170 x 40 feet, of 8 feet lift. The width on the bottom is 50 feet, on

the surface 120 feet, and depth 20 feet. The summit level is 100 feet above the sea, and the cost of the work was about $5 000 000. It was a bold and skillful undertaking, but it has not been a financial success. Wars hindered its progress, and so enhanced the price of materials and labor that its cost was nearly double the estimate, and finally, to save expense, the summit cut was not carried to full depth, and will permit the passage of vessels of only 17 feet draught and 250 to 300 tons capacity.

Had the full depth of 20 feet been obtained, the canal would have admitted vessels of 1 000 tons, and doubtless to this fact, more than anything else, may be attributed its failure in a financial sense.

North Holland Canal.—Formerly the only means of access to the port of Amsterdam was by the Texel Roads and the Zuider Zee. The Zuider Zee abounds in shoals and its navigation is difficult, so early in the present century the Dutch Government decided to make a new route for vessels trading with Amsterdam.

The shortest line from Amsterdam to the North Sea, in a westerly direction, was then considered out of the question, on account of the difficulty of maintaining an entrance on the exposed flat coast of the North Sea, and a northerly route through North Holland was adopted, starting from Lake Y, nearly opposite Amsterdam, and opening into the haven of Niewediep, on the Texel Roads.

The canal built on this route was known as the North Holland, and was commenced in 1819 and finished in 1825, at a cost of $5 000 000. It is 52 miles long, 123½ feet wide at the surface, 31 feet wide at the bottom and 18½ feet deep. It has a double tide lock at each extremity, with chambers 237 x 51 feet and 82 x 18½ feet, and three regulating locks.

This canal was of great value to Amsterdam and was of unusual magnitude for the time when it was constructed.

It is now superseded by the Amsterdam Ship Canal and has lost its importance since the completion of the latter.

Crinan Canal.—This canal, across the peninsula of Kintyre, 9 miles long and 12 feet deep, enables vessels of 160 tons to save a circuit of about 70 miles around the Mull of Kintyre.

Gloucester-Berkeley Canal.—The City of Gloucester has direct access to the sea by the River Severn and the Bristol Channel; but for some miles below Gloucester the course of the river is circuitous, and the rapid flow of the tides renders navigation difficult and dangerous.

An act was obtained in 1793 for connecting Gloucester by a direct ship canal with the estuary below, and after much delay this canal was completed by Telford in 1827. It is 16½ miles long, 13 to 20 feet wide at the bottom, 80 to 100 feet wide on the surface, and 18 feet deep.

At the lower end, where it enters the Severn, there is a tide-lock to maintain a constant level.

Witham Canal.—This canal, which gives Boston a direct communication with the sea, available for vessels of 2 000 tons as compared with vessels of 300 tons previously, is one of the most important of recently completed English works of its class.

The canal is 3 miles long, 27 feet deep, 130 feet wide on the bottom, and cost, with some accessory works, $1 000 000.

St. Louis Canal.—This canal was constructed to avoid the bar of the River Rhone.

It extends from the Rhone above the bar to the Mediterranean in the Bay of For, east of the Rhone outlet.

It is 2 miles long, 206 feet wide at low water level, and 19½ feet deep.

Göta Canal.—This canal gives direct water communication across Sweden from the North Sea to Stockholm, a distance of some 300 miles.

The canal proper, however, is but a series of short links connecting a chain of lakes which occupy four-fifths of the distance.

The canal is 46 feet wide on the bottom, 86 feet on the surface, and 10 feet deep, and is very solidly constructed. The summit level is 300 feet above the sea and there are 76 locks.

About 10 000 craft pass through the canal annually.

Suez Canal.—The main features of this canal, the most successful and important of all ship canals thus far completed, as well as the principal incidents of its inception and construction, are very generally known, yet a brief résumé seems necessary to make the record complete to date.

The value of a channel of communication between the Mediterranean and Red Seas, through the Isthmus of Suez, is evident from a glance at a map, saving, as it does, a long, circuitous route around the Cape of Good Hope for maritime traffic between Europe and the southern coasts of Asia.

The construction of such a canal is not a modern idea, but originated in remote antiquity. It is said that a canal across the Isthmus existed in the time of Sesostris, 1600 B. C., which later was abandoned. Nero

and Darius both contemplated constructing a canal here, and Harcourt states that there is evidence that a canal for small vessels was opened and maintained from about 600 B. C. to 800 A. D., but was subsequently allowed to fall into decay. Pope Sixtus V is said to have thought of cutting the canal in 1585. Louis XIV of France had a proposition submitted to him to construct a canal, and Napoleon I gave the project very serious consideration, but was deterred from carrying it into execution by the erroneous results of his engineers' surveys, these surveys showing that the level of the Red Sea was 10 meters above that of the Mediterranean.

In 1847 these figures were shown to be wrong, and accurate surveys demonstrated that the mean level of the two seas was the same, though the tides in the Mediterranean were only 1 foot, while those in the Red Sea were about 6 feet.

In 1854 M. de Lesseps obtained the concession for the canal, but much time was occupied in discussions and diplomatic negotiations, and the inauguration of the work did not take place until August 25th, 1859.

Work really began in earnest in the following year, and August 15th, 1869, the waters of the two seas mingled in the Bitter Lakes. November 17th of the same year the canal was formally opened and traversed by a numerous fleet of vessels of all nations.

The canal extends from Port Said, on the Mediterranean, at sea level and without locks, across the Isthmus by the most direct route, which carries it through the depressions of Lakes Menzallah, Ballah and Timsah, and the Bitter Lakes, to Suez on the Red Sea, in a line nearly due north and south. Its length is 99 miles, its width on the surface varies from 196 to 328 feet, according to the strata through which it is excavated, but the bottom width is 72 feet throughout. The depth is 26 feet. Through a portion of the Bitter Lakes no excavation was necessary, and the deepest cut, at El Guisr, was only 85 feet to the bottom of the canal. The amount of excavation was about 98 000 000 cubic yards, mostly sand and clay or a mixture of both, except at places south of the Bitter Lakes, where some rock was encountered. The cost of the canal was about $100 000 000.

A harbor formed by two breakwaters was constructed at Port Said, to maintain the channel dredged from the entrance of the canal to deep water of the Mediterranean.

These breakwaters are 4 600 feet apart at the shore ends, and 2 300 feet apart at the end of the eastern breakwater, which is 6 300 feet long, while the western one extends 3 500 feet farther, to deflect the silt laden littoral current from the entrance. During the first two or three years the work on the canal was done almost entirely by forced Egyptian labor, as many as 30 000 men being employed at a time. In later years the work was done almost exclusively by machinery.

From the day the Suez Canal was opened its business increased steadily and rapidly up to 1877, when it amounted to 1 663 vessels annually. In the two following years the traffic decreased, owing to a general depression in trade, then it went up with a jump, more than doubling in amount from 1879 to 1882, and rapidly grew through 1883, when it amounted to 3 307 vessels of a net tonnage of 5 775 861 tons.

With this amount of traffic it became apparent that the capacity of the canal with its original dimensions, and only fourteen *gares* or turnouts, was practically reached, and it was evident that speedy and ample measures must be taken to increase its capacity.

Two projects were discussed—one to build a second canal alongside the original one, another to widen and deepen the present canal.

The latter project was approved in the beginning of 1885, and the canal is to be enlarged to a depth of 29.5 feet, and a width, 26 feet below the surface, of from 213 to 246 feet on tangents, and 246 to 262 feet on curves of less than 8 200 feet radius. The total amount of excavation requisite to complete this enlargement is estimated at 91 000 000 cubic yards, and the estimated cost is about $41 000 000.

The enlargement, however, is to be carried out in three successive stages.

First.—Deepening the canal to 27.8 feet and increasing the bottom width to 121 feet.

Second.—Widening to the final dimensions.

Third.—Deepening to the final dimensions.

The first stage of this work, to give a depth of 27.8 feet, is now in progress, estimated cost about $12 000 000, and pending its completion the traffic of the canal since 1883 has increased more slowly, although the canal has been opened to night traffic, reducing the time of transit from thirty-six to sixteen hours, and the increase has been more in the way of an increased average tonnage per vessel than in the number of vessels.

This increase in the average tonnage of the vessels is very instructive. In 1870 it was 1 000 tons, in 1888 it was 2 743 tons.

With the completion of the enlargement, the traffic will undoubtedly take another vigorous bound upward.

The financial success of the canal can be best judged by the value of its shares.

The ordinary shares drew dividends in 1886 of over 11 per cent. and preferred shares nearly 17 per cent. A year ago ordinary shares of $100 sold in London at $427.50, and in Paris at $434.

Commercially this canal is of more importance to England than to any other nation, as it shortens the voyage to her Indian possessions about 7 000 miles as compared with the voyage around the Cape of Good Hope, and about 75 per cent. of the traffic of the canal is English.

Strategically also it is of vital importance to England, and as events have already shown, in case of complications she will possess and hold it at all hazards.

Amsterdam Canal.—This canal, which the circuitous route through the North Holland Canal, and the increasing size and draught of the vessels trading to Amsterdam, forced that city to construct, in order to hold its own against the more favorably situated ports of Rotterdam and Antwerp, extends due west from Amsterdam across the peninsula of Holland to the North Sea, a distance of 15½ miles. Its bottom width is 88½ feet, its surface width 187 feet, and its depth 23 feet.

The greater portion of it was constructed through a shallow lake, and the remainder through low sand dunes. The principal difficulties in its construction were the formation and maintenance of the entrance on the North Sea, and the complete re-arrangement of the system of drainage of the region traversed by it. This drainage is now pumped into the canal.

At the North Sea end there is, as on the North Holland Canal, a double lock, with chambers 390 x 60 feet and 227 x 40 feet. At the Zuider Zee end there is a triple lock, with one chamber 315 x 60 feet and two chambers 238 x 47 feet.

In the construction of the canal and harbor 21 000 000 cubic yards of sand were removed by dredging, much of it at a cost of only twopence per yard.

The canal was commenced in 1865 and completed in 1876, at a total cost of nearly $15 000 000.

Two railways and one road cross the canal on swing bridges.

The canal is doing a large and increasing traffic, as many as seven hundred vessels having passed through its double locks in one day, to the great benefit of Amsterdam.

St. Petersburg Canal.—The plans for this canal were matured in 1872–73, but work was not commenced until 1878. It was partially opened in October, 1884, and finally completed and formally opened by the Czar and Czarina in the presence of a large number of distinguished persons, May 27th, 1885.

The length of the canal is 18 miles; maximum width, 350 feet; general width, 180 to 240 feet; depth, 22 feet. The total excavation amounted to about 63 000 000 cubic yards of clay, sand and gravel, most of it used in the construction of the embankments. The cost of the canal was about $9 000 000.

The canal starts from the mouth of the Neva, where it opens into a large basin, and trends southward about 2 miles, with a width of 207 feet between embankments. It then joins the canal to Cronstadt and at the same point branches to meet the Neva above St. Petersburg. The foundations of the embankments were a double row of timber cribs filled with gravel, their internal faces planked and the excavated material pumped into the space between. The slopes of the dykes are protected with riprap on a bed of ballast.

The personnel and plant employed upon the canal consisted of 3 500 men, 13 dredgers, 3 locomotives with 230 cars, 86 lighters and barges, 12 tugs and 7 stationary engines.

The canal has both strategical and commercial importance, opening up as it does communication for war ships and large vessels of all kinds directly with St. Petersburg. Previous to the construction of the canal the cargoes of all vessels drawing over 9 feet had to be lightered 20 miles up the river to St. Petersburg, and all goods for export had to be lightered down the river in the same manner.

The effect of this canal was to increase the exports of St. Petersburg from 280 000 tons in 1883 to 950 000 tons in 1886, but it was at the expense of Cronstadt, the commerce of which was practically destroyed.

Ghent-Terneuzen Canal.—Beginning as far back as 1251 several canals have been constructed to maintain the communication of Ghent with the sea, the last one being completed in 1827.

The distance from Ghent to Terneuzen by this canal was only 21 miles, as compared with 105 miles by the River Scheldt.

It, however, proved inadequate for the constantly increasing size of vessels since its construction, and it has recently been straightened and enlarged to a bottom width of 56 feet, a surface width of 173 feet, and a depth of from 20 to 22 feet, with most beneficial results to the City of Ghent.

Completed Canals.—At Home.

Welland Canal.—The history of this canal is similar to that of St. Mary's, though on a smaller scale.

It was begun in 1824 and finished in 1833, by private parties, the depth being 8 feet.

In 1841 it was assumed by the Canadian Government, and its enlargement to 9 feet commenced The depth was afterwards increased to 10 feet by raising the embankments, and the locks enlarged to correspond.

In 1867 the canal was capable of passing a 400-ton vessel, and it had cost up to that time about $7 500 000.

In 1871 it was found that it would be necessary to again increase its capacity, and the work of enlargement was commenced soon after, and completed in 1887.

It was intended at first that this enlargement should consist of an increase in the size of the locks to 270 x 45 feet, and an increase in the depth of the canal to 12 feet; but almost as soon as the work was commenced it was found that this would not be sufficient, and the depth was increased to 14 feet.

The estimated cost of this enlargement was between $12 000 000 and $13 000 000; the actual cost was considerably in excess of that sum.

The canal is 27 miles long, and extends from Lake Erie to Lake Ontario, parallel with and west of the Niagara River. Its depth, as above, is now 14 feet, and its bottom width 100 feet. It has 27 locks, with a total lift of 330 feet, and will pass vessels of 1 000 tons.

A further enlargement is very likely to be undertaken in the not far distant future.

St. Mary's Canal.—This canal, which forms the outlet of Lake Superior, is unique in several respects.

It is 1 mile long and has at present a depth of 16 feet. It contains the largest lock in the world, 515 feet long, 80 feet wide in the chamber, 60 feet wide at the gates, 17 feet of water on the sills and a lift of 18 feet.

The canal was originally constructed in 1855, and there were 2 locks 350 x 70 feet, with 12 feet of water on the sills, and 9 feet lift each. About 1870 it became evident that the capacity of the canal had been nearly reached, and the work of enlargement was undertaken.

This work consisted of the construction of the present lock and the deepening of the canal 16 feet.

These improvements were completed in 1881 at a cost of about $2 500 000, with a most astonishing result upon the traffic of the canal.

The number of vessels increased and their size and draught increased to correspond with the increased water-way. The tonnage of the canal increased from 1½ million tons in 1881, the first year of the enlarged canal, to 4½ million tons in 1886—*i. e.*, it trebled in five years.

In 1888 the tonnage was over 5½ million tons.* From 1885 to 1886 the total tonnage increased 37 per cent., and from 1887 to 1888 the average tonnage per vessel increased some 20 per cent., the average now being about 657 tons.

The annual tonnage of the canal is now nearly as great as that of Suez. 1 685 vessels have passed through the canal in one month.

In 1886 it was seen that the ultimate capacity of the canal would be reached in two or three years—that capacity being 96 vessels per day of twenty-four hours, and 84 having already passed in that time; and a still further enlargement was proposed, and is now in progress. This will consist of a lock 800 x 100 feet, with a depth of 21 feet on the sills and a lift of 18 feet, and the deepening of the canal to 20 feet.

The new lock is to be placed upon the site of the two old ones, and will be used in connection with the present new one. The cost of the enlargement is estimated at $4 738 865, and the time for its execution five years.

If, on the completion of this enlargement, the traffic of the canal takes such an upward bound as it did after the last enlargement, and there is no room to doubt that it will, Suez, even with its own enlargement completed, will have difficult work to keep pace with it.

The present lock in this canal is undoubtedly the finest, as it is the largest, in the world. It is manipulated entirely by hydraulic power furnished by the fall at the lock, and the operation of hauling in, locking and hauling out a vessel is easily accomplished in thirteen minutes.

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* For the year ending July, 1889, the tonnage was 6 932 203.

The cost per ton of passing vessels through the canal in 1882 and 1883 was one and a half to two cents; it is now about one-half cent.

Des Moines Canal.—This canal gives a passage around the Des Moines Rapids of the Mississippi River. Its length is 7.6 miles; width, 300 feet; depth at extreme low water, 5 feet, and at high water, 16 to 26 feet. The locks in this canal are 350 x 80 feet. The total cost of the canal about $4 500 000.

In 1885 about 1 000 vessels passed through this canal.

Louisville and Portland Canal.—This canal, around the falls of the Ohio at Louisville, is similar to the Des Moines canal. In 1885, 5 000 vessels, of a total tonnage of 1 217 231 tons, passed the canal.

Canals in Progress.—Abroad.

Corinth Canal.—This canal will cut the isthmus of the same name, uniting the waters of the Ægean Sea and the Gulf of Lepanto, and make an Island of the Morea.

The Isthmus of Corinth is a narrow neck of land, with a least width of not more than 6 kilos (3.73 miles), and a maximum elevation of about 80 meters (262.5 feet). It is a plateau between two chains of mountains, Mt. Gueranicus on the north and Mt. Onicus on the south, about 3 000 and 2 000 feet high respectively.

The ancients, Periander, Tyrant of Corinth, in 628 B. C.; Demetrius Poliorcetes, one of the successors of Alexander the Great; Cæsar; and Caligula had all seen the commercial advantages and importance of a canal at this point, and Nero actually undertook the execution of the project, and the evidences of his work, after the lapse of eighteen centuries are perfectly distinguishable, and show the measure of the human force that this Emperor had at his command.

On the Ægean side there was a trench 70 meters (230 feet) wide at the highest part, 40 meters (131.3 feet) at the lowest, and about 200 meters (656 feet) long. The earth was banked up on both sides and the trench ends abruptly in a nearly vertical face.

On the Corinth side vestiges of Nero's work are also visible, and across the entire breadth of the Isthmus is a succession of pits 2 meters (6.56 feet) square, where soundings were made to a depth of 20 meters (65.6 feet).

There was a great demonstration at the inauguration of this work, and Nero turned the first sod with a golden spade, in approved modern

style. He is said to have abandoned the project upon being informed by scientists that the sea was higher on one side of the Isthmus than on the other.

In 1881 a French company was organized with a capital of $6 000 000, M. de Lesseps being honorary president. Three routes were surveyed across the Isthmus, and the one finally selected was the same as the ancient one of Nero. The right of way and uncultivated land on both sides were given by the Greek Government on condition of the work being completed without subsidy.

May 4th, 1882, the work was inaugurated, the King of Greece turning the first sod with a silver spade and the queen firing a train of dynamite mines. The line of the canal is perfectly straight and its total length is 6 400 meters (4 miles). Its section as originally proposed was 22 meters (72.18 feet) wide on the bottom, and 8 meters (26.25 feet) deep at low water. The side slopes 1-10 in rock, 2-1 in sand and 1-1 in firm earth, giving a water surface width of from 23.6 meters (77.43 feet) to 45 meters (147.64 feet). It was expected that the canal could be finished in four years, and a contracting firm took the job for $5 280 000, but inaccurate estimates as to the amount and quality of rock to be excavated, the necessity for flattening the side slopes and the corresponding increase of excavation have greatly prolonged the work and increased the expense.

The original contractors were obliged to throw up the work, and it is being completed by another firm. It is now expected that the canal will be completed in 1890 or 1891, and the total cost is variously estimated at from $9 000 000 to $12 000 000.

The completed canal will be 4 miles long, with a surface width of about 28 meters (91.87 feet), a bottom width of 16 meters (52.42 feet), and a depth of 8.5 meters (27.89 feet). The depth of the cut at the highest part of the Isthmus will be 288 feet. The personnel and plant employed upon the canal has naturally varied with the progress of the work; but at its maximum has been about 3 000 men, 15 locomotives, 700 cars, 6 or 8 dredges, with their attendant tugs and barges. The maximum day's work has been 8 000 cubic meters (10 464 cubic yards). The total amount of excavation will be about 8 500 000 cubic meters (11 118 000 cubic yards), of which some 5 000 000 cubic meters (6 910 000 cubic yards) will be rock. Up to the end of last year something over 5 000 000 cubic meters (6 910 000 cubic yards) had been removed.

The work on this canal has been especially interesting from the various systems of attacking the great mass of excavation that have been successively tried.

Two roads and one railroad cross the canal at a height that will clear the masts of all vessels.

This canal will shorten the voyage of vessels going from the Adriatic Sea to Turkey and Asia Minor by 185 miles, and those coming through the Straits of Messina by 95 miles.

It is estimated that the annual tonnage making use of the canal will be 4 500 000 tons, and the tolls for vessels from the Adriatic will be twenty cents per ton, and those from the Mediterranean ten cents.

North Sea-Baltic, or Holstein Canal.—Considering the auspices under which it is to be constructed, and the principal incentive to its inception, this canal will probably be the most commanding work of its kind in Europe.

The project for a ship canal between the Baltic and North Sea dates back about forty years. There are, however, three small canals now in existence between the two seas, one of which—one of the oldest canals in Europe—is still in use, and was commenced in 1391 and completed in 1398.

Another was constructed in 1525, and the third, projected in 1571, was commenced in 1777, and completed by King Christian of Denmark in 1785. This latter has a depth of 10½ feet.

Prussia had given the subject of a ship canal serious consideration previous to 1866, but the war of that year gave the matter a quietus.

In 1878 the project was again brought forward in a vigorous manner, first by private parties, and then by the Government, which gave the entire subject careful legislative consideration during 1881–83, and finally approved the bill in June, 1886.

June 3d, 1887, the German Emperor officially inaugurated the canal, and only last month proposals for some 15 000 000 cubic yards of additional excavation were advertised for by the Imperial Commission at Kiel.

All the projects for this canal having originated in military and political considerations, the same considerations controlled the selection of the final route, from the important naval station of Kiel, on the Baltic, to Brunsbüttel, on the deep water at the mouth of the Elbe, from among some sixteen proposed locations.

From the Elbe the canal passes through swampy land, and then through gradually rising ground, to the "divide" 82 feet above the sea.

Thence to the Eider, a portion of which is utilized, thence to the Eider Lakes, and thence *via* the Eider canal, rectified and enlarged, to Holtenau, near Kiel.

The length of the canal will be between 60 and 61 miles, the usual radii of curves 4 900 and 6 500 feet, with a minimum of 3 275 feet.

The chief value of the canal being in the speed with which it can be traversed, the curves will be as easy and few as possible. The average cross-section of the canal will be 85 feet 3 inches wide at bottom, 197 feet at water surface and 27 feet 10½ inches deep, giving 3 920 square feet of prism, which will permit the ordinary Baltic vessels to pass without trouble. The lakes and special sidings will accommodate war vessels. The depth will possibly be increased to 29 feet 6 inches, and the future enlargement of the canal is provided for by the purchase of a strip of land along the south side of the canal.

The canal is a thorough cut, with tidal locks at each end. The mean range of tides in the Baltic is about 1 foot 8 inches above and below the canal level, and in the Elbe 4 feet 6 inches above the same level.

At Brunsbüttel there will be three locks, viz., one 275 x 41 feet, one 412 x 82 feet and one 1 180 x 196 feet, and at the Baltic end one large one; this latter, however, will be usually open. The locks will be of massive construction, worked by hydraulic power.

Four railroads and several highways will cross the canal on draw-bridges.

The total amount of excavation is 67 000 000 cubic yards, and the estimated cost of the canal is $39 000 000. Of this sum $12 500 000 represents the excess cost of the work as a military canal over what it would cost for purely commercial uses. The estimated cost of maintenance is $450 000 to $500 000.

The commercial advantages of the canal are the saving in distance, time, pilot dues, and loss in going around Denmark. The saving in distance by vessels coming from the south and west of London to the Baltic is 237 miles, and from German ports some 425. The saving in time is from thirty hours for steamers to four days for sailing vessels. The greater safety is also an important item, 200 vessels being annually lost in the dangerous passage from the North Sea to the Baltic.

The North Sea-Baltic traffic is variously estimated at from 36 670 to 40 000 vessels annually, with a registered tonnage of 12 240 000 tons, 5 500 000 to 9 000 000 tons of which would use the canal at a toll of eighteen and three-fourths cents per registered ton.

The great charm of the canal, however, is its military importance, allowing the German fleet to be concentrated either in the North Sea or the Baltic with great rapidity.

Manchester Canal.—This canal, now in process of construction, will make the City of Manchester, at present 50 miles from the sea and 35 miles from the head of the tidal estuary of the Mersey, practically a seaport, and will completely alter the destination of an immense amount of tonnage now entered or cleared at London, Hull, and especially Liverpool.

It is said that the scheme for connecting Manchester with the sea dates back to 1712. In 1882 the matter was taken up vigorously by the local authorities and capitalists of Manchester. The first project considered was to deepen and widen the Irwell, so as to make a tidal waterway, a second Clyde, from the bar of the Mersey to the Manchester docks, a distance of 50 miles. Thorough surveys and studies led to the rejection of this project, and the adoption of the plan on which the canal is now being built, made by Mr. Williams, C. E.

From the outset great opposition was encountered from the Liverpool and Mersey interests, and from the several important railroad companies, whose lines cross the line of the canal, and which would be put to large expense to modify their lines so as not to interfere with the navigation of the canal.

This opposition delayed the passage of the canal bill in Parliament for several years, and it was not until the summer of 1887 that this opposition was overcome and the bill finally passed. The capital of the company was immediately raised, and the contract for the construction of the canal given out the same year. Work was commenced at once, and is now being vigorously pushed, so that there seems to be no doubt but that the canal will be finished within the contract time of four years.

The length of the canal is a little over 35 miles from Manchester to the Mersey estuary, separated into two divisions:

First.—A tidal division from Eastham through the Mersey estuary to Runcorn, 12 miles; then inland 8 miles further to Warrington, with a bottom width of 100 feet and a depth of 26 feet at low tide.

Second.—A canal division from Warrington to Manchester, 15½ miles long, with a bottom width of 100 feet, a depth of 26 feet and a surface width of 300 feet.

There are four locks, or rather series of locks, these locks being built in groups of three of different sizes, and with intermediate gates, so that any size of vessel may be passed without waste of water. The total rise of the canal is 60 feet. The total amount of excavation is about 48 000 000 cubic yards, and the contract price for the work is $30 000 000.

The personnel and plant now engaged upon the canal is about as follows: 15 000 men, 70 steam shovels, 50 steam cranes, 150 locomotives, 5 000 dump cars, etc., etc., and an average of over 1 000 000 cubic yards per month are being taken out. The work on the canal seems to be a model of perfect organization and business-like procedure.

The figures of the expected traffic of the canal I am unable to give; but there seems to be no question that it will be a financial as well as an engineering success, there being a very dense manufacturing population within a radius of a few miles of Manchester, to which supplies must be brought and from which manufactured products must be taken away.

Canals in Progress.—At Home.

Cape Cod Canal.—The project for a canal across Cape Cod was broached a little over two hundred years ago and nearly a hundred years ago complete surveys had been made for the work.

It is only within a few years, however, that work has actually been commenced, and it has not progressed very rapidly.

The line of the canal extends from the mouth of Monumet River, Buzzard's Bay, to a point south of Sagamore Hill, on Barnstable Bay. The length of the canal is 7.6 miles, and the deepest cut is only 25 feet to high water. The proposed section of the canal is 200 feet wide and 23 feet deep below low water. The amount of material to be excavated is about 18 000 000 cubic yards, and the cost is variously estimated at from $3 000 000 to $9 000 000. Time for completion was put at from three to five years. Two miles of the canal are stated recently to be finished to a depth of 15 feet. This canal would shorten the route from Boston to ports south from 70 to 140 miles, and the saving in time for sailing vessels is estimated at an average of three days.

The expected traffic of the canal is put at 4 000 000 to 5 000 000 tons.

The canal has military advantages as important as its commercial ones.

Harlem Canal.—This canal, which will connect the East and Hudson Rivers by way of Harlem River and Spuyten Duyvil Creek, will be about 8 miles long, 15 feet deep and 400 feet wide.

The history of this work dates back to 1874, when General Newton made a survey for the canal. His report was published in 1875, and additional ones in 1876 and 1881. In 1879 Congress appropriated $400 000 for the work, and in 1887 proposals were advertised for and the contract awarded in December of the same year.

The work was commenced early in 1888 and is now in progress. The estimated cost is $2 700 000.

Columbia Canal.—This canal will connect the Congaree and Broad Rivers, S. C. It is 5 miles long, 10 feet deep, 150 feet wide at the surface.

Over a mile of this canal, including the most difficult portion, is already completed. The canal will give Columbia and the Broad River a water outlet to the sea at Charleston.

Cascades Canal.—This canal, now in process of construction around the Cascades of the Columbia River, will be 3 000 feet long, and will have two locks, 462 x 80 to 90 feet, with 8 to 24 feet of water.

This canal will give an outlet to the great plain of the Columbia, as yet almost entirely undeveloped.

Nicaragua Canal.—The birth of the idea of a ship canal across the Central American Isthmus is practically coincident with the discovery of the New World, as it sprang into existence the moment it was found that there was no natural strait connecting the two seas.

But, though the project was frequently discussed and numerous so-called surveys made, nothing like an accurate survey was made until within the present century.

Immediately the Central American colonies threw off the Spanish rule and gained their independence, the project came prominently to the front. In 1836–37 Mr. Bailey made a partial survey, confined entirely to the western portion of the line, between Lake Nicaragua and the Pacific.

In 1850–51 a thorough survey of a complete route from the Atlantic

to the Pacific was made by Colonel O. W. Childs, and plans and estimates presented for a canal 50 feet wide at the bottom, from 78 to 118 feet wide at the surface and 17 feet deep, the estimated cost being a little less than $33 000 000. Colonel Childs' work is of interest as being the first survey made anywhere on the American Isthmus which was really deserving of the name, all previous ones being wholly or in part based upon mere reconnoissance, estimate, or even hearsay. A commission of English engineers and capitalists which examined Colonel Childs' plans came to the conclusion that a canal of the proposed dimensions would not meet the requirements of ocean-going vessels, nor could the commission see at that time sufficient prospective traffic to make the canal a financial success, and consequently the project was dropped.

When the United States Government in 1870–75 undertook through the Navy Department its comprehensive system of exploration of the American Isthmus from Tehuantepec to the head waters of the Atrato, to determine where was the best route for an interoceanic canal, the Nicaragua route was one of the first ones surveyed. Captain Lull, in command of the expedition, presented plans for a ship canal with a bottom width of 50 to 72 feet, a surface width of 106 to 165 feet and a depth of 26 feet, estimated cost a little less than $66 000 000.

In 1876 and 1880 Civil Engineer Menocal, U. S. N., M. Am. Soc. C. E., the engineer of Captain Lull's expedition, revised portions of the route, and, in 1885, radically modified the eastern portion. In the spring of 1887 a concession was obtained from Nicaragua by American capitalists for the construction of a canal, and in the latter part of the same year a large and completely equipped engineering force was sent to Nicaragua to thoroughly re-survey and definitely locate the axial line of the canal. Plans and estimates for a canal with a bottom width of from 80 to 120 feet, a surface width of from 80 to 340 feet and a depth of 30 feet, based upon the location made by this expedition, were prepared in the latter part of 1888, the estimated cost being about $66 000 000, and the time for completion five years.

The last Congress passed the bill incorporating the company with a capital of $100 000 000, with power to increase to $200 000 000, and the work of construction has already commenced.

The distance from sea to sea by the canal as now located is 170 miles. Less than 30 miles of this distance, however, is actual canal, the remainder being lake, river and lagoon navigation. In this respect it resem-

bles the Caledonian Canal. The summit level is 154 miles long and 110 feet above the sea. There are six locks. The principal features of this canal are the creation of 64 miles of slack water in the San Juan River by means of a dam and the formation of several miles of lagoon navigation by impounding the surface drainage by a series of embankments. The estimated time of traversing the canal is 28 hours, and the expected traffic when first opened is not less than 6 000 000 tons annually.

The magnitude of the benefits resulting from the opening of this canal it would be impossible to overstate. It will shorten the commercial water routes of the world from 2 000 to 6 000 miles, and the routes between our eastern and western seaboard by 8 000 miles.

Exceptional as has been the success of the Suez and St. Mary's Canals, the Nicaragua Canal will possess all the favoring elements of both in an enhanced degree.

The Suez Canal opened a shorter route to comparatively fully developed countries, and its business has been almost entirely due to the diversion to itself from longer routes of an already existing traffic.

The St. Mary's Canal, on the contrary, is the only outlet of a region the development of which has been rendered possible by the canal itself, and it may be said to have created its enormous traffic.

The Nicaragua Canal will open up a direct route to regions that, although they already yield a traffic of 6 000 000 tons per annum (which will immediately seek the canal), may be said to have just begun to develop, and the canal will not only divert this existing traffic to itself, but will in addition create a business the proportions of which it is difficult, if not impossible, to estimate.

The increase in the traffic of the St. Mary's Canal from 1 500 000 tons in 1881 to 4 500 000 tons in 1886, as the result of the development of the region about Lake Superior, may, however, serve as a basis for a general idea of the future traffic of a canal which will practically be to the Pacific Ocean what the St. Mary's Canal is to Lake Superior—its only outlet.

Projected Canals.—Abroad.

Birmingham Canal.—The rivalry between the great English manufacturing centers, greatly intensified by the construction of the Manchester Canal, will doubtless result in the near future in the construction of similar works for the benefit of Birmingham, Sheffield, Bristol, and other cities. Judging, however, from the numerous projects which

have been advanced within the past two or three years for giving Birmingham canal communication with almost everywhere, that city feels the rivalry and the disadvantages of its inland position most keenly, and is likely to be the first to follow the example of Manchester.

The principal projects so far are : a barge canal to the Trent ; a ship canal to London, estimated cost $5 000 000 ; a ship canal to the Mersey, consisting of a concreted channel 60 feet wide, 11 feet deep, capable of passing a 300-ton vessel, and estimated to cost $8 000 000 ; and a ship canal to the Severn, estimated to cost $6 250 000.

The latter project seems now in a fair way to be put through. The outgoing traffic of Birmingham is stated at 2 000 000 tons and a toll of four shillings is spoken of.

Great Western Canal.—This canal would be in reality a prolongation of the Birmingham Canal, in the interests of the manufacturing and commercial centers tributary to the Bristol Channel.

The projected route is from Stalford, on the Bristol Channel, *via* Taunton to Seaton, a distance of 62 miles. The canal is to be 125 feet wide at the surface, 36 feet wide at the bottom, and 21 feet deep. Estimated cost, $15 000 000.

The canal would save a voyage of 300 miles around Land's End.

Newcastle-on-Tyne Canal.—The project for a ship canal from the Tyne to the Solway Firth, first talked of some fifty years ago, was again brought forward about five or six years since, and some work done in the way of surveys.

The distance from sea to sea is 80 miles, of which some 12 miles are now navigable by vessels of large size.

It is claimed the canal would offer a convenient short cut for ocean steamers from one coast to the other.

Scotch Canal.—Within the past year a project for a canal from the Clyde to the Forth has been talked of. The length would be 35 miles, and the rise 156 feet. There is at present a 9-foot canal along the route, and it is said there would be no great engineering difficulties to overcome. Estimated cost from $7 500 000 to $10 000 000.

It is claimed that for Channel steamers between Ireland or the west coast of Scotland, England or Wales, to the east coast or the Continent, the canal would be of great benefit, shortening the voyage and avoiding the dangerous navigation around the north of Scotland; that it would also open up the coal and oil districts of Scotland, and that vessels from America to the east coast, the Baltic and the Continent would be benefited; but this latter is doubtful.

Important military advantages are also claimed for the canal, as it would permit the rapid concentration of war ships on either coast.

Irish Canal.—The project for a canal across Ireland, between Galway and Dublin, attracted some attention in England in 1883 and 1884.

It was proposed to enlarge the existing canal to a width of 100 feet

on the bottom and 200 feet on the surface, and a depth sufficient to permit the passage of the largest ocean steamers. The length would be 127 miles, and there were to be thirty locks.

A preliminary estimate of cost was $250 000 000. Further studies reduced this to $100 000 000. Alternative plans to accommodate vessels of 2 500 and 1 500 tons were estimated at $60 000 000 and $40 000 000 respectively. It was claimed that by using this canal transatlantic steamers to ports on the Irish Sea would save both in time and length of sea voyage. A press comment upon the project at the time was that the "scheme is not much more visionary than the Manchester Canal."

Bay of Biscay-Mediterranean Canal.—This is a project which comes up every little while in France, and which would amount practically to an enlargement of the Languedoc Canal.

One of the proposed routes would follow closely that of the present Languedoc Canal from Bordeaux to Narbonne.

The other starts from the Bay of Biscay at Arcachon, south of the mouth of the Gironde, and runs northeast to the valley of the Garonne; thence along the left bank of that river to Toulouse; thence in a direct line to Narbonne. The total distance is 267 miles. The canal will be 120 feet wide in rock cutting and 147 feet in embankments; width of passing places, 203 feet; depth, 25 to 27 feet. The summit level will be 557 feet above the sea, and the locks will have lifts of 30 feet and over. The deepest cutting will be 163 feet, at Naurose. The estimated cost of the canal is $130 000 000, and it would save a voyage of 700 miles around Spain.

North Sea-Mediterranean.—This is one of the most remarkable of great canal projects recently under consideration.

Like most other enterprises of this character, it is old in conception, dating back to the beginning of the Christian era.

It seems to have first taken definite shape about ten years ago in a plan for a canal from Marseilles to Lyons, and thence to Dunkirk. A second route was from Marseilles to Lyons, and thence to Paris and Rouen.

The portion of this last route from Paris to the English Channel seems liable to be executed at a not distant date, a French company having offered within the year to execute the work at its own expense if it is granted a toll of 60 cents per ton on all vessels entering the canal above Rouen.

The proposed canal would be 20 feet deep, and extend along the Seine from Rouen to Paris, 112 miles.

Brussels Canal.—The City of Brussels is agitating the project of deepening her present 10-foot canal to the sea, built in 1836, to a depth of 21–22 feet, to admit vessels of 2 500 to 3 000 tons, at an estimated cost of $4 000 000 to $6 000 000.

Bruges Canal.—The old-time commercial prestige and importance of Bruges has been gradually but completely destroyed by the silting up of its communication with the sea, and it is now proposed to construct a ship canal direct to the sea, a distance of 7½ miles. The proposed section of the canal is: bottom width, 66 feet; width at water surface, 203 feet, and depth, 23 feet. The estimated cost is about $5 000 000.

Paris-Boulogne Canal.—This canal is proposed for vessels of 600 to 800 tons. Its width would be 197 to 230 feet, increased to 262 feet at passing places, and the depth 13 feet.

Italian Canal.—In the summer of 1888 Signor Broca completed the survey of a canal across Italy, to save the long journey around Cape Lucca. The line of the canal extends from Fano, on the Adriatic, to near Castro, on the Mediterranean. Length, 180 miles; proposed width, 33 feet, and depth, 40 feet. Estimated cost, $100 000 000, and time for completion five years. In constructing this canal it would be necessary to drain two lakes. It is claimed that the canal would be of great value to the whole of southern Europe.

Baltic-Black Sea Canal.—Within the past five or six years the project for a ship canal from the Danube near Vienna through Moravia and Austrian and Prussian Silesia to the Oder near Breslau has attracted considerable attention, and has been carefully discussed at Berlin and Vienna. The distance in an air line is 200 miles and the estimated cost of a canal capable of passing large war vessels is put at about $37 000 000. The project has been reported as entirely feasible, and it is said that a preliminary credit for conducting the surveys has been applied for.

It is proposed that three-fifths of the expense should be borne by Austria and the remainder by Germany.

The number of vessels using the canal, it is claimed, would be three times as great as the number at Suez.

Don-Volga or Black Sea-Caspian.—The route of the long-talked-of canal, plans for which are said to have been drawn up in the time of Sultan Selim, is to be from Serepta, on the Volga, to Tzaritzine, on the Don, a distance of 53 miles. It will accommodate vessels 230 x 40 feet of 500 to 600 tons, which it is expected can make the trip in seventy hours.

The estimated cost of the canal is $14 000 000, not including administration, discounts, etc. Of this sum, $8 000 000 is for excavation, $4 000 000 for locks, and $2 000 000 for bridges, buildings, etc. The total excavation is estimated at 35 000 000 cubic yards. The deepest cut is 128 feet.

Perekop Canal.—This canal as projected will cut the Isthmus of Perekop, which unites the Crimea with the mainland, and unite the waters of the Gulf of Perekop and the Azof Sea.

It will be 70 miles long, 65 feet wide and 12 feet deep, and the estimated cost is $40 000 000. It is thought that it can be constructed in

four or five years, and it is claimed that it will shorten the voyage between Odessa and Sea of Azof ports 295 to 434 miles, enable vessels to avoid the dangerous navigation of the Straits of Kertch, and greatly benefit the salt, mineral, grain and fuel producing districts of south-eastern Russia.

A company is now said to be awaiting Russian sanction to build the canal.

Jordan or Palestine Canal.—The general features of this project were as follows: A canal 25 miles long, about 200 feet wide and 30 feet deep would be cut from Acre, on the Mediterranean, to the valley of the Jordan, north of Lake Tiberias.

The maximum depth of the cut would be 285 feet.

A second canal 67 miles long, and presumably of the same dimensions, would be cut from the head of the Gulf of Akabah to the south end of the Dead Sea Valley. The deepest cut on this section of canal would be 728 feet.

The valley of the Jordan and the Dead Sea, portions of which are 1 300 feet below the sea level, would then be flooded, obliterating the Jordan, Dead Sea, Sea of Tiberias and several hundred square miles of land, and forming a lake about 147 miles long and 10 miles wide, and the canal would be complete. The total distance from sea to sea is 240 miles. The estimated cost was from $40 000 000 to $100 000 000, and the estimated time of passage as four hours shorter than the Suez route.

Numerous advantages were claimed for this project, among others that it would be the fulfillment of the prophecy in Ezekiel. "Chinese" Gordon was a supporter of it.

As the origin of this project was entirely English, and as it was prominently agitated just at the time when the question of the enlargement of the Suez Canal was being discussed and England was endeavoring to obtain from M. de Lesseps a reduction in tolls and a greater share in the management at Suez, and nothing has been heard of it since, it is open to the suspicion that it was used merely to obtain as good terms as possible.

Syrio-Persian Canal.—The general project for a canal from the Persian Gulf to the Mediterranean was laid before the French Academy of Sciences in the winter of 1886–87, by a French engineer, without, however, any estimate of cost. The line of the canal as indicated is from Antioch up the Orestes, thence by tunnel through the mountains to the Euphrates, then utilize that river to Babylon, thence by canal to Bagdad, then down the Tigris to the Gulf. It is claimed that this canal would shorten the route between Europe and Asia three days.

Isthmus of Malacca Canal.—The project for this canal was vigorously agitated by the French in Siam in 1882–83, and in the latter year M. de Lesseps asked the King of Siam for a concession.

This canal would unite the Bay of Bengal and Gulf of Siam near Kraw, *via* the bed of the River Tayons.

Its length would be about 66 miles, and it is claimed it would save 500 miles of dangerous navigation between New York or Liverpool and the ports of China. Estimated cost, $20 000 000.

Ceylon-Indian Canal.—This project is to cut a channel through the island of Ramisseram, lying between Ceylon and India, by which it is said a sea voyage of 300 to 400 miles will be saved.

Projected Canals.—At Home.

Panama Canal.—The idea of a canal at Panama is of equal antiquity with that of Nicaragua. The two projects may be said to be twins.

The first actual survey of the Panama line seems to have been made in 1828, but nothing came of it. In 1843–44 M. Napoleon Garella surveyed a line across the Isthmus, and a French company was organized, and preparations made to commence the work, but wars in Europe interfered, and the project fell through. In 1875 a line was surveyed across the Isthmus by Captain Lull, United States Navy, under orders from the Navy Department, and plans and estimates for a lock canal were presented.

In 1876–77 Lieutenants Wyse and Reclus, of the French Navy, surveyed several lines, and in March, 1878, Lieutenant Wyse obtained a concession from the Columbian Government. A company was then formed, with M. de Lesseps at its head, to construct the canal, and after the preliminary arrangements were completed, work commenced in 1881.

Several plans for this canal were discussed, viz., with locks, with a tunnel, and a thorough cut, the last plan being finally adopted. The length of the canal is 47 miles, the bottom width 22 meters in earth and 24 meters in rock, the surface width 50 meters in earth and 28 meters in rock, and the depth 8.5 and 9 meters. The deepest cut is 386 feet, and the total amount of material to be removed was estimated at first at 46 000 000 cubic meters; later at 105 000 000 cubic meters, and still later, 1885, at 151 000 000 cubic meters, which last figures are probably still below the mark.

The original estimate of the cost of the canal was $120 000 000, the time for its completion eight years, and it was expected that 5 000 000 to 6 000 000 tons of traffic would use the canal.

Under the stimulus of M. de Lesseps' wonderful energy and great prestige the stock of the company was rapidly taken, and great quanti-

ties of plant and material were shipped to the Isthmus, a magnificent "installation" was put in commission, and the work went merrily on. Almost at the outset, however, obstacles which had been pointed out by prominent engineers were encountered, and the work did not progress as rapidly as had been expected. Several additional issues of bonds were made without dampening the enthusiasm, until in 1886 many intelligent people, who had hitherto believed in the project, became doubtful as to its success. This feeling spread, and became general in 1887, when it was shown conclusively that about $150 000 000 had been expended, and less than one-fifth the total amount of excavation had been completed, and that the company could raise no more money except at ruinous discount.

To meet this state of affairs the plans were changed provisionally to a lock canal, with locks to be gradually lowered to sea level after the canal was opened. This change very materially reduced the amount of excavation yet to be done, and was not without its effect in restoring confidence in some quarters.

This effect, however, soon wore off, and after a series of desperate financial moves to raise money last summer—among others a grand lottery scheme—the failure of the company to float an additional issue of bonds, even at a ruinous discount, gave the project its death blow, and after a few months of unavailing struggles, it collapsed finally and completely during the past winter, with obligations amounting to over $450 000 000, and after having spent some $250 000 000 in hard cash in the removal of some 50 000 000 cubic meters of material, about one-third the total amount. The laborers employed on the work have been sent home by their respective governments, and the Panama Canal is a thing of the past.

As an engineering work it has not been especially interesting, none of the really serious engineering problems connected with its completion having been touched as yet.

It is interesting, however, as showing the blind infatuation with which the *ipse dixit* of one man may be followed in the face of all facts and experience, and the results of beginning a great work without sufficient or accurate information.

It may be taken for granted that the Panama Canal will never be completed.

Delaware-Chesapeake Canal.—During 1879–82 the Government sur-

veyed three principal routes for a ship canal to connect the Chesapeake and Delaware Bays, and since then the project has been more or less prominently before the public, and a company was formed to construct the canal, but as yet no work has been done.

Of the three routes surveyed, the one known as the Sassafras is the favorite.

The length of the canal by this route is 30 miles from bay to bay, of which distance 14 miles will be obtained by deepening and widening streams, and the remaining 16 miles will be thorough cut.

The proposed section of the canal is: bottom width, 90 feet; surface width, 171 feet; depth, 27 feet.

No locks except tidal locks. The cost is variously estimated at from $6 500 000 to $11 500 000, and the time for completion is put at four years.

The canal would save 215 miles between Baltimore and New York and other Eastern and European ports, and 286 miles between Baltimore and Philadelphia.

It is anticipated that the canal would attract at least three-fourths of the tonnage of Baltimore, or from 5 000 000 to 10 000 000 tons.

From a military point of view this canal would be of great importance.

Niagara Falls Canal.—Several surveys and plans for a ship canal from Lake Erie to Lake Ontario have been made within the past thirty years, and two powerful causes push the project to the front every now and then: 1st, The very natural desire to have a canal of our own between those lakes; and 2d, the fact that the Welland Canal, with its 14 feet depth, is not deep enough to accommodate modern lake traffic, a fact which will be doubly emphasized when the improvements at St. Mary's and St. Clair have been completed and a depth of 20 feet obtained.

A recent project is for a canal 20 feet deep, to accommodate vessels of 3 000 tons. The length is about 25 miles and the estimated cost $18 000 000. An alternative route of 18 miles would cost about $1 000 000 more.

This canal seems more than likely to become an accomplished fact in the not distant future.

Florida Canal.—The project for a ship canal across Florida has been more or less continuously agitated during the past ten years or more, and in 1883–84 it looked as if the work would be actually undertaken.

A charter was obtained, a company organized and surveys made, but nothing came of it.

Starting from the St. John's River, just above Jacksonville, the canal would cut across to the Suwanee River, on the Gulf Coast. Its total length would be 137½ miles, the deepest cut 143 feet for a short distance, and the proposed dimensions were a width of 230 feet and a depth of 30 feet, sufficient to permit 3 000-ton vessels to pass without sidings. It was expected the canal could be completed in three years, and the estimated cost was $46 000 000.

The canal would enable vessels to avoid the navigation of the Strait of Florida, and would save in distance

Between New York and New Orleans	500	miles.
" " Pensacola	600	"
" New Orleans and Liverpool	412	"

Delaware-New York Bay Canal.—This project contemplates the enlargement of the Delaware and Raritan Canal sufficiently to permit its use by large vessels.

This canal, with the Cape Cod Canal, the canal from the Delaware to Chesapeake Bay, and the Dismal Swamp Canal, would form a grand system of inland navigation, which would offer an almost air line route to the coasting trade between Boston, New York, Philadelphia, Baltimore, Norfolk and the Carolina Sounds.

Such a system of navigation could not be duplicated in either hemisphere, nor its commercial and military importance be overestimated.

The military advantages of this project have recently been made the subject of a very instructive report by Admiral Luce, U. S. N., in which he points out the facilities such a canal would offer for the rapid concentration of war vessels at any point, either for attack or defense, and the complete bar it would present to any effectual blockade of any of our great bays or ports.

Other home projects, comprising some in the earliest embryo stage, some of inconsiderable importance, and some in regard to which it has been impossible to learn more than the name, are as follows:

Lake Borgne Canal.—From Lake Borgne to the Mississippi River; length, 12 miles; length of locks, 350 feet; estimated cost, $450 000. Will save coast trade east of New Orleans a voyage of 265 miles.

St. Clair-Lake Erie.—From Lake St. Clair to Lake Erie, Canadian territory.

Cincinnati-Lake Erie.—Two routes proposed—one from Cincinnati

to Toledo, and one *via* Zanesville to Cleveland. Preliminary estimate of cost of enlarging present canals about $28 000 000.

Fresno-San Joaquin River.—Surveys said to have been made, and the canal is to be of size to permit stern wheel steamers to run through from San Francisco. Estimated cost $3 000 000.

Lower Michigan.—From Saugatuck to Detroit *via* Kalamazoo River; length about 178 miles; width, 112 feet on the surface, 80 feet on the bottom; depth sufficient to accommodate vessels of 1 500 tons; six to eight locks. The canal would give an almost air line route from Chicago to New York, saving several hundred miles.

Upper Michigan.—The project for this canal is undoubtedly the outgrowth of the crowding of the St. Mary's Canal by the rapidly increasing lake commerce.

The proposed route is from Bay Autrain, on Lake Superior, to Little Bay de Noquet, on Lake Michigan. Length, 36 miles; two locks; estimated cost about $5 000 000, or a little more than the estimated cost of the proposed improvements at St. Mary's. The distance between Duluth and Chicago would be reduced 271 miles.

Lake Erie-Ohio River.—This is a project to obtain a deep water navigation from the Great Lakes to the Gulf of Mexico. The probable route would be from Beaver, on the Ohio, 28 miles below Pittsburgh, *via* Newcastle and Conneaut Lake to Erie, Pa. Distance, 136 miles; summit, 416 feet above the Ohio and 509 feet above Lake Erie.

Harcourt divides ship canals into three principal classes, and one supplementary class, viz.:

First.—Ship canals with locks and a summit level.

Second.—Ship canals passing through low-lying districts, and with only a regulating lock at each end.

Third.—Ship canals without locks, and of world-wide importance.

Fourth.—Ship canals from inland ports to the sea, and lateral ship canals in place of river navigation.

In an engineering sense the division into canals with locks and those without locks would seem to be a sufficient refinement.

A classification based upon the causes which lead to the inception and execution of such canals suggests itself, viz.:

Commercial Canals, originating in the demand of cities or regions, unfavorably situated by nature, for an outlet for their products; or in the struggle between rival cities or states for commercial precedence or perhaps even existence, as the Manchester, St. Mary's and Amsterdam canals.

Military Canals, originating in a nation's military and strategic exigencies or ambitions, as the Holstein Canal.

Interocean Canals, originating in the imperative demands of the commerce of the world, as the Suez and Nicaragua Canals.

Commercial canals may, by force of circumstances, become of temporary strategic importance, and military canals are certain to be of more or less commercial utility.

Interocean canals are the highest class. Aiding no one nation at the expense of another, they benefit every country that owns a ship.

Naturally in time of misunderstanding they become of inestimable strategic value to that nation that can control one of them. There are but two locations for such canals—one in each hemisphere, Suez and Nicaragua; one is completed, the other in course of construction.

After making all allowance for the fashion set by the completion of the Suez Canal, and the added impetus given by the commencement of the Panama Canal, eight years ago, to all kinds of projects for ship canals, many of which the recent catastrophe at Panama will cause to drop out of sight, we may safely say that the ship canal has come to stay and to become a powerful commercial and political factor. The next few decades are certain to see wonderful results in that direction.

The completion of the Nicaragua Canal will leave no more worlds to conquer in the class of interocean canals. The universal impetus it will give to commerce, together with the already active tendency in the direction of ship canals to give inland ports a direct and unobstructed communication with the sea, will probably for a time concentrate the activity in ship canal construction to works of that class.

The completion of many of these, and the continued growth of ocean commerce and its ever-increasing demand for shorter routes, will then result in the construction of canals of the character of the Corinth, Italian, Isthmus of Malacca, and Delaware-New York Bay canals, until every sea and ocean route will have been reduced to its minimum length. And though we may hardly expect to reach the state which recent astronomical observations are said to have shown upon our neighbor Mars, where a perfect network of canals, from 30 to 80 miles wide and some of them 1 000 miles long, appears to traverse the planet in every direction, it perhaps is not too wild a flight to anticipate the time when every maritime nation of importance will have, as one of its most important adjuncts of military offense and defense, a ship canal, for the rapid concentration of its huge floating fortresses at different parts of its coast.

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THE VICKSBURG SETTLING BASINS.

By CLARENCE DELAFIELD, M. Am. Soc. C. E.

Vicksburg, Miss., has always been considered a difficult point to supply with water, and, being called upon to design and construct works for this place, it became necessary to make a careful study of the sources from which the supply could be secured. By careful borings I ascertained that water could not be obtained from subterranean sources at any reasonable expense.

Centennial Lake, the original river, now cut off by a wash-out, is the receptacle of the drainage not only of the City of Vicksburg, but also of a large area of swamp, and is unfit for use.

The only source of supply remaining was the Mississippi River. This turbid water is thoroughly enjoyed by those who are accustomed to it, and its muddy appearance seems to offend none of the old citizens of the many cities and towns consuming it; but to those unfamiliar with its use, the appearance of the liquid is disgusting, and a clear, bright water is insisted upon.

The different conditions existing in the Mississippi River demand thoughtful care in selecting the proper treatment to meet the requirement of clear water.

When the river is low, and the current has little velocity, the amount of material held in suspension is very small, but in high stages of the river, when the banks are constantly caving, the quantity of solids carried by the stream is greatly increased. Through the kindness of Colonel L. H. Gardner, Superintendent of Water Works, New Orleans, La., I am enabled to give the result of tests made by him for thirty-nine weeks during the past year. The table exhibits the amount of solids in suspension in the Mississippi River water, expressed in grains per gallon:

Week.	Grains, Per Gallon.	Week.	Grains, Per Gallon.	Week.	Grains, Per Gallon.	Week.	Grains, Per Gallon.
1	1 688	11	88	21	108	31	33
2	297	12	51	22	83	32	79
3	74	13	137	23	164	33	140
4	53	14	183	24	173	34	80
5	164	15	159	25	87	35	120
6	1 650	16	193	26	80	36	73
7	60	17	178	27	77	37	34
8	100	18	166	28	34	38	13
9	162	19	75	29	19	39	45
10	104	20	201	30	14		

These figures show the wide fluctuations which exist in the amount of solids carried by the stream, and give a fair idea of the ever-changing conditions to be contended with.

If this water is allowed to stand at rest in a glass, the sand and coarse particles are almost immediately deposited, the larger part (by weight) of the suspended matter being precipitated in one day, leaving a certain depth at the surface fairly clear. The second day the water is clear to a greater depth, and in from four to ten days most of the suspended matter has been deposited. What remains is such an impalpable powder that it continues in suspension for a period far beyond any experiments I have made; it is, however, readily removed by a filter.

But another condition exists in the lower Mississippi River, that is, a stain caused by the overflow of swamps during floods at periods of high water. This matter is in solution, and cannot be removed by filtration unless previously treated by chemicals.

With these conditions, it is evident that to remove the matter in suspension, the water must be at rest, and that the finest particles can only be taken out by filtration.

Basins that would permit the constant inflow and outflow of water for daily use, with a minimum of disturbance, were desirable, and to accomplish this result I designed the works illustrated in the accompanying plan, Plate XXVIII.

The water is pumped directly from the river into the influent basin; from here it passes through seep-holes in Dividing Wall *A*, into Settling Basin No. 1, in slow, divided streams, without any tendency to produce circular currents or eddies. In Basin No. 1 by far the largest part of the sedimentation takes place, and the motion of the water is very slight.

To introduce the water into Basin No. 2 with the least possible disturbance, it is carried over the top of Dividing Wall *B* for its full length, drawing only the clearest surface water, and is then conveyed by interior passages to within 3 feet of the bottom of Basin No. 2. This thin stream, when delivering a million gallons of water per diem, flows at the rate of one-eighth gallon per second per lineal foot of wall, and the discolored water passing from Basin No. 1 to No. 2 has not sufficient current to raise much of the matter still held in suspension—hence the surface of the water is fairly clear.

To remove the slight cloud remaining, the water passes over the top of Wall *C* into Basin No. 3, then through the sand filter *D* into clear water basin No. 4.

The apertures in the filter are 4½ inches wide, and of sufficient height to allow the sand a slope of at least three to one, this height varying with the thickness of the walls; the current is so slight that no wash or disturbance of the sand takes place.

When the sand is fouled by continued use, it can readily be removed by washing it out of these apertures by a hose into Basin No. 3, and then the filter can be replenished with clean sand at the top. It will be remembered that this filter has but a slight duty to perform, as the amount of matter to be removed is infinitesimal.

Ordinarily, the water is now found clear and admirable for use, but at times, when the stain of the swamps is in solution, a small amount of either alum or perchloride of iron is found necessary to convert it into a form which can be removed by precipitation or filtration. A solution of alum is now used, which is introduced in absolute quantity from a tank connected by a pipe to the suction main of vertical pumps, thus intimately mixing it with the entire body of water, and presenting

every portion to its chemical action. The result is absolutely clear water.

The percentage of alum used is about one grain to the gallon, and it is entirely inert and harmless, even if not doing its proper duty.

The perchloride of iron is more energetic and has the effect of precipitating all matter in suspension within forty-eight to sixty hours, and is valuable in allowing a diminished area of settling basin, and leaving less duty for the filter. To those who have used filters this feature will be highly appreciated, as many a filter gets credit for the duty already performed by alum or perchloride of iron.

For the removal of matter deposited, I have designed the bottom of Settling Basin No. 1 with five valleys, each ending in a 10-inch outlet pipe closed by a gate. Basin No. 2 having but a slight deposit of very light material, has but one valley, with corresponding outlet pipe and gate. Basins Nos. 3 and 4, as a matter of precaution, and to wash out fouled filtering material, are each provided with an outlet and gate.

These outlet or drain pipes join one main sewer and empty into the river at a point down stream from the intake. For the purpose of removing the material deposited, a hose is attached to the force main to stand pipe, and with water under a pressure of 90 pounds, the deposit is rapidly removed to and through the drain pipes, and at small cost.

As it will take some years of use to determine the rate of deposit, and as the filter, with its small duty of removing inert, inorganic matter, will only be renewed at long intervals, it is difficult to estimate the cost of operating the settling basins and filter in relation to the amount of water pumped. I believe it will be small as compared with the cost of the large amount of water used in cleansing the ordinary filters, and the increased coal bills incurred in obtaining the additional pressure required to pump through the obstruction offered by such filters.

These works have now been in use for several months, and the water delivered to consumers is giving perfect satisfaction; it is the first adaptation of well known ideas as to settling basins and filtration direct.

The cost of the plant is about $17 500, but of course on a larger scale would be much less proportionately; it is asking too much of filtration alone to produce the same result as economically.

The fundamental principles of this design are, first, to avoid circular or direct currents of any velocity, or at least to reduce the same to a

minimum; second, to carry the surface water, as being the clearest, from one basin to that next adjacent; third, to lead the water from one basin to the lower portion of the next, where, on account of the absence of currents, the suspended matter would have no tendency to rise, thus leaving the superficial water clear to pass into the next basin.

A fence was erected, inclosing the basins, in order to prevent any disturbance of the water by wave action and currents caused by the wind.

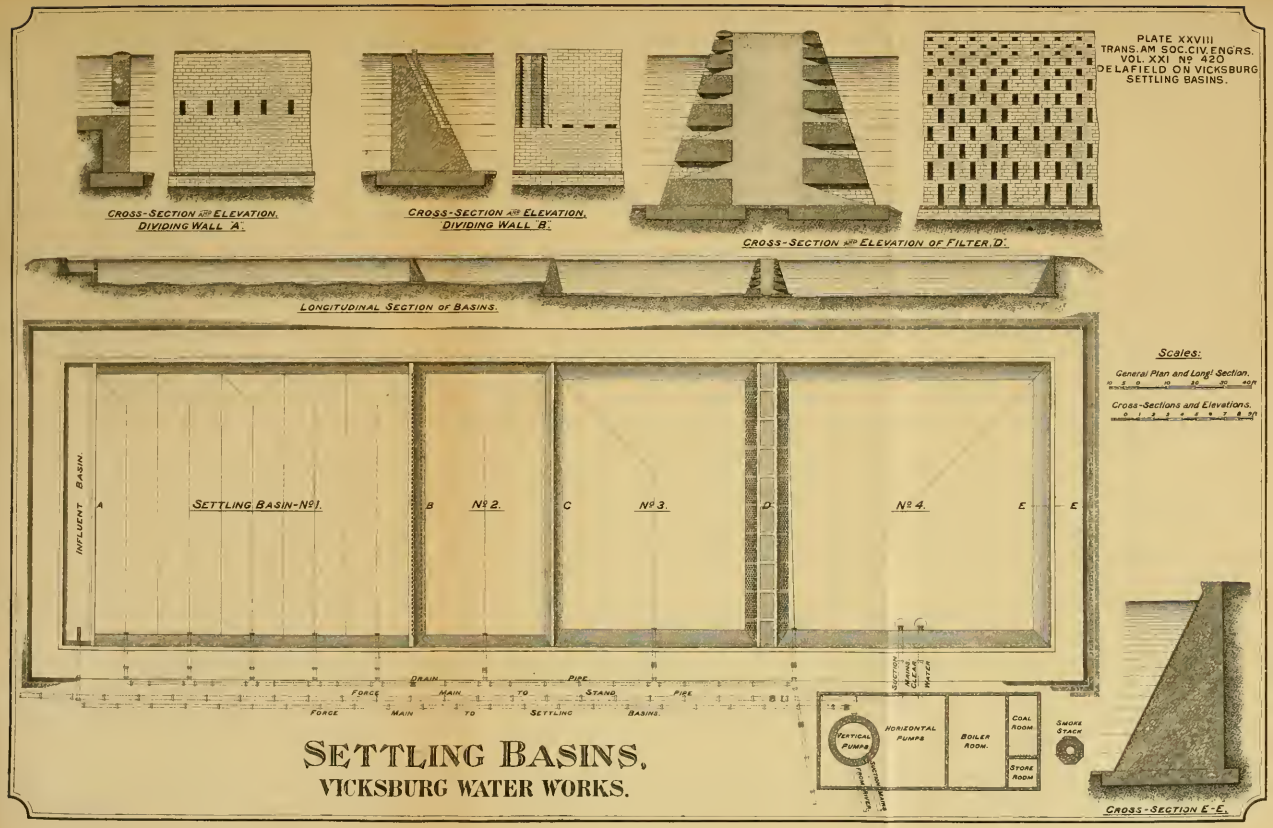
I would advise, in any future constructions, the use of a permanent system of piping, with numerous outlets of small aperture, directed toward the drain pipes, for the purpose of washing the deposited mud from the basins and the fouled sand from the filter. These pipes, being attached to the main system of water supply, and under that pressure, would rapidly do the work required, carrying the silt and sand into the river. This would unquestionably save labor in cleaning the basins, at the cost of pumping the amount of water required. Coal used in making steam for power is undoubtedly cheaper than the labor of man.

Objection may be made that puddle was not used instead of brick walls in this design. The answer is that a puddle clay could not be found in the locality, as all clays in the neighborhood contain a large percentage of sand, rendering them pervious and unfit.

Slopes covered with brick or béton I have always found uncertain and difficult to render water-tight; they also retain a large proportion of the deposited material; for these reasons I have preferred brick walls.

A limited experience demonstrates that these basins easily give 500 000 to 600 000 gallons clear water, pumping only ten hours *per diem*, and I believe their capacity for twenty-four hours' use *per diem* to be over one million gallons.

As the average amount of material carried in suspension is about 186 grains per gallon, I estimate the deposit at 265 cubic feet per million gallons, or 38 million gallons per foot in depth of deposit in Settling Basin No. 1; hence, cleaning the basins once each month will be sufficient, and this can easily be done during a night.



SETTLING BASINS,
VICKSBURG WATER WORKS.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

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421.

Vol. XXI.—August, 1889.

MAXIMUM RATES OF RAINFALL.

By DESMOND FITZGERALD, M. Am. Soc. C. E.

The diagrams accompanying this paper show the principal maximum rates of rainfall at Chestnut Hill Reservoir, Boston, between the years 1879–89. (Plates XXIX, XXX, XXXI, XXXII, XXXIII.)

Previous to this time the observations were of the ordinary kind, viz., those taken from a simple rain gauge which was weighed after each storm. The writer had been impressed for some years with the importance of possessing an automatic and continuous record, but none of the gauges upon the market seemed to possess all the qualities which were desirable; accordingly, a gauge was designed and built in 1878 to plot a profile of all rains, and the following diagrams are tracings made directly from the record sheets of this gauge. It will be noticed that the vertical scale is 2 inches of paper to 1 inch of rainfall. The scale was doubled in this manner to admit of close approximations to hundredths of an inch in readings by the eye. The horizontal scale of hours gives an inch of paper to the hour. The cylinder on which the sheets are wrapped is sufficiently large to record 6 inches of rainfall, but larger amounts can

easily be plotted by drawing off the water from the receiving tank. A description of the gauge has already been printed in *Engineering News*, May 31st, 1884.

The figures on the margins of the diagrams give in each case the total rainfall, as recorded by a standard gauge, placed with its rim one foot above the ground. The time of the beginning and ending of the rain is given, the time of beginning and ending of the "down pour," and also the proper correction for the elevated gauge due to its height above the ground. It is already a well-established fact that the higher we rise above the ground the less rain we can collect, unless special construction is resorted to. This result is probably due to the fact that the velocity of the wind increases from the surface upwards so materially as to affect the amounts collected. The receiver of the self-recording gauge being placed on the roof of a small building about 25 feet above the ground, it becomes necessary to make the proper correction for altitude, which is done by taking the ratio of each storm in the upper gauge to that in the ground gauge and applying this percentage to any particular portion of the storm. Generally the down pour portion of the storms only is given, as, for our particular purpose, it is desirable simply to show the maximum rates.

As the question of the proper sizes for sewers has recently been occupying the attention of the Society, these maximum rates have been selected from the records of the past ten years in the hope that they may be of service as an index of what may be expected in the vicinity of Boston.

These storms are not necessarily those which have given the greatest amounts of rainfall, or caused the most wide-spread damage. This class of rains, as far as applicable to drainage areas of any extent, is generally spread over several days, and may not give a large rate per hour at any portion of its profile.

In examining the diagrams, it will be noticed that the rain which gave the most vertical profile, and consequently the greatest maximum rate, occurred on July 18th, 1884, and yielded at the level of the ground 0.70 inches in five minutes. (Plate XXXI.) The rain literally fell in a flood, but its duration was short, and it was a local shower, probably confined to an area of a few hundred acres. This fall, which is at the rate of 8.4 inches per hour, might almost be doubted, but it has also been observed elsewhere.

A rate of 9.6 inches per hour, lasting for six minutes, has been recorded in Washington, D. C.

The rain of July 20th, 1880 (Plate XXX), yielded 1.46 inches in forty minutes.

The storm of August 21st, 1888, recorded on Plate XXXIII, was remarkable, extending, as it did, over a wide area, and yielding 3.22 inches in three hours and twenty minutes. Near the end of this storm it rained 0.85 inches in twenty-two minutes, which is at the rate of 2.3 inches per hour.

During some of the winter months the self-recording gauge has not been in use, and from this cause the interesting rain of February 10th to 14th, 1886, escaped the record. The experience of the writer on the Boston Water Works covers the period 1873–89, and during this time there have been but two great freshets at the sources of supply. Although foreign to the immediate purpose of this paper, a few words in regard to these two storms may not be out of place. The second of these was the one above alluded to. The first took place in March, 1876. There was a considerable amount of snow upon the ground, and on this snow a warm rain of 2.27 inches fell on March 21st, 1876.

This caused a freshet, but was farther reinforced on March 25th by another rain of 3.20 inches, which carried the flow of the Sudbury River up to a maximum rate for a fraction of a day of nearly two thousand million gallons in twenty-four hours (drainage area, 76 square miles.)

The following is an abstract from a report by the writer to the Boston Water Board upon the other freshet of 1886:

"On February 10th there was quite a body of snow upon the ground, probably equivalent to 2 inches of rainfall. At 7 P.M. on the above day rain began to fall and continued until noon of the 13th. The total rainfall upon the Sudbury River water-shed was 4.64 inches, but adding the snow on the ground would increase this amount to over 6 inches. The greatest freshet that I have ever seen on the works followed. The snow melted but slowly at first, or the flow of the streams would have been even larger than it was. On the 13th the yield of the river was very nearly 2 000 000 000 gallons in twenty-four hours. The maximum rate of yield was from 7 A.M to 12 M. of the 13th, and was at the rate of 2 136 000 000 gallons in twenty-four hours. The total yield of the 76 square miles from the 11th to the 18th inclusive was 6 505 000 000 gallons. The weather during the freshet was mild and the snow was all

melted from the ground. The following shows the yield for each day:

February 11th	149 600 000	gallons.
12th	918 600 000	"
13th	1 994 700 000	"
14th	1 287 300 000	"
15th	837 800 000	"
16th	531 000 000	"
17th	428 700 000	"
18th	357 400 000	"

Other important data in regard to this storm may be found in the report of the Stony Brook Commission. Emil Kuichling, M. Am. Soc. C. E., in a recent paper before the Society,* has given some abstracts from this report, and it will be unnecessary to repeat the data here.

Larger rainfalls than these two cases cited have often occurred during other portions of the year when the evaporation and percolation have prevented the water from getting to the streams, but they have never happened to occur in recent years on the Boston Water Works, when the ground was frozen, or when there was an accumulation of snow, but in designing waste weirs and channels, it is evident that the engineer must provide for just this contingency, which is almost certain to occur sooner or later.

In urban districts the problem is, of course, a different one. Here percolation and evaporation may not enter materially into the question, but it is extremely necessary for the engineer to have some data like that taken from self-recording rain gauges to guide him in proportioning the capacities of his sewers. Considering the importance of the subject, it is a matter of considerable wonder that more of these gauges are not in use.

Certainly, every large city should have one or more, properly located and in competent hands for maintenance. Records of this kind, covering long periods, will prove of value for many purposes connected with the practice of the engineer.

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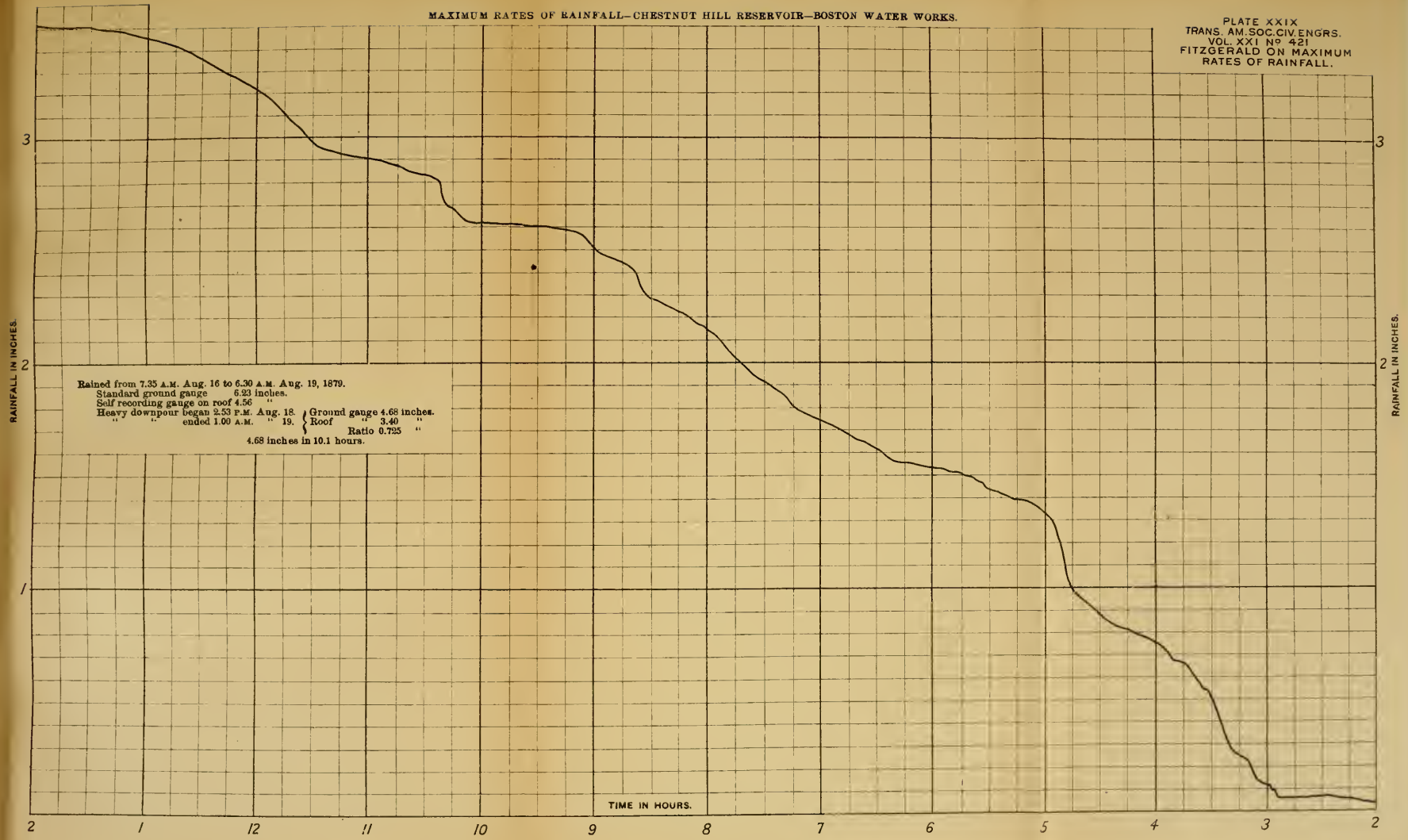
* Transactions, Vol. XX, p. 1, January, 1889.

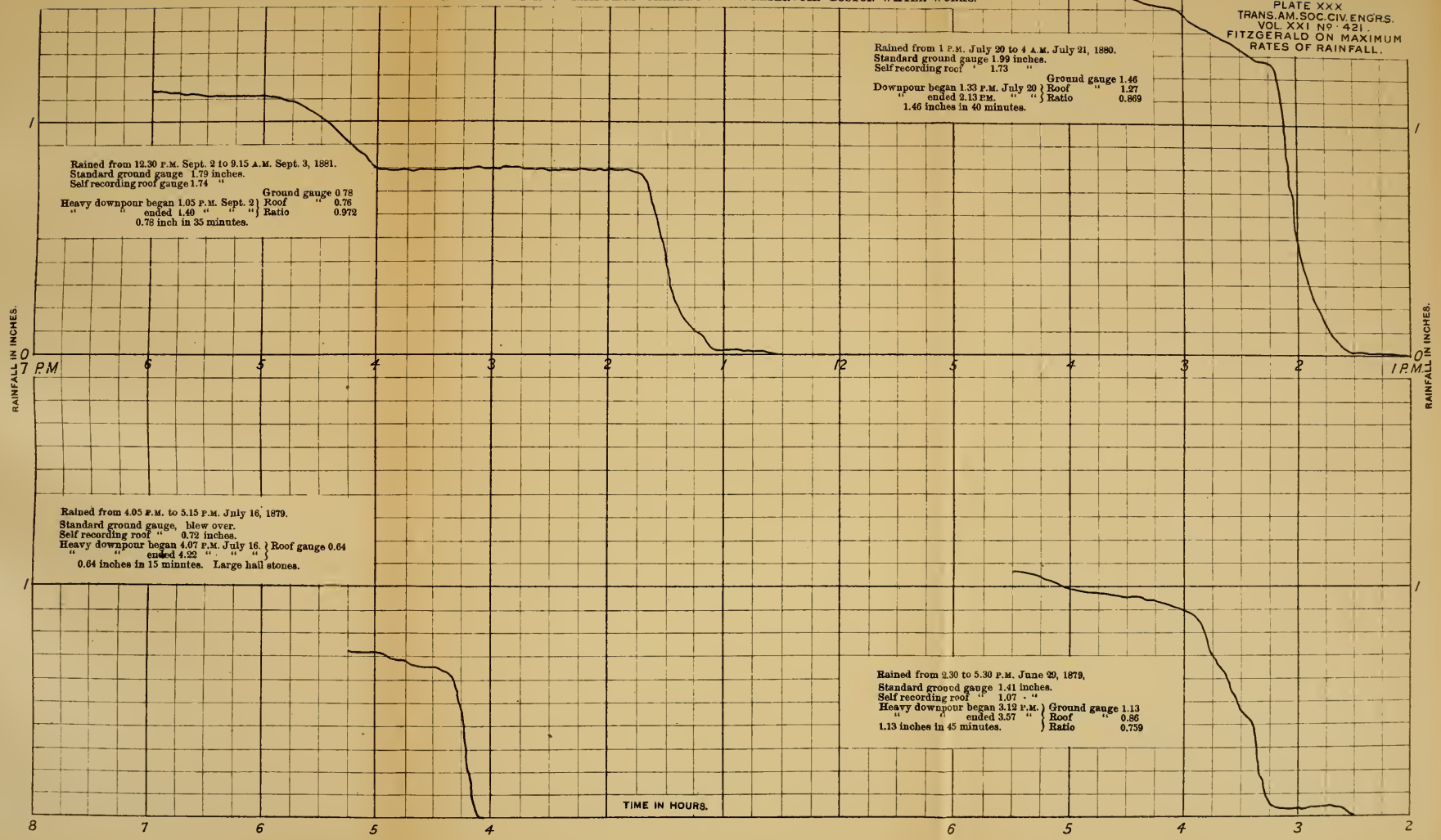
MAXIMUM RATES OF RAINFALL—CHESTNUT HILL RESERVOIR—BOSTON WATER WORKS.

PLATE XXIX
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXI No 421
FITZGERALD ON MAXIMUM
RATES OF RAINFALL.



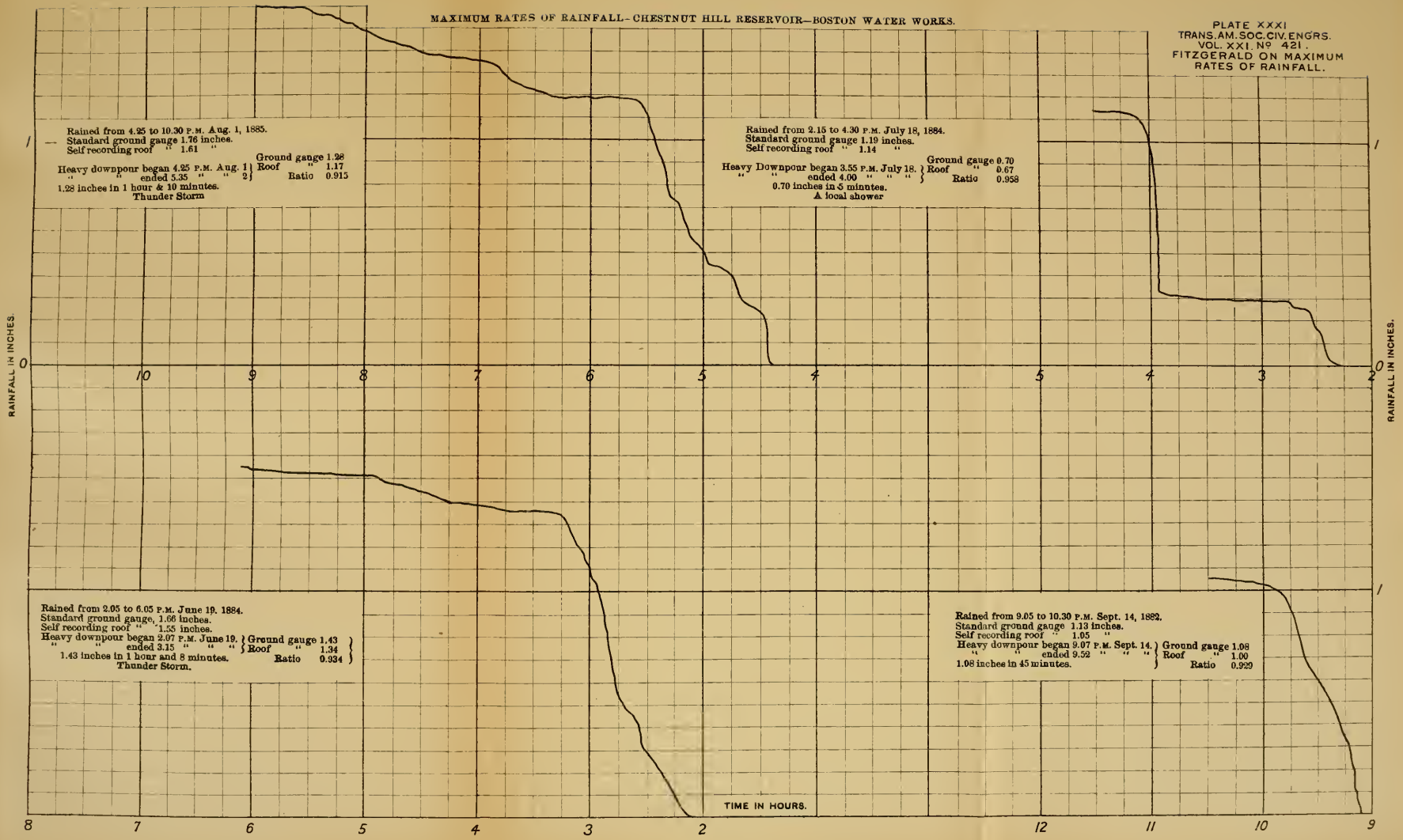
MAXIMUM RATES OF RAINFALL—CHESTNUT HILL RESERVOIR—BOSTON WATER WORKS.

PLATE XXX
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXI No. 421.
FITZGERALD ON MAXIMUM RATES OF RAINFALL.



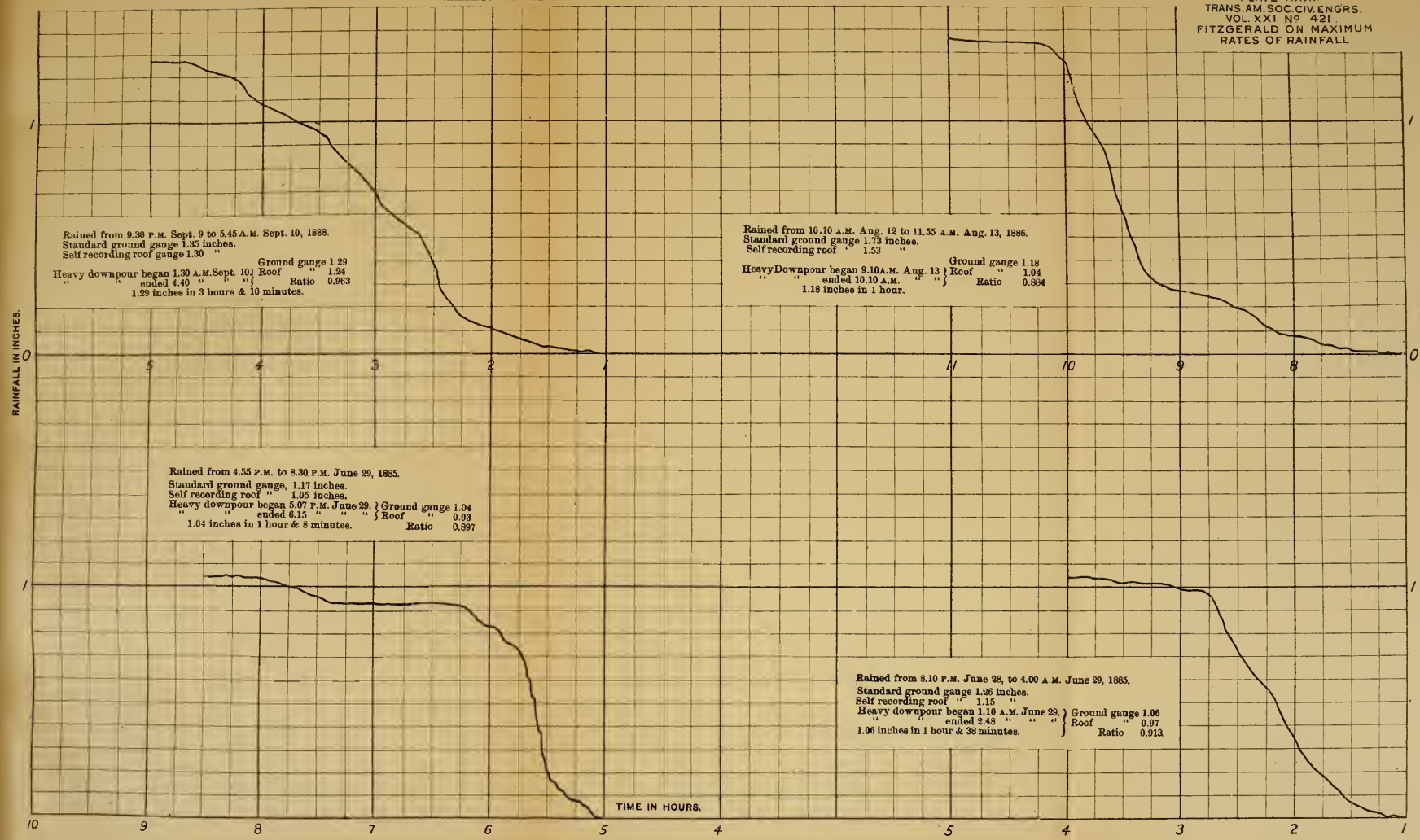
MAXIMUM RATES OF RAINFALL—CHESTNUT HILL RESERVOIR—BOSTON WATER WORKS.

PLATE XXXI
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXI. Nº 421.
FITZGERALD ON MAXIMUM
RATES OF RAINFALL.



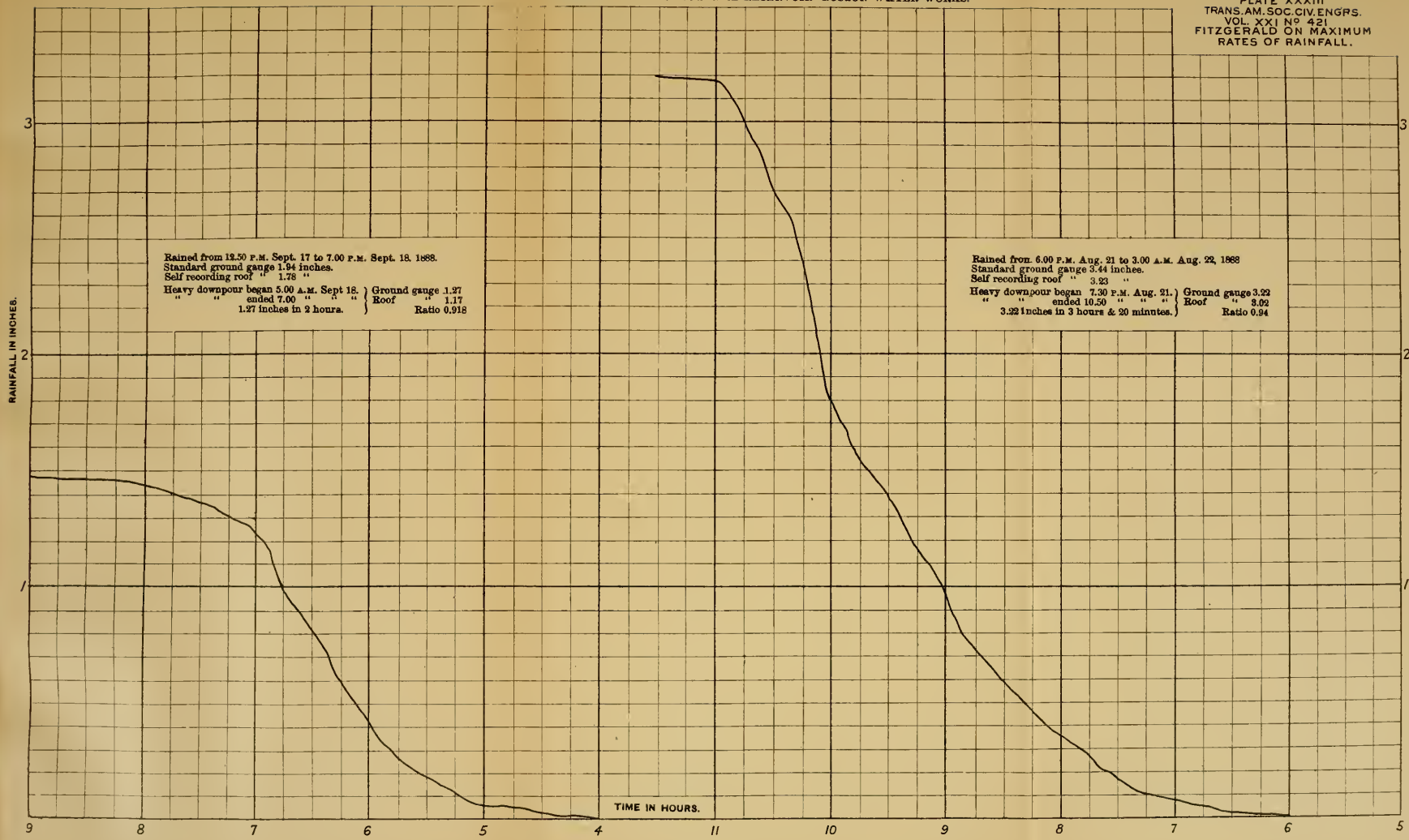
MAXIMUM RATES OF RAINFALL—CHESTNUT HILL RESERVOIR—BOSTON WATER WORKS.

PLATE XXXII
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXI No 421
FITZGERALD ON MAXIMUM
RATES OF RAINFALL.



MAXIMUM RATES OF RAINFALL—CHESTNUT HILL RESERVOIR—BOSTON WATER WORKS.

PLATE XXXIII
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXI No 421
FITZGERALD ON MAXIMUM
RATES OF RAINFALL.



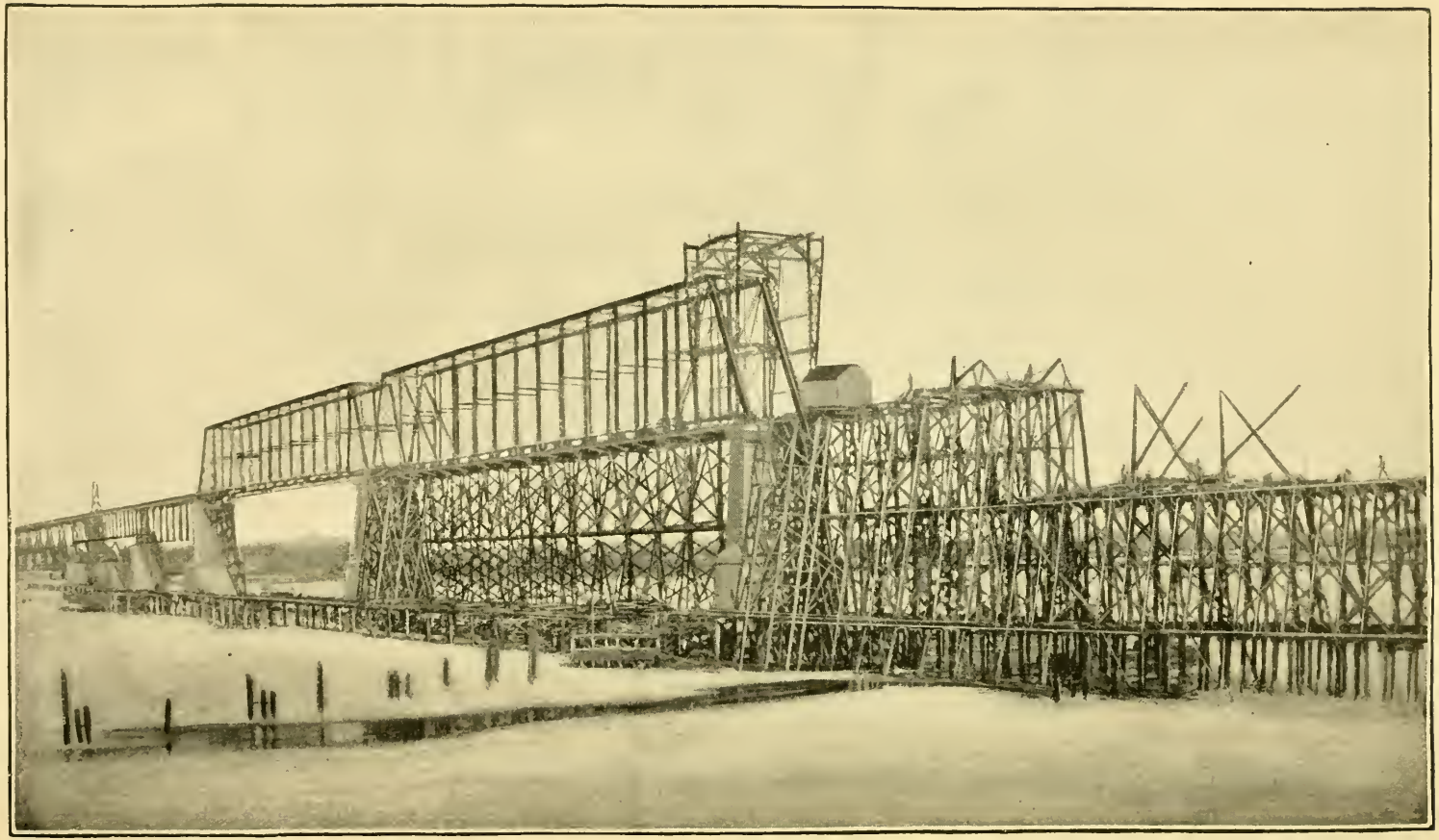


PLATE XXXIV. SIBLEY BRIDGE IN PROGRESS. Vol. XXI, p. 97.

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422.

Vol. XXI.—September, 1889.

THE SIBLEY BRIDGE.

By O. CHANUTE, JOHN F. WALLACE and
W. H. BREITHAUPT, Members Am. Soc. C. E.

INTRODUCTION.

By O. CHANUTE, M. Am. Soc. C. E.

The bridge over the Missouri River at Sibley, Mo., was built under an act of Congress which became a law July 3d, 1884, and which granted to the Kansas City, Topeka and Western Railroad Company, its assigns and successors, the alternative of either building a low bridge, with spans not less than 160 feet in the clear, or a high bridge with spans of not less than 300 feet in the clear, with, in the latter case, the lowest part of the superstructure at least 50 feet above extreme high water mark.

When, in December, 1886, it was decided to extend the Atchison, Topeka and Santa Fé Railroad from Kansas City to Chicago, these rights were transferred to the "Sibley Bridge Company;" Mr. A. A. Robinson,

M. Am. Soc. C. E., was appointed Chief Engineer, and the writer was made Consulting Engineer in charge of the works.

Previous surveys and estimates had contemplated a low bridge; but the more complete surveys, which were begun as soon as it was decided to build the bridge, led to the conviction that it was best to build a high bridge.

It was found that the Channel in the vicinity of the bridge site had shifted some 1 100 feet from 1878 to 1882, and some 700 feet more from 1882 to 1886, with the prospect of still further changes in the near future, so that not only would the low bridge entail a considerable and uncertain expenditure for controlling the channel through the draw-bridge, but it would have been far less favorable for navigation.

It was therefore decided that a high bridge should be built, and the choice lay between two locations.

The upper one was at "Sibley Point" proper, a spur of hills or bluffs jutting out from the general line of the valley, and extending northerly for about half a mile, so as to afford supporting ground for an east and west high bridge line.

The other and lower location was at what is known as the "Sibley Reef," about half a mile below, and involved crossing in a north and south direction, at right angles with the general trend of the valley.

It would have been much preferred to locate the bridge at the upper crossing. Not only was this in the general direction of the road, but it saved nearly a mile in distance and required far less curvature.

The objections to this location were as follows :

First.—It was at the upper end of a "bend" in the river, or rather at the lower end of a "crossing" in which the channel of the river, after impinging against one shore, crosses its bed diagonally, and impinges against the other shore. At such points the channel is proverbially unstable, and changes with the abrasion of the shores, so that a greater number of high through spans, under which boats could pass, would have been required than at the other and more stable location.

Second.—The river was wider at the upper than at the lower location, requiring a bridge 2 550 feet long, as against a bridge 2 000 feet long.

Third.—The bed rock was at greater depth at the upper location; being from 42 to 47 feet below the low water datum, as against 30 to 40 feet at the lower location.

These facts so influenced the cost that the estimates were $1 109 700 for the upper crossing, and $814 950 for the lower or "Sibley Reef" crossing.

The latter was adopted. It derives its name from a reef of boulders which stretches nearly half way across the river, and which abuts on the south or bluff side, upon the remains of a glacial moraine, the only one which the writer knows of above low water in the Missouri River.

The surveys and borings demonstrated the glacial origin of this moraine and reef. The boulders in the bank and in the bed of the river largely consisted of granite, porphyry and metamorphic sandstone, which are not found in their native beds anywhere within three hundred miles of the bridge site. Indeed it is believed that the parent rocks are chiefly found near Lake Superior, and the probabilities are that these boulders drifted down all the way from that region during the glacial period, the site of the bridge being very near the southern line of the glacial drift, as laid down upon geological maps.

Where the bed rock under the river was reached in the pneumatic caissons, it was found striated and streaked with the shallow grooves which are generally considered the marks of glacial action, and all the concurrent circumstances led to the belief that the valley of the Missouri River had been chiefly excavated out of the surrounding bluffs, by the slow and resistless action of a great glacier, bearing upon its upper surface the boulders which had been deposited in the moraine.

The cause of the latter deposit becomes apparent by the inspection of a map of the valley in this vicinity. It is seen that the exact location of the moraine at Sibley Reef was the heel or pivot of a change of direction in the glacier, which thus left a quiet corner in which the boulders in transit were deposited. This is confirmed by the borings, which disclose the fact that the river now flows over the shallowest bed rock to be found between the two bluffs, 4½ miles apart, which bound the valley. Half way between these, under the bottom land, the bed rock was found to be abraded to a depth of 55 and 60 feet below low water, gradually rising thence to the bridge site, where, under the two main piers in the river, it was but 30 feet below the same datum.

The borings showed, moreover, that at the bridge site, the bed rock, instead of being covered with sand, was overlaid with 8 to 10 feet of blue and yellow clays, gradually tapering out toward the north side, thus indicating that after the glacier had ceased its excavating action,

there had been a still pool at this spot, in which the clays had been deposited; their upper surface having subsequently been covered by the boulders, swept down from the glacial moraine by the action of the river, so that it seems certain that the current at this point has never excavated to the bed rock, as so frequently happens during floods at other points in the river.

It will thus be seen that this point offered exceptional conditions of security for the foundations of bridge piers if carried down to bed rock. All anxiety about future undermining might be safely dismissed, and the site being below the bend in the river, it is equally certain that the channel cannot materially change.

The only drawback was that the boulders and clay were likely to increase the cost of pneumatic excavation, but this was practically offset by the shallower depth.

Arrangement of Spans.

Comparative estimates were made of various arrangements of spans, considering the cost of the piers and of the superstructure, and it was found that the best and most economical arrangement was to cover the river, which is here 1 200 feet wide at low water, with three spans of 400 feet each. The following distribution was therefore adopted, the spans and piers numbering from the south or bluff side, toward the north side of the river:

	Feet.
One shore span—deck; Piers 1 to 2	200
" river " through; Piers 2 to 3	400
" " " " " 3 to 4	400
" " " " " 4 to 5	400
" sand bar span—deck; " 5 to 6	250
" " " " " 6 to 7	175
" " " " " 7 to 8	175
Total	2 000

In addition to which an iron viaduct was built over the bottom lands, 1 900 feet long, in spans alternately 60 and 30 feet, resting upon masonry pedestal piers, and there was added also a temporary wooden trestle, resting upon piles and 3 600 feet long, which is to be filled in hereafter.

The flood marks of 1844 and 1881 were pointed out by local residents upon an old half ruined brick house, about a mile above the bridge site. These proved to be several feet higher than other marks

adopted by the United States Engineers at points both above and below; but as it was thought best to adopt the highest water marks, as understood at the point of location, the bottom chord of the through spans was placed 50 feet above the high-water mark of 1844.

It will be noted from the foregoing description that there are thus but two piers exposed to the full force of the current in the bed of the river at low water. These are placed parallel with the current, the bridge line being at right angles thereto.

Upon the completion of the surveys and final location, plans were duly submitted to the Secretary of War, and his approval was obtained in the spring of 1887.

Character of Foundations.

The bed rock in the Missouri River has generally been found to slope rapidly from one side to the other. It is generally found abraded to a depth of 20 to 30 feet below low water near one bank, and sloping to a depth of 70 to 100 feet near the other. At "Sibley Reef," however, in consequence of the formation which has already been described, and of the considerable width of the valley at this point, it was found practically level under the present site of the low-water river, and thence sloping north gradually for 3½ miles to a depth of 60 feet below low water. Thence it rose again for about 1 mile to the bluffs which form the northerly boundary of the valley. A line of borings across the bottom lands disclosed the fact that the greatest abrading action of the glacier had occurred towards the northern side of the valley, the one furthest away from the bridge site.

At the latter point the bed rock was found at a depth of 30 feet below low water, under Pier No. 2, on the south bank of the river at low water. Thence it ran practically level for 1 200 feet to the north bank of the river, under Pier No. 5, at which it was 31.5 feet below low water. Beyond this it dipped 9 feet in 425, under the sand bar, to the site of Pier No. 7, which was the furthest northerly that it was deemed best to carry down to the rock, and thence the bed rock continued dipping under the bottom lands.

Across the river the bed rock was found covered—first, with a thin stratum of fine sand or gravel not more than 2 feet thick. Above this there lay beds of yellow and blue clays, gradually tapering from the south to the north side, and covered with the boulders which had been

whirled down from the glacial moraine, and these boulders tapered from a thickness of some 20 feet at Pier No. 2, to a thickness of 7 or 8 feet at Pier No. 5, thus acting practically as a continuous bed of rip-rap around the river piers. Above this again there lay at low water the ordinary sand and silt of the Missouri River, which, as is well known, is frequently swept away at high water.

In order that no question should ever arise concerning the stability of the foundations, it was decided to carry down to bed rock, by the pneumatic method, the two piers in the bed of the river, and three out of the four across the sand bar—that is to say, Piers Nos. 3, 4, 5, 6 and 7.

Pier No. 1, or the south abutment, being located well inland, was founded upon the yellow clay or silt composing the bluff. Pier No. 2, at the low-water southerly edge of the river, was founded upon a mass of boulders 20 feet thick, underlaid by blue clay, and Pier No. 8, on the north abutment, was founded upon piles driven in the sand, which is here more or less intermixed with boulders.

The following table exhibits the depths at which each pier is founded, and the character of its foundations:

FOUNDATIONS OF SIBLEY BRIDGE.

Pier Nos.	Height of Pier. Feet.	Depth foundation at low water.	Founded on
1	25.75	+ 34.	Yellow clay and silt.
2	79.	+ 5.	20 feet of boulders underlaid with clay.
3	114.	— 30.	Bed rock.
4	113.4	— 29.4	" "
5	115.5	— 31.5	" "
6	89.22	— 34.6	" "
7	81.85	— 40.5	" "
8	41.22	00	Piles and grillage.

It will be noted from the above that the greatest depth to the bed rock was 40.5 feet, under Pier No. 7 on the sand bar, and that the highest pier above its foundations was Pier No. 5, which is 115.5 feet high.

The weights which these impose on their several foundations will be given in a subsequent table.

DESIGN OF MASONRY.

It was decided to build the piers of "Cotton wood" stone. This is a dolomite which is found on the line of the Atchison, Topeka and Santa

Fé Railroad in Kansas, about 180 miles from the bridge site, and which has been proved in similar works to resist well the effects of the weather and frost.

As a measure of additional safety, Piers Nos. 3 and 4, the two in the main river, were cased in Minnesota granite, from about low water mark to a point 12 feet above, this forming an ice breaker in the shape of a Gothic arch, with the radii diminishing toward the top.

The form and dimensions of these piers at various points will be found on Plate XLII.

The following table exhibits the size of the masonry at top and bottom, the area of the foundations, the estimated weight when loaded with the trains and the resulting pressures :

FOUNDATIONS OF SIBLEY BRIDGE.

Pier No.	Under coping.		Shape of Starling.	Size of foundation.	Area of foundation. Square feet.	Weight in tons.	Tons per square foot.
	Thick.	Length.					
1	6 feet.	24 feet.	None.....	33′ 8″ x 15′ 8″	527	764	1.45
2	8	28	Round both ends....	54 x 22	1 188	3 628	3.05
3	9	30	Round and Gothic...	63′ 9″ x 27′ 5″	1 748	6 770	3.87
4	9	30	" " ...	63′ 9″ x 27′ 5″	1 748	6 621	3.78
5	8	28	Round both ends....	43 x 26	1 118	6 041	5.40
6	8	26	None.....	42 x 24	1 008	3 466	3.43
7	9	25	"	36 x 18	648	2 478	3.82
8	9	25	"	84 piles.	.....	1 071	13 T per pile.

In order to save both weight and expense, Piers Nos. 7 and 8 were carried up but 4 feet above the high-water mark of 1844. Above this they were surmounted with iron towers, which latter carry the deck spans over the sand bar.

The 1 900 feet of iron trestle across the bottom lands is carried on twenty-one iron towers, each resting on four masonry pedestal piers, built of Cotton wood cut stone above the surface of the ground. Below this they are built of concrete, increasing in size by successive steps, so as to give a bearing area upon the soil which shall not exceed a pressure of 1 ton to the square foot. This was deemed judicious because of the light character of the silt composing the bottom lands, and in some instances, where the ground was soft, the areas were extended so as to limit the pressures to ¾ of a ton to the square foot ; the foundations being generally carried down from 8 to 10 feet below the surface.

Levels taken since the bridge has been opened to traffic show that these pedestal piers have settled very uniformly, by the compression of the soil from $\frac{3}{8}$ to $\frac{3}{4}$ of an inch, except at two points, where the settlement has been $1\frac{3}{8}$ inches and $2\frac{1}{2}$ inches, respectively. This will be rectified by the insertion of iron shimming plates, under the iron legs of the towers resting upon the masonry and carrying the alternate 30 and 60 feet iron plate girders of the superstructure.

Construction.

As soon as the location was decided upon, and the surveys had sufficiently progressed, a contract was entered into (January 10th, 1887) with the firm of Sooysmith & Co., to put in the foundations and to build the masonry, at an agreed schedule of prices. This firm was at that time completing the substructure and masonry for a similar bridge at Randolph, some 20 miles above Sibley, and as fast as practicable it transferred its plant and forces to the latter point.

This contract proved thoroughly satisfactory. The work was carried through in a less period of time than at any bridge heretofore built across the Missouri River, and it was thoroughly well done. The foundations were made perfect, the masonry was very good, and the job was satisfactory in every respect.

As there were over 30 000 tons of materials to be received (timber, stone, cement, iron, etc.) for the foundations, masonry and superstructure, the first requirement was to obtain, if possible, a connection by rail with the bridge site. The Wabash Railway passed within 3 miles, and an arrangement having been made with that company, second-hand rails and ties were procured from the Atchison, Topeka and Sante Fé Railroad, with which a service track was laid about 3 miles long, from the Wabash Railway to Sibley Bridge site, together with about a mile of the necessary sidings. This was operated by a light locomotive which the contractors procured, and being completed in February, the accumulation of material began.

On March 23d, 1887, ground was broken at the bridge site, by commencing excavation for the pile foundations of Pier No. 8, or the present north abutment.

From this time forward the work was prosecuted vigorously; but as the operations were begun in the spring of the year, when two floods were to be encountered, that of April from the breaking up of the upper

river, and that of June from the melted snows in the mountains, it was not practicable to begin first upon the two river piers (3 and 4), as would have been desired, and the first pneumatic foundations were accordingly put in on the sand bar.

The following condensed table shows the dates of beginning the excavation at each pier, of the completion of the foundation proper, of the beginning of laying the masonry after the foundation was completed, and of the finishing of the pier.

SCHEDULE OF SUBSTRUCTURE WORK—SIBLEY BRIDGE.

Foundation.	Character.	Sinking Begun.	Foundation Completed.	Masonry Begun.	Pier Completed.
8.	Piles.	March 23d, 1887.	May 10th, 1887.	May 13th, 1887.	July 8th, 1887.
7.	Pneumatic.	April 22d, "	Aug. 2d, "	Aug. 5th, "	Aug. 11th, "
6.	"	May 7th, "	Aug. 22d, "	Aug 22d, "	Sept. 8th, "
5.	"	June 7th, "	Aug. 25th, "	Sept. 21st, "	Oct. 13th, "
4.	"	Aug. 27th, "	Oct. 29th, "	Oct. 30th, "	Nov. 18th, "
3.	"	Aug. 27th, "	Nov. 22d, "	Nov. 22d, "	Dec. 15th, "
2.	Excavation.	Aug. 6th, "	Aug. 17th, "	Sept. 21st, "	Oct. 21st, "
1.	"	June 8th, "	June 20th, "	July 14th, "	July 28th, "
Pedestals.	"	April 14th, "	July 27th, "	June 6th, "	Sept. 1st, "

Meanwhile proposals had been received from various bridge works for the superstructure, and the contract having been awarded to the Edgemoor Iron Company on the 15th of January, 1887, the plans were made, the materials ordered and rigidly inspected, and the manufacture of the parts began at the shops in June, the iron viaduct being first taken in hand.

The following table shows the dates of erection at the bridge site of the several spans:

SCHEDULE OF ERECTION OF SUPERSTRUCTURE—SIBLEY BRIDGE.

Between Piers.	Span.	Date Begun.	Date Completed.
Iron Viaduct.	60 and 30 feet.	July 25th, 1887.	Sept. 10th, 1887.
7 to 8	175 feet deck.	Sept. 13th, "	Sept. 24th, "
6 " 7	175 "	Sept. 30th, "	Oct. 10th, "
5 " 6	250 "	Oct. 13th, "	Oct. 30th, "
4 " 5	400 feet through.	Nov. 24th, "	Dec. 15th, "
3 " 4	400 "	Dec. 24th, "	Jan. 19th, 1888.
2 " 3	400 "	Jan. 28th, 1888.	Feb. 11th, "
1 " 2	200 feet deck.	Nov. 22d, 1887.	Dec. 12th, 1887.

While the writer was in general charge of the works, and visited them frequently, the more immediate charge of the substructure and erection was placed in the hands of Mr. John F. Wallace, M. Am. Soc. C. E., who was appointed Resident Engineer.

The supervision of the plans, and inspection of the materials and shop work for the superstructure, was likewise intrusted to Mr. W. H. Breithaupt, M. Am. Soc. C. E., who was appointed Assistant Engineer of superstructure.

These two gentlemen having carried through their respective tasks to a successful termination, and completed an important bridge which is thought to reflect credit upon all concerned, they are herewith left to tell how they did it, by continuing this narrative.

CONSTRUCTION.

By John F. Wallace, M. Am. Soc. C. E.

On March 14th, 1887, the writer was placed in charge of the construction of the Sibley Bridge as Resident Engineer. Prior to this date the location and general plan had been determined upon by O. Chanute, M. Am. Soc. C. E., Consulting Engineer, and approved by A. A. Robinson, M. Am. Soc. C. E., Chief Engineer. A contract for the substructure had been let to Sooysmith & Co., of New York City; a service track 3 miles long, with the necessary sidings, had been constructed connecting the bridge site with the Wabash Western Railway near Orrick, Missouri; extensive borings, under the immediate direction of Otto Sonne, M. Am. Soc. C. E., had also been made covering the entire vicinity of the bridge site.

During March the various piers were located, base lines were laid out and carefully measured, and the triangulation made connecting the various bases and points upon both sides of the river. Levels were run connecting the various bench-marks of both the railroad and Government surveys and checked on the high-water marks pointed out by the old settlers. The relation between the railroad datum, which was adopted as the bridge datum, the St. Louis Directrix, and the datum of the Missouri River surveys by the United States Engineers (which is mean gulf tide at Biloxi, Miss.) was found to be as follows:

United States datum, mean gulf tide.....	0.000
Chicago, Santa Fé and California Railroad datum	135.613
The St. Louis Directrix.....	413.300

In reference to the railroad datum, low water was assumed to be 550.000, being 685.613 feet above the sea level. High water of 1881 was found to be at an elevation of 580.300. High water of 1844 was found to be at an elevation of 587.400. The lowest part of the iron work was calculated to be placed at an elevation of 637.400, giving a clear headroom for navigation of 50 feet between extreme high water and the bottom chord of the bridge, the fluctuation between high and low water being 37.400. The elevation of the base of the rail was calculated to be 642.500, 92.5 feet above low-water mark. On account of transportation facilities it was decided to conduct the work from the sand bar on the north side of the river, and to construct the viaduct foundations and

the foundations of the piers situated upon the sand bar first, leaving the construction of the main piers located in the river proper until after the subsidence of the spring floods. Work was, therefore, commenced at the site of Pier 8, on the 23d day of March, 1887, by excavating the surface material down to a level with the stage of the water in the river. In this excavation a coffer-dam was constructed and completed April 8th. This coffer-dam was sunk to a depth of 20 feet below the level of the sand bar, and the inclosed sand removed by a centrifugal pump. Inside of the coffer-dam eighty-four white oak piles 50 feet long were driven. The piles were cut off below low-water mark. These piles penetrated a stratum of boulders and clay at a mean depth of 20 feet below low water. The final penetration of these piles did not in any case exceed 1 inch during the last ten blows of a 3 200-pound hammer falling 25 feet. The pile driver was operated by a double drum Mundy hoisting engine. Pile driving was completed May 10th, the work being slow and tedious on account of the length of the piles and the hardness of the material into which they were driven. On May 13th the grillage was constructed on the piling and the laying of masonry commenced. The masonry was completed to the coping by May 25th, the coping was added July 8th, finishing the pier.

The foundations of Piers Nos. 7, 6, 5, 4 and 3 were constructed by the pneumatic process in the order named.

Pier No. 7.

The caisson for the foundation of Pier No. 7 was constructed in place and sinking commenced April 22d, 1888, with the cutting edge at an elevation of 560.000. The sinking progressed rapidly through sand until May 5th, when at an elevation of 536.0 boulders were encountered, which became very numerous and compact as the sinking progressed. At an elevation of 518.0 a ledge of limestone rock 2 feet thick, covering about one-fifth of the area of the bottom of the caisson, was encountered, which required blasting for its removal. Bed rock was finally reached August 2d and the masonry on the pier completed August 11th.

Pier No. 6.

The caisson for this pier was also constructed in place; sinking commenced May 7th, and was completed August 22d. The character of the material through which this caisson passed was sand, boulders and

clay in the order named. The cutting edge was sunk to an elevation of 515.4; the masonry on the pier was completed September 8th. The details of this pier are shown on Plate XLI.

Pier No. 5.

The caisson for this pier was constructed on the shore ½ mile above the bridge site, and on May 3d was launched and floated into approximate position between mooring piles which had previously been driven to assist in placing the caisson. As soon as the caisson was anchored to the piling, a sand bar commenced to form below and on the in-shore side, which caused so much trouble in bringing the caisson into proper position that five days intervened before the exact position was obtained. This was accomplished by keeping a heavy strain upon the mooring lines leading in the desired direction, and, at the same time, jetting around the edge of the caisson. In order that the caisson might be as light a draught as possible, only one course of roofing was placed upon it until after it was placed in final position, after which the seven remaining courses of decking were constructed. The sinking commenced upon June 7th and continued until August 18th, at which date the caisson landed upon the bed rock at an elevation of 518.5. Under this pier the bed rock sloped 3 feet in the length of the foundation. The high portion of the ledge was removed by blasting until a uniform bearing was obtained and the foundation firmly secured. By August 25th the air chamber and shafts were filled with concrete and the laying of masonry under way upon the pier above. During the sinking only as much masonry was laid in position as was needed to provide weight for sinking the foundation and to keep the work above the surface of the water. The material through which the caisson passed in sinking consisted of a conglomerate mass of sand, gravel, boulders and clay, which became very hard and compact near the bed rock. While in sinking the foundations of Piers Nos. 6 and 7 much of the material, consisting of sand and silt, was removed through the blow-out pipes by the air pressure. Most of the excavation for this foundation was loosened by pick and hoisted through the air lock in the excavating shaft. The manner of operating the excavator, and the drawings for the same, are shown by the details on Plate XL, which also shows the general plan of caisson used. The size and details, however, on the drawings are of Caisson No. 6. Pier No. 5 was completed October 13th.

Pier No. 4.

Owing to the high stage of the June flood and its prolonged duration, it was not deemed expedient to place the caissons for Piers Nos. 3 and 4 in position in the stream until the waters commenced to subside during August. The caissons had, however, in the meantime been constructed and were in readiness to launch when the proper time arrived. The caisson for Pier 4 was launched August 9th and was floated into position and secured between the mooring piles without incident. Afterward seven courses of decking were added and 9 feet of crib work filled with concrete constructed upon the top of the caisson. Sinking commenced August 27th and continued until October 23d, on which date the cutting edge of the caisson was landed securely upon bed rock at an elevation of 520.0. In sinking Caisson 4, boulders were encountered in the sand, at once followed by several strata of hard, compact clays, which continued within a short distance of bed rock, when a shallow stratum of gravel was encountered immediately upon the rock. It was found necessary to remove fully 90 per cent. of the material excavated in sinking this foundation through the excavating lock, which rendered the sinking slow, tedious and expensive. On October 29th, the working chamber having been filled with concrete, the air was cut off, and the laying of masonry was pushed forward upon the pier, which was completed November 18th.

Pier No. 3.

The caisson for Pier No. 3 was launched August 24th. Prior to launching Caissons 3, 4 and 5, false bottoms were constructed over the lower part of the working chambers to render them light of draught. After securing the caissons in position, and filling the chambers with compressed air, these bottoms were broken up and the material out of which they were constructed was shoved out under the cutting edge. In launching Caisson 3, the false bottom was broken by contact with a snag, and, as the water was not deep enough to float the structure, it grounded in some 8 feet of water. One of the barges containing an air compressor was at once brought alongside and the caisson raised by pumping air into the working chamber. The steamer "Park Bluff" then took the caisson and compressor barge in tow and dropped down the stream toward the bridge site. The current at "Sibley Reef," at the stage of water then existing, ran so fiercely that the steamer was not able to hold the

fleet in tow, and when the future position of the caisson was approached, stone anchors were cast, and by their help the caisson was swung into position between the mooring piles, and then allowed to settle upon the river bed. The bottom of the river at this point was covered with boulders from 1 to 9 feet in diameter. The caisson rested upon the boulders in such a manner that the lower right hand corner was some 5 feet lower than the upper left hand corner, and after the false bottom was removed, a channel some 9 feet deep and 30 feet wide was found to exist, running diagonally across under the caisson, which was upheld by boulders of various sizes under different parts of the cutting edge. Owing to the peculiar position of the caisson and the fierceness of the current, the water inside of the working chamber dashed to the roof, and rendered all work in the chamber slow, difficult and dangerous. But few workmen could be induced to enter the chamber, and these were compelled to use life lines, which were attached to the body of one workman and held by two of his fellows clinging to the interior bracing of the chamber. The compressed air would, of course, only keep the water down level with the highest corner of the chamber, even with the water calm and no current existing. It was necessary to level the caisson and to secure a bearing for the cutting edge before the air pressure could be relied upon to clear the chamber of water and enable the work to be pushed with force. The work of blasting the boulders and distributing the fragments over the bottom of the chamber continued day and night. Sand bags were also cast into the river on the upper side of the caisson, and were carried through to the lower side of the chamber by the current and assisted in stopping the channel under the caisson. After twenty-one days and nights of continuous effort, the cutting edge was finally embedded in the boulders and the full complement of pressure men induced to enter the working chamber. The average elevation of the top of the boulder bed was 548.0. At 535.0, after passing through a compact mass of boulders and gravel, a stiff blue clay was encountered which was free from gravel and sand and picked apart in large dry flakes. At 529.0 the blue clay gave place to a tough yellow clay, very hard and stiff, which was so difficult to pick that blasting was rendered necessary to loosen it. At 525 a stratum of gravel and sand was found, which continued to the bed rock, which was reached at an elevation of 520.0 on November 13th. After filling the working chamber and the different shafts with concrete, the laying of masonry on the pier was

resumed, and finally completed December 15th, 1887. The masonry on the pier was laid during freezing weather. Warm brine containing eight per cent. of salt and the best Louisville cement was used in the mortar. Subsequent examinations have shown the mortar in this pier to be equal in quality to that used during weather of a higher temperature. The completion of Pier No. 3 finished the substructure, as the construction of the shore Piers Nos. 2 and 1, resting upon beds of concrete 8 feet in thickness, had been carried forward and completed during the progress of the work on the other piers. The eighty-four foundations for the viaduct towers had also in the meantime been completed. These foundations consisted of stone piers from 8 to 12 feet in height, resting on beds of concrete 8 feet in thickness, which were so proportioned that the weight on the soil beneath should not exceed 1 ton to the square foot.

The following quantities of material were used in the substructure:

Piling, 3 360 lineal feet.
Timber in caissons and cribs, 1 070 636 feet B.M.
Iron in caissons and cribs, 127 776 pounds.
Concrete in air chambers, 1 625 cubic yards.
Concrete in cribs and foundations, 4 174 cubic yards.
Limestone masonry, 7 094 cubic yards.
Granite masonry, 313 cubic yards.
Limestone coping, 416 cubic yards.
Granite bridge seats, 38 cubic yards.
Viaduct foundations, 670 cubic yards.
Total masonry, 8 521 cubic yards.
Riprap, 4 200 cubic yards.
Displacement of caissons, 234 417 cubic feet.
Boulders excavated from caissons, 78 788 cubic feet.
Excavation for dry foundations, 4 814 cubic yards.

Cement.

The following amount of cement of different manufacture was used in the construction of the substructure:

Portland cement	500	barrels.
Milwaukee "	100	"
Utica "	1 468	"
Kansas City "	4 440	"
Louisville "	3 506	"
Total	10 014	"

The Portland cement was used in sealing the air chambers and in pointing. The Louisville cements were used in laying the masonry.

The Kansas City and Utica cements were used, mainly, in foundation and concrete work. All cement used was first stored upon arrival in a cement house and was carefully tested before being permitted to enter the work. During the season seventeen car loads of cement were rejected out of the total amount received.

Water Records.

Owing to the work being conducted from the sand bar on the north side of the river, and to the constant fluctuation for which the Missouri River is noted, it was necessary to provide some system by which we could be duly warned of the approach of flood waves. The records of the various water gauges on the Missouri River from Kansas City to Sioux City were diagramed, in order that we might have a graphical presentation of the previous flood waves. Daily telegraphic reports were received from Sioux City, Omaha, Plattsmouth and Kansas City, and were recorded upon this diagram. After the first flood we were able to determine the approach and probable height of the rise of the water during the succeeding forty-eight hours. Under the peculiar circumstances existing at Sibley Reef, the information derived from these reports and this diagram was of great assistance in the prosecution of the work.

Erection.

On July 5th, 1889, the first car load of material for the superstructure arrived at the bridge site, and on July 25th the first tower for the viaduct was raised. The viaduct was erected by means of a traveler, which "straddled" the entire structure on legs resting on wheels that ran on rails located upon each side and outside of the two lines of foundation piers. Between the two lines of piers a standard gauge track was laid connecting with the material yard tracks; upon this track under the viaduct the cars containing the material for the superstructure were placed. The material was then lifted directly into place by the traveler without further handling. This "straddler" was also used to erect the three deck spans between Piers Nos. 5 and 8. The last girder for the viaduct was hoisted into place September 10th. The progress table on page 105 gives the dates of the erection of the various spans. The long spans over the low water river were erected from false works resting upon piling driven into the river bed by the use of an ordinary traveler. The character of the river bed at "Sibley Reef"

enabled us to drive piling for this false work without fear of scour, but the erection not being completed by the time the ice began to run in the river, we experienced considerable trouble by the ice cutting the piling and the work was in constant danger from moving ice during the erection of the three 400-foot spans. Up to the 19th of December, although the weather was cold, yet it did not prevent the vigorous prosecution of the work. On that date a "blizzard" bore down upon the works from the northwest, and for four weeks storm followed storm in rapid succession, covering the false work and lines with ice and snow, the thermometer at times falling as low as 25 degrees below zero. The work was continued and completed during this weather, as it was feared that as soon as the weather moderated the ice would move out of the river, carrying away our false work and as much of the spans as might be in course of erection at the time. During this period two of the workmen lost their lives by falling from the false work and one by slipping from the ice into the river.

In order to open up the Chicago, Santa Fé and California Railroad for traffic, the false work between Piers Nos. 2 and 3 was erected strong enough to bear up trains and also the span in course of erection, and on the 26th of January the first train was permitted to cross the bridge, and this span was subsequently erected without interfering with the operation of the railroad. On February 11th, 1888, the last end pin was driven and the bridge substantially completed. Fortunately the ice in the Missouri River did not move until after this, to us, very important event, but the day following the ice moved out of the river.

Rectification Works.

In bridging the Missouri River, a very important matter to consider is the proper means to adopt to retain the river under the bridge proper and to prevent the structure from being flanked by the turbulent stream. The valley of the Missouri is generally from 4 to 6 miles wide, and the bed rock from 30 to 80 feet deep below the top of the alluvial deposit. This alluvial deposit consists generally of a very fine silt, but beds of clay and black soil are occasionally found. The stream itself swings from side to side of the valley like a mighty serpent, until it strikes the limestone bluffs which bound it. During and succeeding the spring floods the concave bends are continually cutting and the bars on the convex side of the river continually increasing. The banks

are generally from 25 to 30 feet above the low water river, and are covered with from 1 to 3 feet of black soil washed into the valley by the tributary streams. This land is thickly settled and covered with beautiful farms, which, in the convex bends, are to a greater or less extent yearly sacrificed to the river. The greatest amount of cutting occurs during the falling water succeeding a rise, the water running toward the river through the fine silt and continually undermining the banks. Where some points are temporarily held by a deposit of stiff clay, the bank below is rapidly cut out by the fierce action of the eddy that is generated by the water whirling around the point extending into the river, and the clay points are eventually flanked. The progress of the cutting has been known to extend from $\frac{1}{2}$ to $\frac{3}{4}$ of a mile back in the convex bends in a single season. The expense of controlling the river in the vicinity of the bridges that have been built over the Missouri River has, in one or two cases known to the writer, eventually exceeded the cost of the bridge itself. At Sibley the location of the south end of the bridge is against the bluffs, and the land on that side of the river lying along the bluffs above the bridge is low and uninhabitable. The problem consisted simply in our ability to hold the bank on the north side of the river for a distance of some 4 miles above the bridge site connecting on to rectification works previously constructed by the Wabash Western Railway, and letting any cuttings that might occur on the south side of the river, as the result of our proposed works, take care of themselves. As a first step toward securing the data that would give the necessary information to enable the works at Sibley Bend to be planned and executed in the most economical manner, a careful examination of the different Government Surveys extending over a period of from ten to fifteen years was made.

In the winter of 1887 a careful survey was made of the river in the vicinity of Sibley. In August of 1887 a second survey was made. (Plate XXXVII.) Between these two surveys the river cut back a distance of some 1 200 feet in the bend above the bridge. The survey of January, 1887, showed that the deep-water channel of the river followed close to the north shore line around the entire bend. Owing to the material in the upper part of the bend being softer than that in the lower part, and a clay point existing about $\frac{1}{4}$ of a mile above Dike "C," the bank in the upper part of the bend cut very rapidly, while little or no cutting occurred below the center of the original bend; this caused the current to

deflect toward the south side of the river, and temporarily protected the bank immediately above the bridge site. It was decided to construct Dike "C," shown on Plate XXXVII, a distance of 700 feet into the river, in the upper part of the lower bend as it existed in August of 1887; it was also decided to construct Dikes 1, 2, 3, 4, 5 and 6 in the lower part of the upper bend, and, as an extra precautionary measure, Dikes A and B were planned to protect the convex bank about $\frac{3}{4}$ of a mile above the bridge site. At a point marked "Mud Bar" (see Plate XXXVII), a stratum of hard blue clay existed, which, in a measure, had protected the convex bank immediately below it. This bar, however, as shown by the difference between the dotted and full lines, was wearing away at the rate of about 100 feet a year. During the fall and winter of 1887 the dikes heretofore mentioned were constructed. Dikes 1, 2, 3, 4, 5 and 6 were constructed as submerged dikes, in the following manner: a foundation mattress was first constructed on inclined ways on the river bank above the location of the dikes as follows: a lattice foundation was laid upon these ways, consisting of 2 x 6-inch strips of timber, crossing each other at intervals of 4 feet; hard wood pins were placed at the intersections of these timbers, in holes bored for the purpose, and extending some $2\frac{1}{2}$ feet in height; around these pins long willows, from 15 to 20 feet in length, were woven to a depth of 2 feet; cross binders of 2 x 6-inch timbers were then placed on top of the mattress, holes being bored through them to receive the pins; the tops of the pins were then split, wedges inserted and driven to place, securely binding the entire mattress together. These mattresses were generally 60 feet wide by 100 feet long. After being completed they were launched into the river, and, being held by lines from the shore, were floated into position, the in-shore end resting against the shore; the lines holding these mattresses were so arranged that, after sinking the mattress, by pulling a check line they could be detached and secured. After the mattress was floated into position, barges loaded with rock were brought alongside and stone thrown upon the mattress. The greatest weight was placed upon the upper part of the mattress, and as it slowly sank below the water the barge was floated over it until it was entirely covered, the current assisting in sinking the mattress. After the mattress was sunk to the bed of the river, additional stone was thrown upon it until it was covered with about 2 feet of rock; an additional mattress was then constructed and sunk upon it. This was continued

until the dike was brought to the height planned, each succeeding mattress being smaller in its several dimensions than the one below it. Where these dikes were constructed in water 25 feet deep 100 feet from the shore, they consisted of three mattresses, and the entire dike was covered with a layer of rock about 2 feet thick. The crest of these dikes sloped about 5 to 1 as originally constructed, it being the intention to have the dikes cause as little resistance to the flood waters as possible, to the end that the smallest amount of eddy should be created. Dikes A and B were constructed after the river had frozen over, and were built upon the ice directly over the location of the dike; the ice was then cut around the dike, in order that the weight of the material of which it consisted might cause it to sink; the ice on the upper end being cut first, the pressure of the current assisted in sinking the dike, and the buoyancy of the ice caused it to shoot out from underneath; afterward additional rock was placed upon the shore end of the dike, securing a firm connection with the shore. The long dike "C" consisted of a foundation mattress 40 feet wide, 2 feet thick, covered by 18 inches of rock, constructed in place. This dike was 700 feet long. At the outer end an ice breaker, consisting of a pile of rock 9 feet high and 80 feet long, was constructed. Between this ice breaker and ordinary high water mark two rows of piling were driven through the mattress, the rows being 8 feet apart, and the piles being 10 feet apart in the rows. A close wattling consisting of 2 x 4-inch scantling was woven in and around this piling in such a manner as to retard the current in order to cause a deposit above and below the dike. The top of this wattle fence, at the shore end, was 13 feet above low water, sloping to 9 feet above low water at the outer end. The flood of 1888, which was the first flood that occurred after the construction of the dikes mentioned above, rose to an elevation of 17 feet above low water, and caused the deposit of a heavy bar above and below Dike "C," about ¾ of a mile long, and from 500 to 600 feet in width, the elevation of the top of the bar being, on an average, 13 feet above low water. In June of 1888, after the subsidence of the spring floods, a survey was made, and is indicated on Plate XXXVII by a full black line. The difference between the dotted line marking the shore line of August, 1887, and the full line, shows the change that occurred during the spring floods of 1888. It will be noted that at the location of Dikes A, B, 1, 2, 3, 4, 5 and 6, the two shore lines are coincident; but immediately below Dikes 1, 2, 3, 4, 5 and 6 cutting

occurred in the form of scallops; this cutting was caused by the return action of the eddies generated by the current around the ends of the dikes, but, as shown on the sketch, the general shore line in the lower part of the upper bend was substantially maintained and additional land made advancing the shore line in the lower bend above and below Dike "C." After the spring flood Dike 7 was constructed. During the autumn of 1888, additional mattresses were sunk in the eddies on the flanks of the different dikes, the bank sloped above them and rip-rapped. Soundings taken in the fall of 1888 showed that the dikes in the upper bend had gradually settled at their outer ends, and that the slope of the crests of the dikes had steepened to about 1½ to 1 at the outer ends of the dikes, changing the slope of the crests of the dikes to a convex form. The channel had also deepened opposite the dikes from 25 to 50 feet, the greatest depth of water occurring some 50 feet below the outer points of the several dikes. The floods of 1889 have caused no additional changes to the shore line, and the object of the protection works has been obtained so far at a very reasonable cost, 4 miles of river being controlled at an expense, to date, of about $60 000.

SUPERSTRUCTURE.

By W. H. Breithaupt, M. Am. Soc. C. E.

The superstructure, resting on masonry, consists of three main spans of 400 feet each, flanked on the south, here the bluff side of the river, by one 200-foot span, and on the north, in the order named, by one 250-foot span, two 175 foot spans and 1 900 feet of viaduct, the whole being on a tangent, the truss spans level, and the viaduct track on a grade of 0.8 foot to the hundred. The three 400-foot spans are through spans; on all other parts of the bridge the track is uppermost. In the 400-foot, 200 foot and 250-foot spans there are iron stringers and floor beams; in the 175-foot spans and in the viaduct spans the ties rest directly on top chords of the trusses and girders respectively.

Span lengths as above given are from center to center of piers. Lengths, center to center of end pins, and other general dimensions of the various spans, are as follows:

400-foot spans.—Length, center to center, of end pins, 396 feet.
Number of panels, 16; panel length, 24 feet 9 inches.
Height, center to center, of chord pins, 50 feet.
Width, center to center, of trusses, 21 feet.

200-foot span.—Length, center to center, of end pins, 197 feet 6 inches.
Number of panels, 8; panel length, 24 feet 8¼ inches.
Height, center to center, of chord pins, 25 feet.
Width, center to center, of trusses, 17 feet.

250-foot span.—Length, center to center, of end pins, 247 feet 1 inch.
Number of panels, 10; panel length, 24 feet 8½ inches.
Height, center to center, of chord pins, 30 feet.
Width, center to center, of trusses, 17 feet.

175-foot spans.—Length, center to center, of end pins, 172 feet 6 inches.
Number of panels, 9; panel length, 19 feet 2 inches.
Height, center to center, of chord pins, 25 feet.
Width, center to center, of trusses, 14 feet.

On Piers Nos. 7 and 8, *i. e.*, under adjoining ends of the two 175-foot spans, and where viaduct adjoins 175-foot span, there are pyramidal iron towers, 22 feet high, which effect a considerable saving in masonry.

The structure is proportioned for maximum strains resulting from the following loading and wind pressure:

Static Load.—Total iron and steel weight, plus floor weight of 480 pounds per running foot for wood and rails.

Moving Load.—Two 86-ton consolidation engines followed by train load of 3 000 pounds per running foot, as per diagram.

Wind Pressure.—On loaded chords, 30 pounds per square foot on train and two surfaces of one truss; on unloaded chords, 50 pounds per square foot on two surfaces of one truss.

In proportioning sections the Launhardt formula was used for the 400-foot spans, but not for the shorter spans. Assumed unit strains are as follows:

400-foot spans.—*Tension.*—Main tie and bottom chord bars (steel), per square inch,

$$12\,000\left(1+\frac{\text{minimum stress in member}}{2\times\text{maximum stress in member}}\right)\text{ pounds.}$$

Strains, however, nowhere to exceed 14 000 pounds per sqare inch.

Counter ties (steel), 8 000 pounds per square inch.

Suspenders (steel), 10 000 pounds per square inch.

Lateral rods, steel, 18 000 pounds per square inch; iron, 15 000 pounds per square inch.

Wind strains in bottom chords, per square inch.

$$15\,000\left(1+\frac{\text{minimum stress in member}}{2\times\text{maximum stress in member}}\right)\text{pounds.}$$

For tension in any built members $\frac{1}{7}$ less than for bars or rods.

Flanges of floor-beams or stringers, 8 000 pounds per square inch.

Compression.—For posts, struts or top chords use

$$\text{formula } b=\frac{s}{1+\frac{l^2}{a\,r^2}}$$

In which b = permissible stress per square inch.
l = length of member in inches.
r = least radius of gyration.
a = 40 000 for columns with two flat ends.
= 30 000 for columns with one flat and one pin end.
= 20 000 for columns with two pin ends.

$$s=10\,000\left(1+\frac{\text{min. stress in member}}{2\text{ max. stress in member}}\right)$$

for end posts and top chords (steel).
= 8 000 for intermediate posts (iron).
= 12 000 for struts (iron).

Flanges of floor beams and stringers, 6 000 pounds per square inch.

Shear.—In webs of floor beams and stringers, 4 000 pounds per square inch.

In rivets and pins, 7 000 pounds per square inch.

Section of rivets in field connections of stringers and floor beams to be increased by 25 per cent.

Bearing.—On rivets and pins, 12 000 pounds per square inch.

Extreme fiber strain on pins, 15 000 for iron, 20 000 for steel.

Permissible pressure of bed plates on masonry, 250 pounds per square inch.

Permissible pressure on expansion rollers $\sqrt{540\,000\ d}$. d = diameter of roller.

All other spans and viaduct:

Tension.—Main ties and bottom chord bars, 12 000 pounds per square inch (steel).

Counters, 8 000 pounds per square inch (steel).

Compression.—For posts and top chords (iron), use formula $b = \frac{s}{1 + \frac{l^2}{r^2}}$ in which $s = 8\,000$. All other values same as given under 400-foot span.

All other unit strains same as given under 400-foot span.

The main tension members, chord pins and flat laterals in all truss spans are of tension steel; top chords and end posts of the 400-foot spans are of compression steel (rivets in them being of tension steel of the maximum ductility given in table); otherwise iron is used.

Steel is specified as follows:

No steel which proves brittle shall be used under any circumstances. If it develops this character at any stage of its manufacture or working, it shall at once be condemned.

All steel for the same span and the same class of work shall be made by the same process and at the same works.

A sample bar ¾ of an inch in diameter shall be rolled from every melt for preliminary tests. If this bar fails to meet requirements, the whole charge may be rejected.

Test pieces of finished steel may be cut either lengthwise or crosswise from the originals.

In addition to regular tests, daily cold bending tests shall be made as often as practicable during the manufacture, by bending double under the hammer such scraps as may be sheared off from the steel being worked.

Tension steel shall stand such forge tests, both hot and cold, as may be sufficient, in the opinion of the engineer, to prove soundness of the material and fitness for the service.

Specimens cut lengthwise and crosswise must, after being heated to a low cherry red, and cooled in water of the temperature of 82 degrees Fahrenheit, stand bending double, in a press or under the hammer, to a curve of which the diameter is twice the thickness of the piece.

No steel must be worked at a black heat.

Requirements for steel and iron are as per following table:

Material.	Ultimate strength per square inch.	Minimum limit of elasticity per square inch.	Per cent. of elongation in 12 diameters.	Per cent. reduction of area at fracture.	Cold Bending.	
					Angle to which specimens must bend without breaking.	Diameter of bend in terms of thickness of specimen.
IRON:					Degrees	
Bar Iron	52 000 $\frac{\text{5 000 by area of original bar}}{\text{circumference of original bar}}$	26 000	18	25	180	1½
Channels and I Beams, Flange	50 000	26 000	15	20	140	1½
Channels and I Beams, Web	47 000	25 000	12	18	140	2
Angles	50 000	26 000	15	20	140	1½
Plates, Under 18″ wide	50 000	26 000	13	18	140	2
Plates, 18″ to 36″ wide	48 000	25 000	10	15	140	3
Plates, 36″ to 54″ wide	47 000	25 000	8	12	90	
Plates, Over 54″ wide	46 000	25 000	8	12	90	
STEEL:						
Compression	75 000 to 85 000	40 000	18	35	180	2
Tension	60 000 to 70 000	35 000	23	42	180	1

Wood in ties and guard rails is well seasoned white oak.

The trusses in the 400-foot spans are of the Whipple, double intersection, type; all other trusses are of the Pratt type. Tension members in general are eye-bars or rods, with die forged heads made in every case wholly by upsetting. Compression members are built of plates and angles, except in the viaduct, where they are of channels. End posts and top chords are trough shaped, with the open sides latticed; inter-

mediate posts have two plate and angle sides and two latticed sides; lateral struts are generally from angles with single latticing for central web, the whole forming a double T section. Flanges are in all cases turned outward. All girders and other beams are built of plates and angles. I beam sections are nowhere used. In packing of eye-bars on pins, pin moments are reduced where practicable, by pairing the bars so as to give minimum resultant moment.

The stringers are in four lines, two main stringers almost directly under the rails, figured for the total loading, and two side stringers, under outer guard rail, of half the strength of main stringers, to act in case of derailment only. The stringers being of an average length of almost 25 feet, it was found economical to use plates in the flanges, in addition to the flange angles. In bottom flanges there is one cover plate, put on as usual, while in top flange, rivet heads being undesirable on tie bed, the required central section is obtained by laying a narrow plate on the vertical leg of each angle. End stringers attach to end floor beams, and nowhere rest directly on piers, so that each span rests wholly on its four pedestals. Ties, except where otherwise described, are 8 inches by 8 inches by 16 feet, spaced 14 inches between centers. There is an inner iron guard rail 8 inches from track rail, and an outer oak guard rail, 18¼ inches from inside of main rail. The oak guard rail is protected on its inner edge by a 2 by 3-inch square root iron angle. (See floor cross-section, Plate XLIV.)

Lateral and sway systems are in every case adjustable, except between girders of viaduct, where stiff bracing is used. Adjustment is by means of a sleeve nut or by clevises, a clevis being used for fork end where two rods of adjacent panels, one in each, come on the same pin.

Further features of the various parts of the superstructure are as follows:

400-*foot Spans.*—Diagonals in the trusses are in two lengths, joined by a pin passing through the center of the intermediate post. Pin hole in the post for this pin is enlarged, leaving room for both post and diagonal to assume their proper directions when span deflects, without distorting either. The post is figured for its full length from pin to pin of chords. Sway rods 1 inch square are at every panel point. Depth of sway bracing from center to center of struts is 23 feet 8 inches. The portals are in one web, covering the upper half of end elevation of span,

made of 3 inch by 3 inch by $\frac{3}{8}$ inch intersecting angles, with heavy ornamental struts at top and bottom. Clearance from top of track rail to bottom of portal is 19 feet.

Bottom chords in Panels 1 and 2 from ends are stiff members, being built of plates and angles. There is a horizontal end stiffening strut from the center of the first intermediate post to the center of the end post.

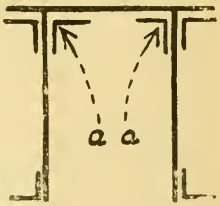
The open sides of intermediate posts are parallel to the center line of the bridge. Floor beams are riveted to posts directly above bottom chord pins, the posts being stiffened, where floor beams attach, by means of a center web. The bottom lateral rods attach to lower flanges of floor beams, which form the bottom lateral struts.

200-*foot and* 250-*foot Spans.*—These spans have each one square and one inclined end. Intermediate sway rods 1 inch square extend the full depth of the trusses. At inclined ends bottom lateral system transmits to top through vertical system at first panel point.

The floor beams rest on top chords directly over posts, and have longitudinal knee braces at their outer ends.

The top lateral rods attach to a plate lying on the top chord and serving as base plate to the floor beam, being riveted to both. This makes a compact and effective connection.

175-*foot Spans.*—In these spans the ties—8 inches x 14 inches x 16 feet, spaced 14 inches center to center—rest directly on the top chords, which are proportioned for both their stress as truss members, and for the bending stress resulting from their direct loading. Continuity of chords brings, with the lower reinforce angles at panel points, the points of contrary flexure 2 feet 1 inch out from pin on either side, so that bending moments need to be taken in the middle 15 feet only of each section. Only the top flange of chord needs addition of material for strain due to bending, as in the bottom flanges, such strain is tension to which is opposed its regular compression strain as truss member. This extra section in top flange is in angles *a a*, which are 14 feet 8 inches long along central part of each chord panel.



Lateral and sway bracing is similar to that in other spans, with the only difference that there are struts in the loaded chord lateral system, there being no floor beams.

Viaduct.—(Plate XLVII). The 1 900 feet of iron viaduct consists of one 58-foot 9-inch girder next to truss span, followed by twenty-one 30-foot and twenty 60-foot 6 inch girders, resting on double bents, forming towers under the 30-foot spans. The girders are spaced 10 feet, center to center; 10-inch x 10-inch x 12-foot ties, spaced 16 inches between centers, and resting directly on girders, carry the track. A foot walk 5 feet wide, resting on wooden brackets fastened to sides of the girders, extends the entire length of the iron viaduct. The longer girders have a depth of 5 feet and the shorter a depth of 3 feet. Bearing on bents is arranged by having in the end of the longer girder a recess of equal depth with the shorter girder. The longer girder rests on the bent and the shorter on the longer. Provision for expansion and contraction is made by having both girders slide on each other and on bent, on every south bent of towers, and having both fixed on every north bent. To bring girders of unequal depth to a common bearing by bending upward the bottom flanges of the longer girder, and reducing depth of its web, entails expensive shop work in the bending of the heavy flange angles, and was for that reason avoided. Arrangement as above necessitates a top longitudinal strut in the tower, but such a strut is a desirable feature anyway. Webs in all girders are $\frac{3}{8}$ inch, with flanges, of section depending on length of girder, made up of angles and cover plates. Lateral and sway bracing between the girders, which is of angles, consists in a horizontal system riveted to under side of top flanges, and in cross-frames of the same depth as the girders. There are seven such cross-frames in the longer and four in the shorter spans. Web stiffeners come at the cross-frames, and half way between them, and are in every case of two 3-inch x 3-inch $\frac{3}{8}$-inch angles, one on each side of web. No filler plates are used under stiffeners; ends are bent to conform to flange angles. Special care was taken to have stiffeners fit in closely to flanges.

The towers are in two stories. Main columns in them are of a trough section, made of two channels and one plate, with open side latticed, the plates being on transverse outer sides of towers. The height of towers from top of pedestal capstone to bottom of girders varies with the grade from 53 feet $1\frac{1}{2}$ inches in Tower 2, to 38 feet $2\frac{3}{4}$ inches in Tower 21. Tops of masonry pedestals, on which towers rest, are level throughout.

The columns fasten to pedestals by means of four anchor bolts in

each. These anchor bolts, $1\frac{1}{8}$ inches in diameter, are built into the pedestals, are four feet or more long—length varying to accommodate thickness of stone courses—and head on plates imbedded in the masonry. The towers have a transverse batter of 1 in 6, at which batter the structure is slightly more than balanced with no tension on windward anchor bolts at maximum wind pressure. Longitudinally the towers have no batter. Longitudinal bracing is proportioned to resist the pull due to stopping of the assumed moving load, friction being taken at 20 per cent. of the load.

Workmanship and finish in superstructure throughout are very creditable. Tension members are made by the regular Edgemoor process, the heads being made in one operation, and are noticeable for their neat finish and uniform good quality.

But little trouble was experienced in getting material fulfilling specifications, and uniform for the various classes. In steel of over three hundred specimen tests, the averages were as follows :

For tension steel, limit of elasticity, 42 000; ultimate strength, 63 900; per cent. of elongation in 8 inches, 26.5; per cent. of reduction of area at fracture, 51.
For compression steel, limit of elasticity, 47 100; ultimate strength, 80 100; per cent. of elongation in 8 inches, 22.7; per cent. of reduction of area at fracture, 44.5.

A little less than 8 per cent. of all tests fell below requirements, and the bulk of these were but little under.

Tests on finished tension members (see Table No. 1) were on the whole very satisfactory.

Two compression specimens (see Plate XLVIII) being as nearly as feasible reduced fac-similes of top chord sections of the 400-foot spans, their size limited by the capacity of the largest testing machine available, were made specially for testing. They developed, fairly closely, the ultimate strengths, as determined by formula, it being in each case about four times the amount of permissible unit strain—(see Table No. 2 and Plate XLVIII). Elastic limit values, if taken as first "set" in shortening of length of specimens, were fully high, but varied considerably in the two specimens. They can only be considered as approximately determined by this test. Deflections showed "set" early in the tests; but such "set" was in each case, until test was well advanced, so extremely small as compared to length of the specimen, as to be of little importance.

Messrs. J. A. Colby and S. C. Weiskopf, M. Am. Soc. C. E., had charge of both mill and shop inspection of the entire superstructure.

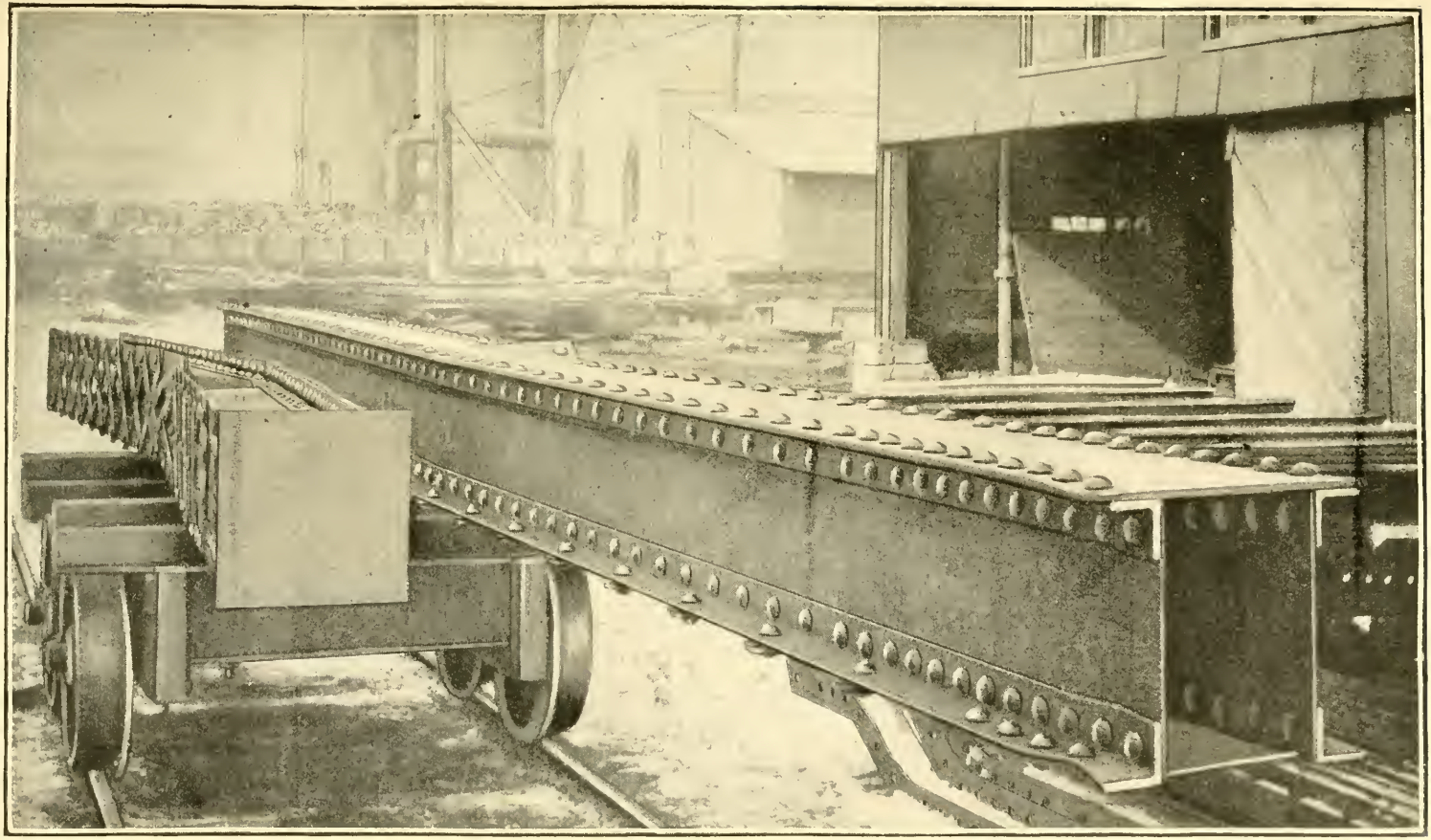


PLATE XXXV. COMPRESSION SPECIMENS AFTER TEST. Vol. XXI, p. 126.

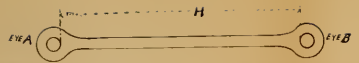
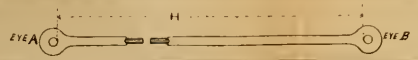


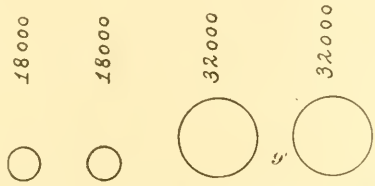
TABLE No. 1.—Tests on Finished Eye-bars.

Section.	Mark and Span.	Area of Section.	B.	Eye A. Diameter of Eye.	Eye A. Diameter of Pin Hole.	Eye A. Per cent. Excess over Bar.	Eye B. Diameter of Eye.	Eye B. Diameter of Pin Hole.	Eye B. Per cent. Excess over Bar.	Elastic Limit. Pounds per Square Inch.	Ultimate Strength. Pounds per Square Inch.	Per cent. of Elongation. In 8 Inches	Per cent. of Elongation. In —.	Per cent. of Elongation. In H.	Per cent. Reduction of Area at Fracture.	Elongation of Pin Holes. Eye A	Elongation of Pin Holes. Eye B.	Location of Fracture	Description of Fracture.	Blow No.	Material Tests on same Blow Numbers. On Preliminary ½-inch Rounds. Elastic Limit.	On Preliminary ½-inch Rounds. Ultimate Strength.	On Preliminary ½-inch Rounds. Per cent. Elongation in 8 Inches.	On Specimens from Rolled Bars. Elastic Limit.	On Specimens from Rolled Bars. Ultimate Strength.	On Specimens from Rolled Bars. Per cent. Elongation in 8 Inches.
Inches.	Ft. In.	Sq. In.	Ft. In.	Inches.	Inches.		Inches.						Ft. ⅝			Inches.	Inches.	Ft. In.								
5 x 1	T_8 172 6....	5	31 1½	12.65	4.92	60.6	12.79	4.93	58.7	44 484	68 959	37.5	23—12.2	11.76	47.5	0.43	0.68	1 7½ from Eye A.....	Silky.....	246 986	44 830	68 040	25.	.....	.....	.....
"	" "	"	"	12.67	"	59.4	"	"	60.3	44 484	65 984	"	"—13.9	13.34	46.68	0.56	0.66	7 9½ " " "	Cupped center; hard edges....	247 141	42 441	62 300	29.6	36 190	66 620	30.5
5 x 1 7/16	T_7 " "	7.19	"	12.62	"	52.9	12.68	"	61.2	46 210	66 930	37.75	"—13.2	12.58	48.54	0.72	0.76	7 2 " " " "	Silky " " " ..	247 020	42 410	65 600	28.	33 540	61 450	27.1
7 x 1¼	CL_2 247 1....	8.75	24 3 5/16	15.75	5.47	44.4	15.81	5.47	47.7	39 826	64 295	43.	22—14.5	13.5	48.6	1.28	1.32	1 8 " " " R.....	Granular; jagged.....	246 831	42 790	63 490	23.7	.....	.....	.....
7 x 1	BL^{18} 396	6.97	24 3	15.87	5.97	43.5	15.87	5.97	42.1	43 611	68 608	36 6	"—13.6	11.2	49.3	1.19	1.27	1 8½ " " " A.....	Silky; granular edges	248 782	42 790	63 260	29.1	34 620	61 600	28.
"	" "	6.90	"	15.82	"	39.7	"	"	43.4	41 905	66 618	45.	"—12.6	11.8	57.2	1.49	1.25	2 3½ " " "	" ⅞ cup. " "	246 631	47 980	64 770	27.	37 700	61 210	27.
7 x 1¼	BL^{24} "	12.3	"	15.75	"	42.	15.75	"	37.2	36 776	59 197	In 12 in. 37.5	"—13.9	13.6	42.7	1.38	1.34	2 3 " " "	50 % silky, " granular.							

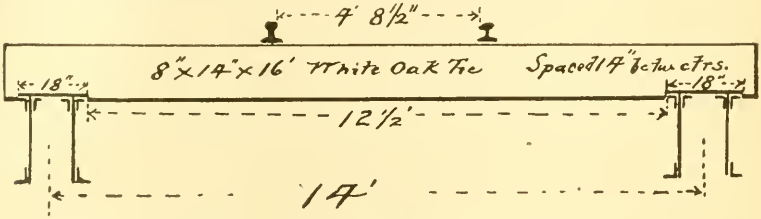


Section.	Mark and Span.	Area of Section.	H.	Eye A. Area of section.	Eye A. Diameter of Pin Hole.	Eye A. Per cent. of Excess over Bar.	Eye B. Area of Section.	Eye B. Diameter of Pin Hole.	Eye B. Per cent. Excess over Bar.	Upset. Diameter.	Upset. Length.	Elastic Limit. Pounds per Square Inch.	Ultimate Strength. Pounds per Square Inch.	Per cent. of Elongation. In 8 inches.	Per cent. of Elongation. In —.	Per cent. of Elongation. In H.	Per cent. Reduction of Area at Fracture.	Elongation of Pin Holes. Eye A.	Elongation of Pin Holes. Eye B.	Location of Fracture.	Description of Fracture.	Blow No.	Material Tests on same Blow Numbers. On Preliminary ½-inch Rounds. Elastic Limit.	On Preliminary ½-inch Rounds. Ultimate Strength.	On Preliminary ½-inch Rounds. Per cent. Elongation in 8 Inches	On Specimens from Rolled Bars. Elastic Limit.	On Specimens from Rolled Bars. Ultimate Strength.	On Specimens from Rolled Bars. Per cent. Elongation in 8 Inches.
Inches.	Ft. In.	Sq. Inches.	Ft. In.	Sq. Inches.	Inches.		Sq. Inches	Inches.		Inches.	Inches.																	
4 x 15/16	CT_6 172 6....	3.75	31 1 5/16	5.83	4⅛	47 7	6.6	4⅛	49.4	2¼	5¼	45 470	48 450	Broke at union of bar to upset.			53.46	All others then carefully reannealed.				252 048	46 600	59 420	25.6		
3 x 15/16	K_2 "	2.81	22 3½	4.61	3¼	64.1	4.58	3¼	63.2	2½	5	48 810	69 720	30.	21—10.4	9.86		0.16	0.15	3' 5½" from Eye A.....	Silky.....	251 334	46 260	66 460	25.4	42 490	65 740	29.4
3 x 1¼	CK_2 247 1....	3.75	27 10½	5.73	3 7/16	52.5	5.83	3½	55.5	2¼	5¼	44 480	67 200	11.25	28—7.8	7.61	8.92	0.31	0.18	1' 11" " " "	Square.....	251 287	43 230	64 280	25.4	36 710	62 660	25.1
4 x 7/8	ACT_2 197 8....	2.75	32 10	3.66	2 15/16	33.	3.81	4⅛	38.5	2¼	4½	47 180	70 097	37.25	32—14.9	14.6	60.1	0.32	0.54	9' 10" " " B.....	Silky, cupped.....	251 284	43 780	65 000	26.2			

On the 175-foot spans where ties rest directly on top chords, and the viaduct where they rest on main girders, the rail being in both cases well within tie support, deflection of ties under load was noted, with the following results:



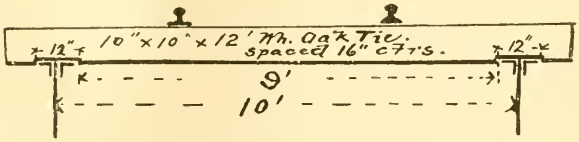
Load—Engine with 18 *tons on truck,* 32 *tons on drivers.*



175-*foot span.*

Downward deflection of center of tie taken on three ties:

	Deflections.
1st Tie.—Ordinary tie; rail bearings on it and on adjacent ties fair. Deflection with engine going at 15 miles per hour	$\frac{1}{4}$ inch.
2d Tie.—Somewhat windshaky. One adjacent tie rail does not bear. Deflection with engine going at 20 miles per hour	$\frac{1}{2}$ "
Deflection with engine stationary	$\frac{7}{16}$ "
3d Tie.—A good tie; rail bearings on it and on adjacent ties good. Deflection with engine going at 20 miles per hour	$\frac{5}{16}$ "



Viaduct.

Deflection taken on one tie only; a good ordinary tie, rail-bearings on it and on adjacent ties fair:

Deflection with engine going at 20 miles per hour..... $\frac{3}{32}$ inch.

SPECIMEN "B"

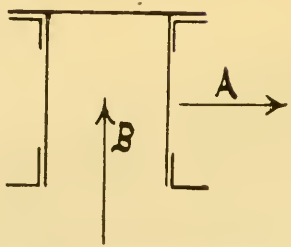


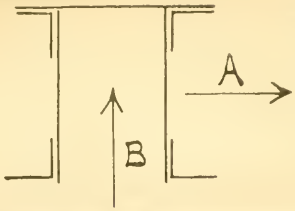
TABLE No. 2.—COMPRESSION TESTS.

TEST No. 1.

Member with two square ends.
All deflections recorded in directions of arrows.
Weight of member..... 1 640 pounds.
" counterbalance ..... 205 "
Counterbalance applied at center of post, and suspended from arm, four times length of arm attached to post.

Load.	Deflections.				Contraction.		Load.	Deflections.				Contraction.	
	A.	Set.	B.	Set.	C.	Set.		A.	Set.	B.	Set.	C.	Set.
Pounds.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Pounds.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
40 000	.00	.00	.05	.00	.03	.00	400 000	.08	.07	.27	.09	.21	.00
80 000	.00	.00	.07	.00	.05	.00	420 000	.14	.10	.27	.09	.23	.00
100 000	.00	.00	.07	.00	.05	.00	440 000	.14	.10	.27	.09	.23	.00
120 000	.00	.00	.08	.00	.06	.00	460 000	.20	.16	.27	.09	.23	.00
140 000	.00	.00	.10	.02	.08	.00	480 000	.22	.18	.29	.11	.25	.00
160 000	.00	.00	.12	.02	.09	.00	500 000	.22	.18	.30	.14	.27	.00
180 000	.00	.00	.12	.02	.10	.00	520 000	.23	.18	.33	.15	.27	.00
200 000	.00	.00	.13	.02	.11	.00	540 000	.23	.18	.33	.18	.28	.01
220 000	.00	.00	.15	.03	.11	.00	560 000	.23	.19	.34	.20	.29	.01
240 000	.00	.00	.17	.03	.12	.00	580 000	.25	.20	.35	.22	.30	.01
260 000	.03	.02	.20	.04	.12	.00	600 000	.26	.20	.40	.27	.30	.01
280 000	.03	.02	.22	.05	.13	.00	620 000	.26	.22	.40	.30	.32	.02
300 000	.03	.02	.22	.06	.13	.00	640 000	.26	.25	.44	.33	.32	.03
320 000	.03	.02	.24	.06	.15	.00	660 000	.30	.26	.58	.40	.34	.04
340 000	.03	.02	.24	.05	.20	.00	680 000	.38	.30	.60	.40	.35	.05
360 000	.04	.03	.24	.06	.20	.00	700 000	.48	.43	.72	.65	.39	.08
380 000	.06	.05	.27	.09	.21	.00	709 800	Failed in direction A and B.					

SPECIMEN "A"



TEST No. 2.

Member with one square and one pin end.
Weight of member..... 825 pounds.
" counterbalance..... 103 "
All deflections recorded in directions of arrows.
Deflection A recorded at center of member.
" B " 5 feet from center of pin.
Counterbalance applied at center of post, and suspended from arm, four times length of arm attached to post.

Tests made by COLBY & WEISKOPF, Inspectors.

Load.	Deflections.				Contraction.	
	A.	Set.	B.	Set.	C.	Set.
Pounds.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
40 000	.00	.00	.04	.00	.00	.00
80 000	.08	.06	.06	.02	.00	.00
100 000	.09	.08	.07	.03	.01	.00
110 000	.09	.08	.07	.03	.01	.00
120 000	.09	.09	.07	.03	.02	.00
130 000	.09	.09	.07	.03	.02	.00
140 000	.10	.09	.08	.03	.03	.00
150 000	.11	.09	.09	.04	.04	.00
160 000	.11	.10	.09	.04	.05	.00
170 000	.11	.10	.09	.04	.05	.00
180 000	.11	.10	.10	.04	.05	.00
190 000	.11	.10	.11	.04	.06	.00
200 000	.15	.10	.11	.05	.06	.00
210 000	.15	.14	.12	.06	.07	.00
220 000	.15	.14	.13	.06	.07	.00
230 000	.15	.14	.13	.06	.08	.00
240 000	.15	.14	.14	.06	.08	.00
250 000	.15	.14	.14	.06	.08	.00
260 000	.15	.14	.14	.06	.09	.00
270 000	.15	.15	.14	.06	.09	.00
280 000	.15	.15	.15	.07	.10	.00
290 000	.15	.15	.16	.07	.10	.00
300 000	.15	.15	.17	.07	.10	.00
310 000	.16	.16	.17	.07	.10	.00
320 000	.16	.16	.17	.07	.11	.01
330 000	.16	.16	.17	.07	.12	.01
.....	.....	.....	.....	.....	.....	.....

Load.	Deflections.				Contraction.	
	A.	Set.	B.	Set.	C.	Set.
Pounds.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
340 000	.17	.17	.17	.07	.13	.02
350 000	.17	.17	.17	.07	.13	.02
360 000	.17	.17	.17	.07	.14	.03
370 000	.19	.16	.18	.08	.14	.03
380 000	.17	.17	.18	.08	.14	.03
390 000	.17	.17	.19	.09	.15	.03
400 000	.17	.17	.20	.09	.15	.03
410 000	.17	.17	.20	.09	.16	.03
420 000	.17	.17	.20	.09	.16	.03
430 000	.17	.17	.20	.09	.16	.03
440 000	.17	.17	.21	.09	.16	.03
450 000	.18	.19	.21	.09	.16	.03
460 000	.19	.19	.21	.10	.17	.03
470 000	.19	.19	.21	.10	.17	.03
480 000	.19	.19	.22	.10	.17	.03
490 000	.19	.19	.22	.11	.17	.03
500 000	.19	.19	.22	.11	.18	.03
510 000	.19	.19	.23	.11	.18	.03
520 000	.19	.19	.24	.12	.20	.03
530 000	.19	.20	.24	.13	.20	.03
540 000	.20	.20	.24	.13	.20	.03
550 000	.20	.20	.27	.14	.23	.04
560 000	.21	.21	.29	.18	.23	.04
570 000	.21	.21	.32	.20	.25	.05
580 000	.21	.21	.36	.25	.26	.07
590 000	.21	.21	.45	.34	.28	.08
595 400	Failed in direction A and B.					

TABLE No. 3.

Superstructure Weights.

Part of Bridge.	Weights in Detail.		Total Weights.		Per cent. of Difference.	Steel or Iron.	
	Calculated from Detailed Drawings.	Scale.	Calculated.	Scale.		Steel.	Iron.
1 900′ Viaduct:							
Girders	958 638	958 820					
Columns	348 513	351 320					
Bracing	480 146	483 742	1 787 297	1 793 882	+0.37	8 126	1 785 756
2—172′ 6″ spans:							
Pier columns			439 200	442 800	+0.82	146 984	295 816
1—247′ 6″ span:			60 333	61 697	+2.26	.....	61 697
Trusses and bracing	395 750	405 612					
Floor	160 917	155 470	556 667	561 082	+0.79	145 734	415 348
3—396′ spans:							
Trusses and bracing	2 842 697	2 905 675					
Floor	781 939	792 420	3 624 636	3 698 095	+2.02	2 156 252	1 541 843
1—197′ 6″ span:							
Trusses and bracing	246 256	246 005					
Floor	129 126	125 290	375 382	371 295	—1.09	79 620	291 675
Totals	.....	.....	6 843 515	6 928 851	+1.25	2 536 716	4 392 135

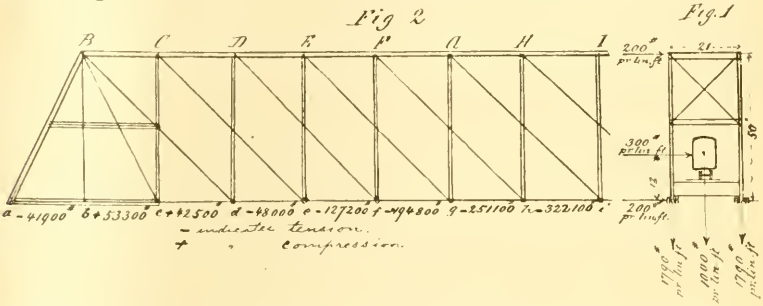
DISCUSSION.

William Scherzer, M. Am. Soc. C. E.—The above paper must be considered a valuable contribution to our bridge literature. I was particularly interested in the description of the superstructure of the 400-foot spans, and will venture a few remarks on my observations. One peculiarity of a Whipple truss of long span, having a Pratt truss for a lower lateral system, is the tendency of the wind to produce compressive stresses in the second and third panels of windward lower chord. It is quite customary, in bridges of this type, to use stiff members in the two end panels of lower chord, but in no instance, to my knowledge, have these stiff members been used in the third panel.

A train of empty box cars weighing 1 000 pounds per lineal foot covering the entire length of span, and a wind force of 30 pounds per square foot on train and exposed surfaces of trusses, are the conditions necessary to produce compressive stresses in the panels mentioned above. For wind on trusses 400 pounds per lineal foot of bridge was assumed, one-half acting on upper and one-half on lower lateral system. This assumption may be considered low. Wind force on train was taken at 300 pounds per lineal foot, as usual. Weight of bridge, including floor, 3 580 pounds per lineal foot. Fig. 1 is a cross-section, showing

points of application of wind forces and vertical loads. It was further assumed that the entire upper wind forces pass through the inclined end posts to the abutments, thereby producing a moment whose horizontal component must be resisted by the lower chords. This moment produces compression in the windward lower chord, which is constant for all members.

Fig. 2 is a diagram showing the resultant stresses due to wind, dead and live loads in lower windward chord. In this instance the second and third panels are the only ones which receive compression.



Enlarging the pin holes at the intersection of the diagonal suspension bars with the vertical posts will certainly relieve the posts of cross-strains during erection; but the bending stresses in posts due to the eccentricity of the lower lateral connections will thereby be increased, and it is an open question whether for this reason it would not be best to hold the posts midway. The end stiffening strut from the center of the first intermediate post to the center of inclined end post cannot be of any service, unless the point of intersection with the intermediate post be made fixed.

The illustrations accompanying the paper on the Sibley Bridge are:

Plate	XXXIV.	January 18th, 1888. Sibley Bridge in progress.
"	XXXV.	Compression test specimens after test.
"	XXXVI.	Missouri River in vicinity of Sibley, Mo., 1887, showing location of bridge.
"	XXXVII.	Missouri River in vicinity of Sibley, Mo., 1887–1888, showing erasion of banks and shifting of channel.
"	XXXVIII.	General profile of Sibley Bridge.
"	XXXIX.	Diagram showing rate of progress in sinking caissons.
"	XL.	General plan of caisson.
"	XLI.	Pier No. 6.
"	XLII.	Pier No. 4.
"	XLIII.	End view of 400-foot span.
"	XLIV.	Cross-section of 400-foot span.
"	XLV.	General details of 200-foot span.
"	XLVI.	General details of 175-foot span.
"	XLVII.	General details of viaduct, and cross-section of viaduct tower.
"	XLVIII.	Compression test specimens.

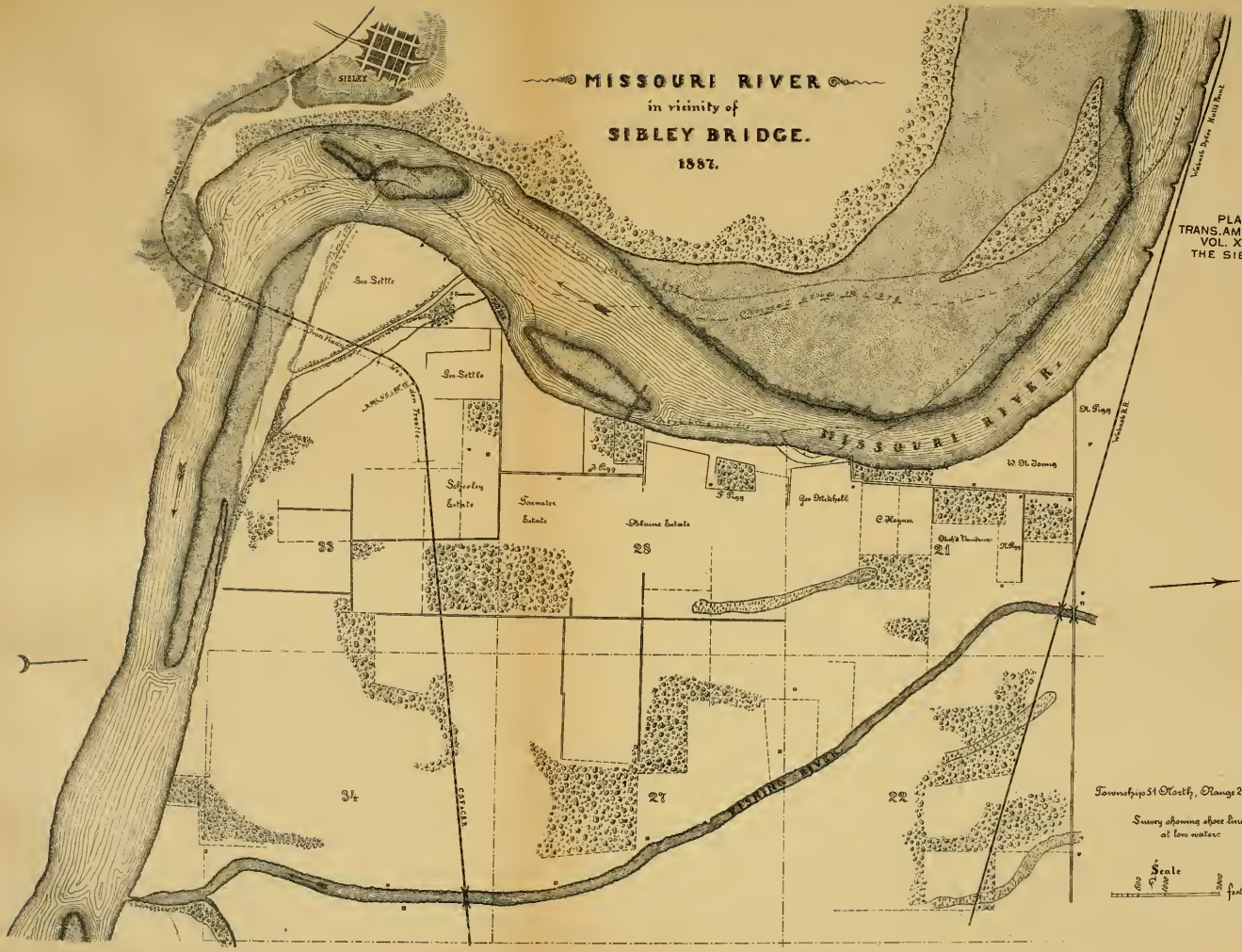


PLATE XXXVI.
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXI No 422.
THE SIBLEY BRIDGE.

MISSOURI RIVER
— IN VICINITY OF —
SIBLEY BRIDGE
SURVEYS OF 1887 AND 1888
Scale,
Note:- Full lines denote Shore of 1888 Water 15 ft. above L.W.
Broken " " " Jan. 87 " 5 ft. " " "
Dotted " " " Aug. 87 " " " "
PLATE XXXVII
TRANS. AM. SOC. CIV. ENGRS.
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THE SIBLEY BRIDGE
OVERFLOWED AT ORDINARY HIGH WATER
ORDINARY HIGH WATER BANK
BANK LINE 25' ABOVE L.W.
MISSOURI
RIVER
SHORE LINE 15' ABOVE L.W.
BLUFF BANK 23' HIGH
DIKE 1888
DIKE
Sibley Bridge
Viaduct
OLD SLOUGH
LOW WATER LINE OF 1887
HIGH WATER BANK 23' ABOVE L.W.
C. S. Pa. & C. R. R.
LINE OF BAR AUG. 1887
LINE OF BAR JAN. 1887
SAND BAR AUG. 1887
SHORE LINE JULY 1888
AREA FORMED DURING HIGH WATER 1888
HIGH BANK 23' ABOVE L.W. & SHORE LINE 1887
SHORE LINE JULY 1888
SAND BAR LINE AUG. 1887
SHORE LINE OF JAN. 1887
SAND BAR AUG. 1887
SHORE LINE OF JAN. 1887
SAND BAR AUG. 1887
SHORE LINE OF JAN. 1887
SHORE LINE AUG. 1887
AREA CUT AWAY DURING HIGH WATER 1887
SHORE LINE OF AUG. 1887
SHORE LINE OF JUNE 1888
Approximate Location of Material Track
Wabash Western Ry.

General Profile.

Scale 0 100 200 ft

PLATE XXXVIII.
TRANS. AM. SOC. CIV. ENGRS.
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THE SIBLEY BRIDGE.

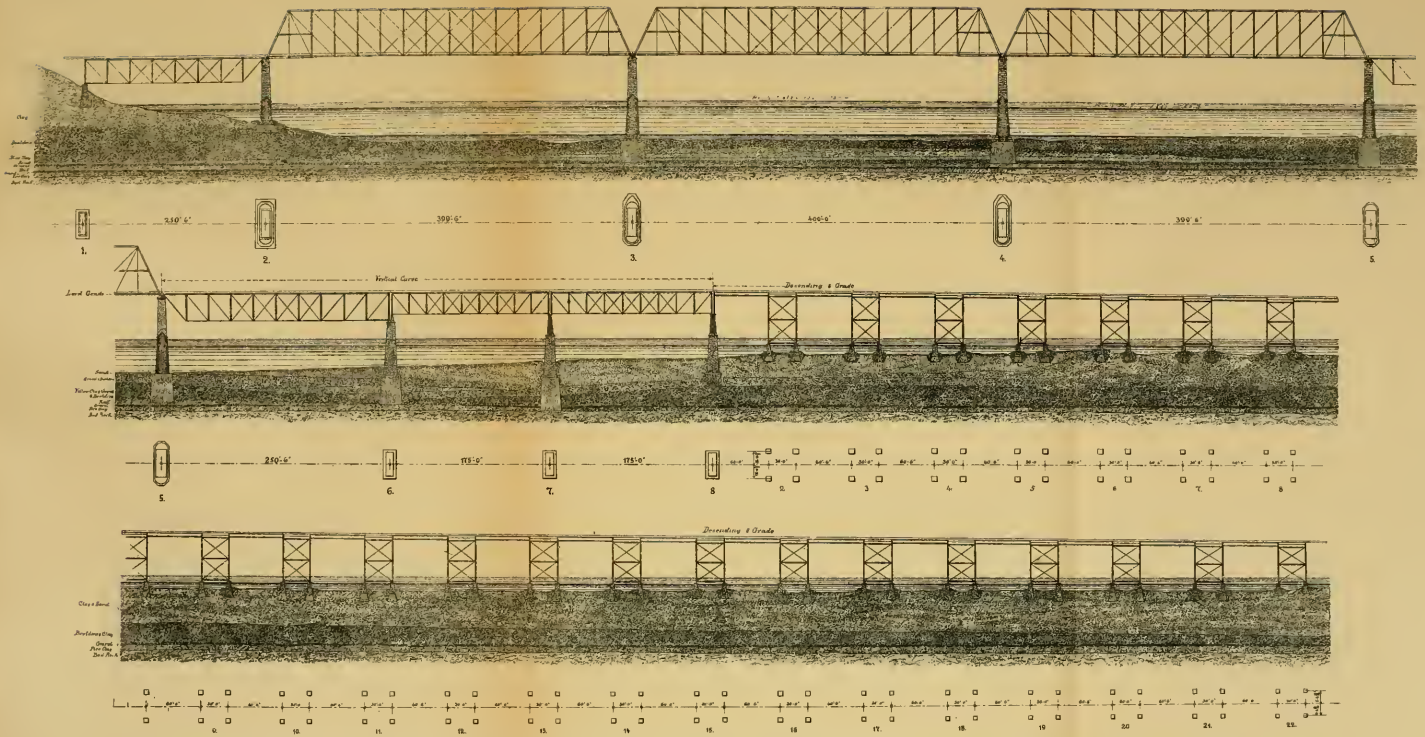
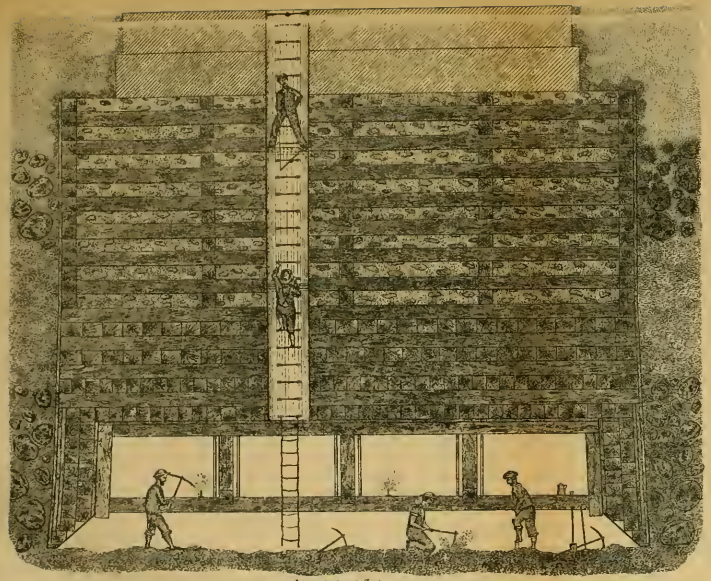
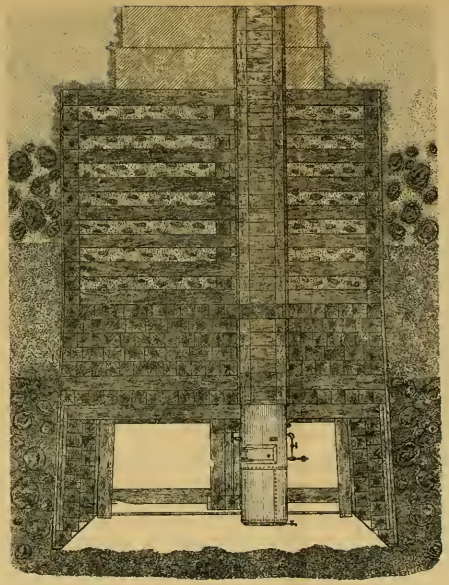


DIAGRAM SHOWING RATE OF PROGRESS IN SINKING CAISSONS

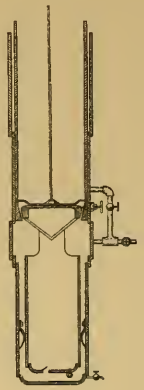
General Plan of Caisson.

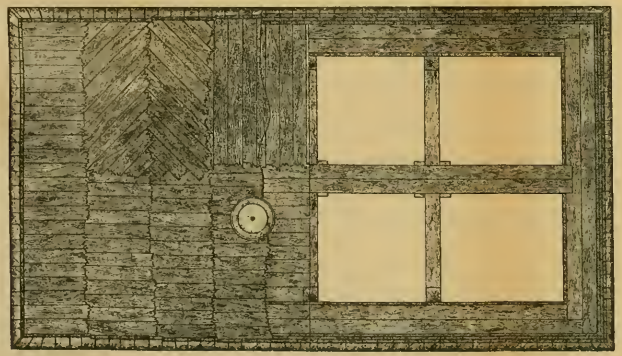


Longitudinal Section.



Cross Section.





Scale
2 4 6 8 10 ft.

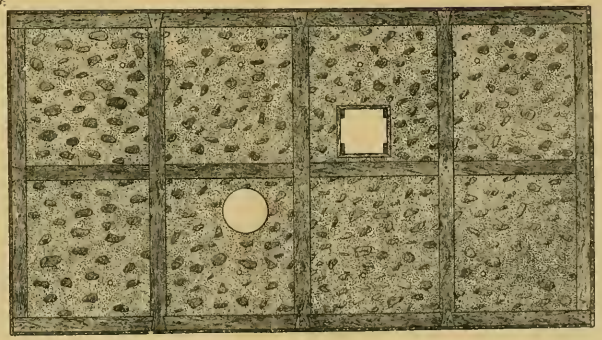


PLATE XLI
TRANS. AM. SOC. CIV. ENGRS.
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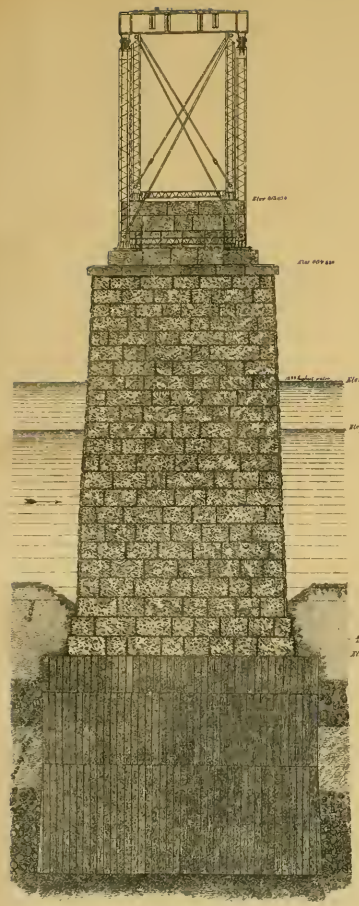
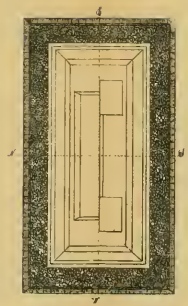
THE SIBLEY BRIDGE.

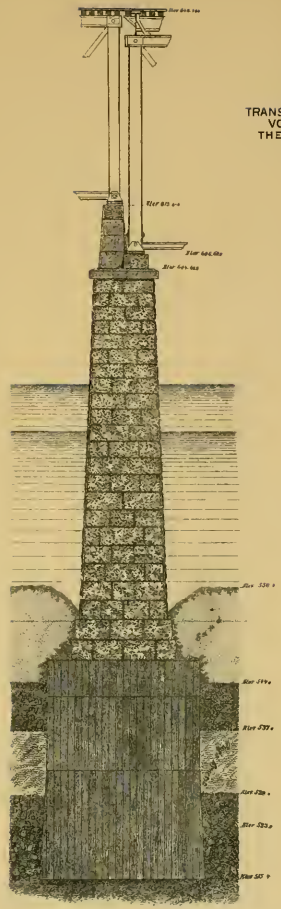
PIER Nº 6



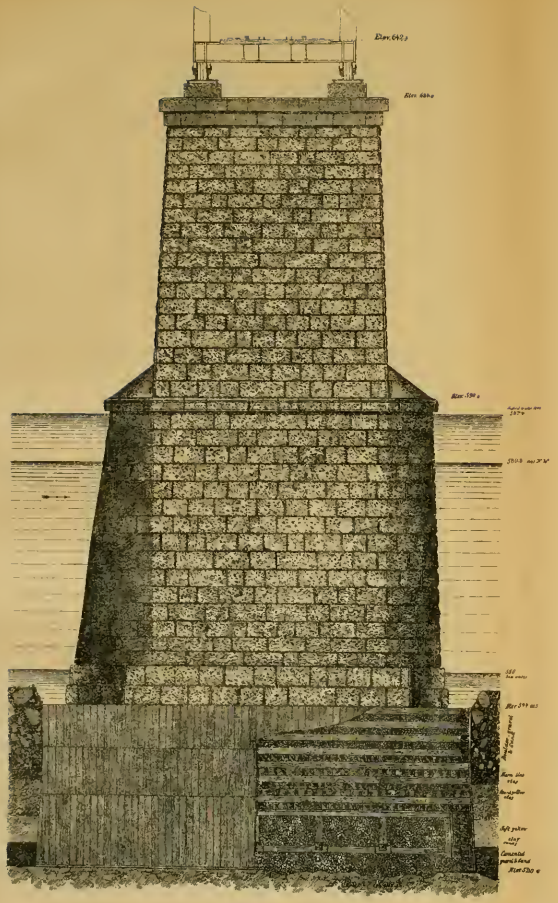
ELEVATION
(South side)



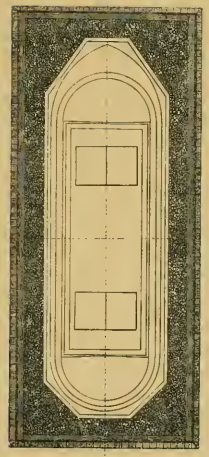
PLAN



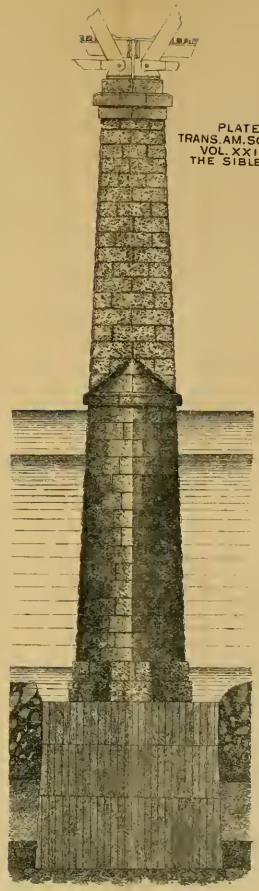
ELEVATION
(West side)



ELEVATION
(South side)



PLAN



ELEVATION,
West side

PLATE XLIII.
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXI Nº 422.
THE SIBLEY BRIDGE.

Scale

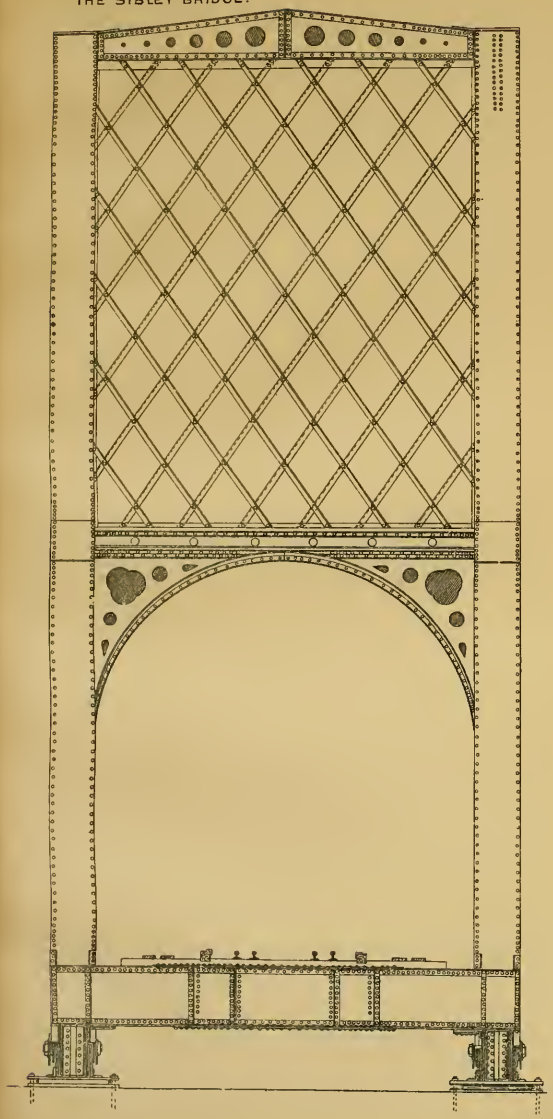


PLATE XLIV.
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXI Nº 422.
THE SIBLEY BRIDGE.

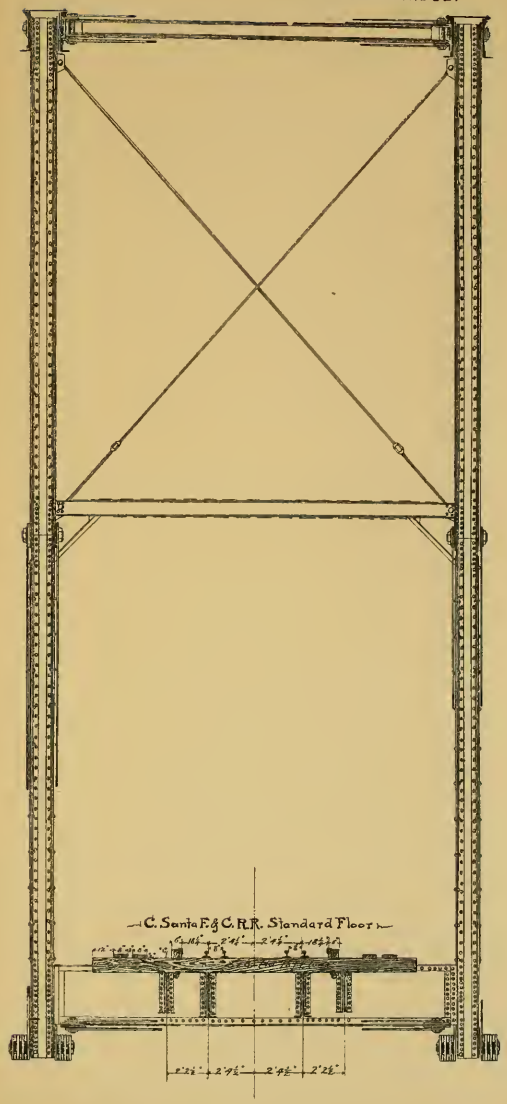


PLATE XLV
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXI No 422.
THE SIBLEY BRIDGE.

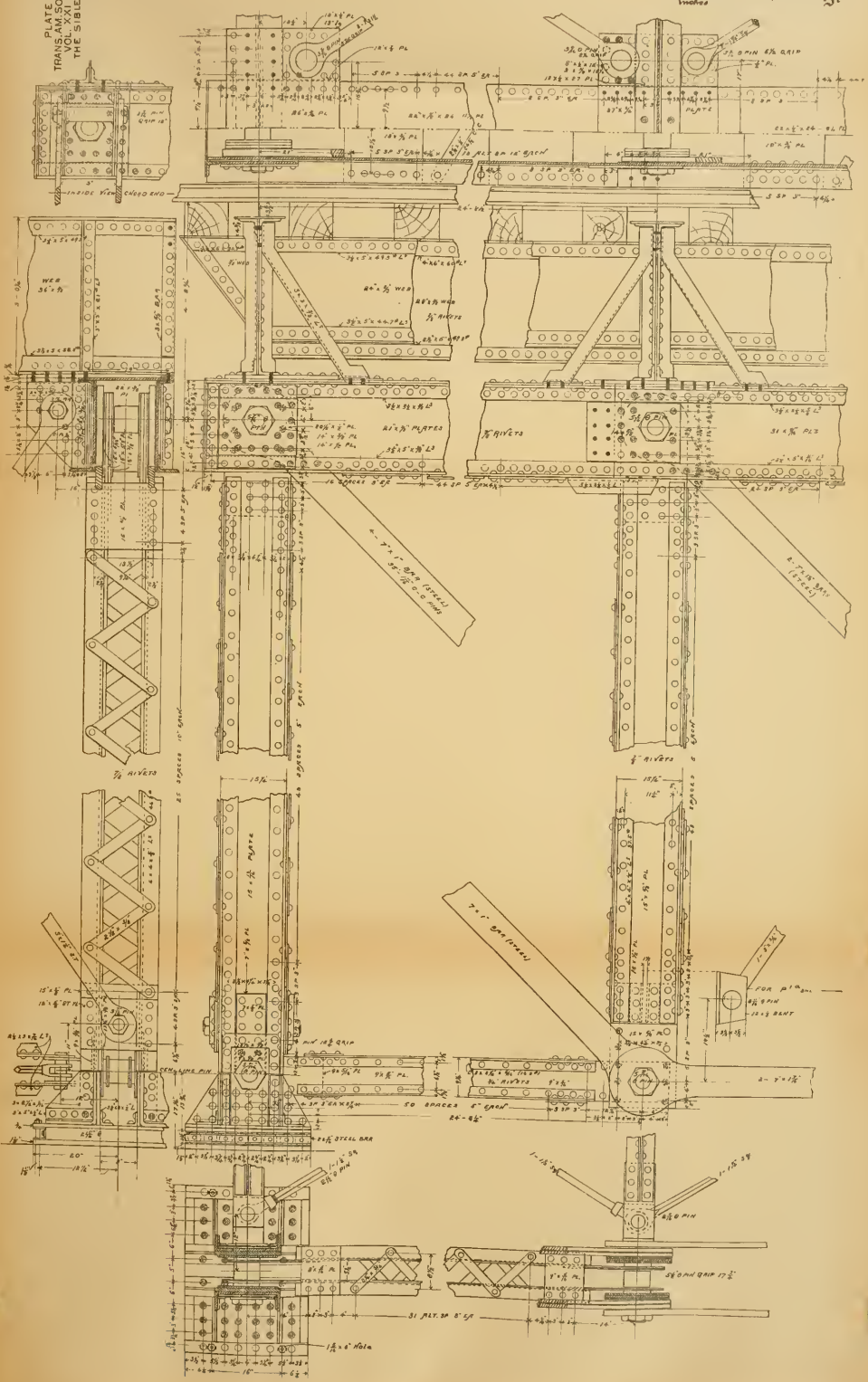


PLATE XLVI
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXI No. 422.
THE SIBLEY BRIDGE.

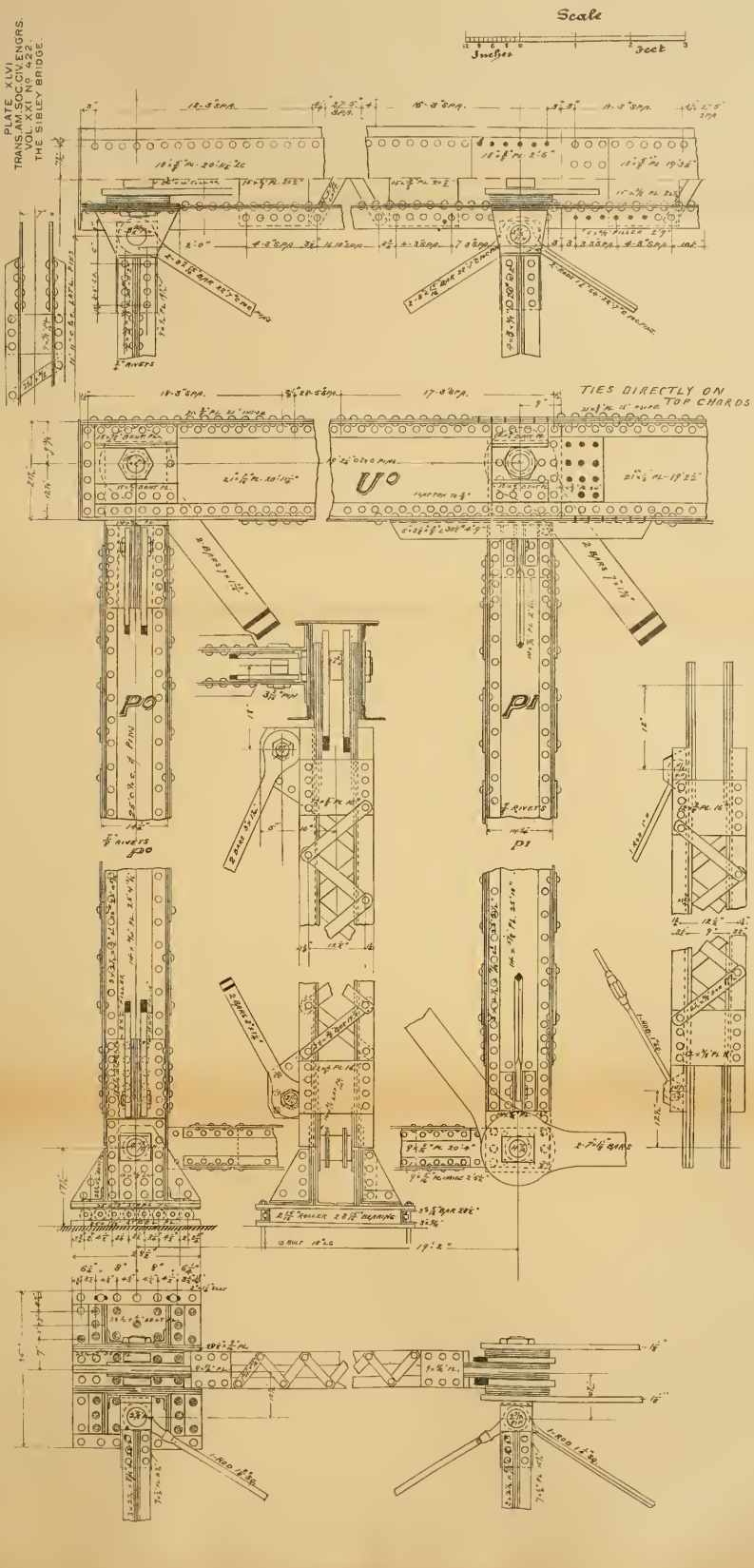


PLATE XLVII
TRANS. AM. SOC. CIV. ENG'RS.
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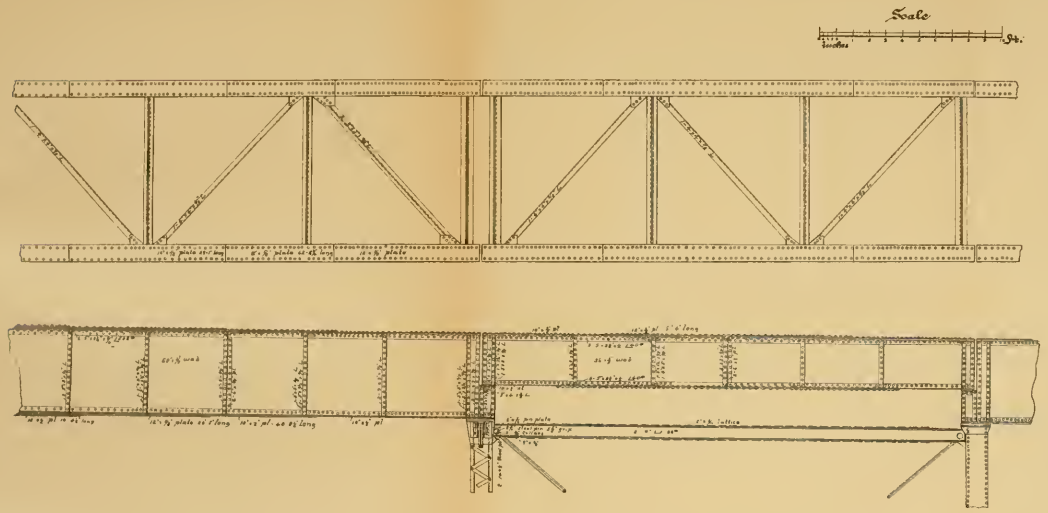
THE SIBLEY BRIDGE



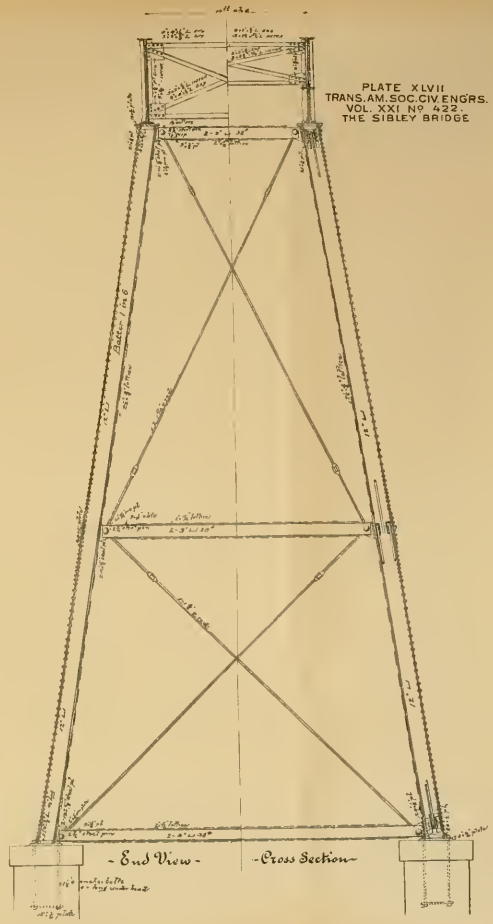
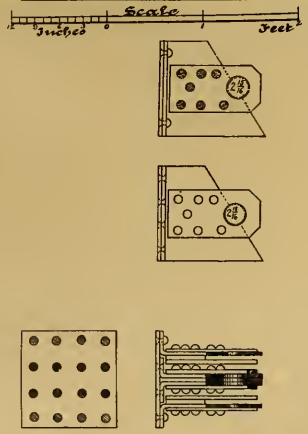


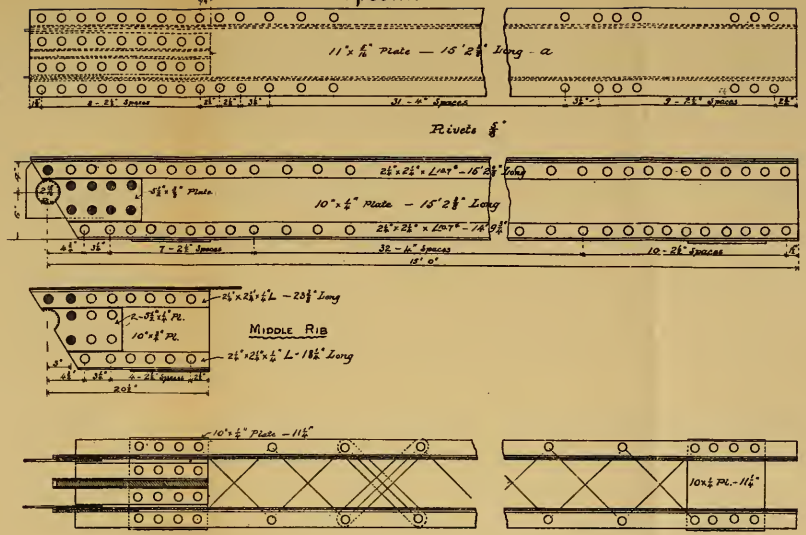
PLATE XLVIII
TRANS. AM. SOC. CIV. ENGRS.
VOL. XXI Nº 422.
THE SIBLEY BRIDGE.

Steel Compression Test Specimens

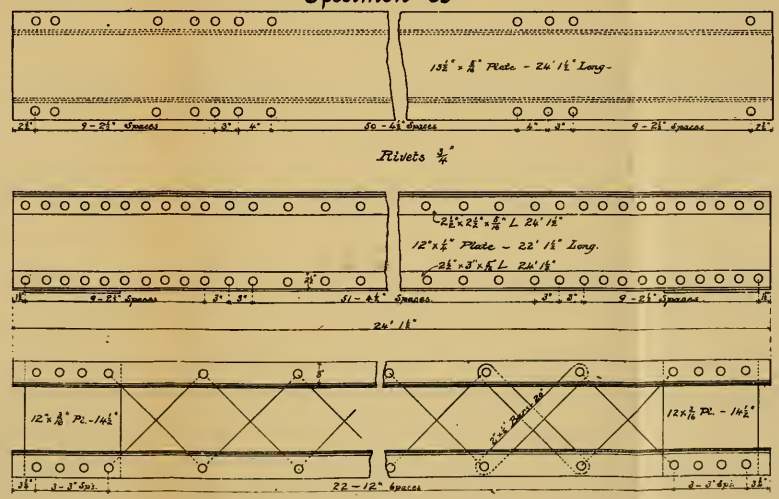
Approximate fac-simile Top Chord Sections Reduced



Specimen A



Specimen B



Unit Strains

A

$$P = \frac{10000}{1 + \frac{L^2}{30000\,R^2}} \left(\text{or } \frac{\text{min. stress in member}}{\text{Ex. max. stress to member}}\right) \qquad R^2 = 15.04$$

P = 11718 per sq. in.
Ult. str. per □" developed by Test = $\frac{58400}{12.68}$ = 46056

B

$$P = \frac{10000}{1 + \frac{L^2}{40000\,R^2}} \left(\text{or } \frac{\text{min. stress to member}}{\text{Ex. max. stress to member}}\right) \qquad R^2 = 22.59$$

P = 11484 per sq. in.
Ult. str. per □" developed by Test = $\frac{70800}{16.42}$ = 43238

Specimen Tests on Material in both Posts.

	Ult. str.	E.B.	% El. in 8"	% Red. of Area
Angles	76750	45860	22.6	47.1
Plates	81740	50370	22.5	33.62

N. W. Bridgeman
Engr. Supt.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

423.

(Vol. XXI.—September, 1889.)

CYLINDRICAL WHEELS AND FLAT TOPPED RAILS FOR RAILWAYS.

By D. J. WHITTEMORE, Past President Am. Soc. C. E.

"Fool," said my Muse to me, "look into thy heart and write."
—*Sir Philip Sidney.*

The Scrap Heap—that inarticulate witness of our blunders, and the sepulcher of our blasted hopes; the best, but most humiliating legacy we are forced to leave to our successors—has always, to me, been brim-full of instruction.—THE AUTHOR.

Present practice imposes from two to three times the weight on railway wheels that was customary a few years ago. This taxes not only the permanent way rolling-stock and engine-power to the utmost, but also the genius, and, I may say, the courage of the engineer, in his endeavor to secure safety under the most trying conditions.

Economy of train service has become so important, that it is safe to say there will be no return to lighter loads unless it shall be thoroughly demonstrated that both safety and economy require it. Instead of this being the result, I apprehend that still further increased capacity will be demanded, until the loads carried will be limited on our standard gauge only by the size of cars that can pass over the line with sta-

bility and at such high velocity as may hereafter be demonstrated as practicable.

Car wheels and rails to the value of nearly a billion dollars pass into the scrap heap every few years on this Continent alone. It is my belief that they would give a much longer service if they were designed and constructed according to established and well known laws of mechanics. In this we have been too long the slaves of precedent. In making this broad statement I do not wish to imply that through forty years of professional service I have been without guilt in the matter of which this paper treats; or that the persistence in errors is confined to American engineers; for the particular errors to which I allude probably exist in a higher degree outside our own country.

If it is true that the tendency is to heavier loads, and that the scrap heap claims wheels and rails which should be capable of longer service —and I think there is no one who will dispute it—then there is hardly any one problem that deserves the earnest consideration of our members and of this Society more than the question of the relation of wheels and rails to each other. The disastrous effects produced by the monstrous loads per wheel must surely impress us with the necessity of an early solution of the problem they create. It would be interesting if we could determine what should be the tonnage service of rails. The report of the Committee on the relation of wheel and rail to each other* indicates that this is approximately 10 000 000 tons† to $\frac{84}{100}$ pound per yard of tangent rail abraded. If the section of rail referred to approximates the one shown in the report, this means that the rail should be worn down about one millimeter. Couard, of France, estimates that one millimeter of wear corresponds to 16 800 000 tons, and that a rail can be made that can be worn down ten millimeters. Lanino calculates the life service at 150 000 000; and Funk, of Germany, 125 000 000; and the latter also states that a good rail may be worn down over one-half inch.

Many of our rails go into the scrap heap with only $\frac{1}{16}$ of an inch abraded. Undoubtedly this is partly due to poor material in many instances, but in the large sense, I contend, it is due to faulty design of rail.

---

* Preliminary Report of the Committee on the Proper Relation to each other of the Sections of Railway Wheels and Rails. Transactions, Vol. XIX, July, 1888.

† The ton referred to in this paper is 2 000 pounds.

It is equally interesting and pertinent to inquire into the wear of wheels reduced to a tonnage basis. Of the ordinary cast iron car wheel we have an abundance of statistical information as to its mileage ; but I have not at hand the requisite data to permit me to determine the ton service with that approximate degree of accuracy desired. Of engine drivers I gather something from an interesting paper read before the Institution of Civil Engineers a few years ago by William Stroudly, of the London, Brighton and South Coast Railway of England—that tires of locomotive drivers, 78 inches in diameter, carrying a weight of 7.84 net tons, wear down $\frac{1}{8}$ of an inch for each 28 000 miles of travel. This would indicate that the service of this $\frac{1}{8}$ inch was 56 770 000 tons, or for a wear of one millimeter, 18 000 000 tons.

Through the kindness of our Member, Mr. J. N. Barr, Superintendent of Motive Power of the Chicago, Milwaukee and St. Paul Railway Company, I am able to present the following table, on page 136, showing tire wear of forty locomotives that had made an aggregate of over 6 000 000 miles—equivalent to turning one wheel 27 000 000 miles, and carrying a weight of from 14 000 to 16 000 pounds ; from which we deduce the result that a tonnage of 41 000 000 causes a wear of $\frac{1}{8}$ of an inch in the tread of the tire.

Through the courtesy of Mr. G. W. Rhodes, Superintendent of Motive Power of the Chicago, Burlington and Quincy Railway, I give tire wear of several standard locomotives in use on his line, in which there is shown a great difference in service in the use of tires of different thickness, and in both cases much lower service than is shown by Mr. Barr's report :

TABLE No. 1.

WEAR OF TIRE, CHICAGO, BURLINGTON AND QUINCY RAILWAY.

CLASS OF ENGINE.	Size.	Number of Drivers.	Total Weight on Drivers.	Miles run to $\frac{1}{16}$ Wear, 3-inch Tire.	Miles run to $\frac{1}{16}$ Wear, 4-inch Tire.
Switch engine, no truck.....	15 x 22–44	4	50 150	6 045	3 711
Switch engine, no truck.....	16 x 22–44	4	59 238	4 675	3 387
Road engine, forward truck.....	16 x 24–53	4	44 367	7 168	4 937
Road engine, forward truck.....	17 x 24–57	4	48 960	8 903	7 119

Reduced to ton service for $\frac{1}{8}$-inch wear of tire, the approximate average is :

For 3-inch tire.....31 380 000
And for 4-inch tire.....20 674 000

TABLE No. 2.

Wear of Driving Wheel Tire, Chicago, Milwaukee and St. Paul Railway.

Class of Engine.	Average diameter of tires.	Number of driving wheels.	Total weight on drivers in pounds.	Number of sets.	Number of runs.	Total Wear in 32ds of an inch.	Total miles run.	Average miles run to ⅛ inch tire wear.	Tons service to each ⅛ inch of tire wear.	Tons service for one millimeter of tire wear.
Freight.										
17-inch Rhode Island	62 inch.	4	55 450	5	19	193	995 555	20 633	46 512 591	
17 " "	62 "	4	55 450	5	12	173	699 040	16 163	36 435 149	
17 " "	62 "	4	55 450	5	17	235	893 320	15 205	34 276 470	
Passenger.										
18-inch Rhode Island	62 "	4	58 500	5	19	201	978 506	19 473	46 303 785	
18 " "	62 "	4	58 500	5	14	159	744 453	18 728	44 533 699	
Ten-wheel freight	62 "	6	81 000	10	23	334	1 458 332	17 465	38 348 471	
Standard 18-inch passenger	68 "	4	64 400	5	5	49	373 319	30 475	72 758 595	
								Average, excluding last....	41 042 414	12 935 769

The only explanation I can offer for the superior service shown by the 3-inch tire over that of the 4-inch is, that the material in the last must have been inferior in quality.

It will be seen by this table that the tire of the larger diameter gives the best service, as should be expected.

As you are aware, drivers are turned in sets to suit the greatest wear of any one wheel of its set, or rather to the spot of greatest wear of the most worn wheel ; hence this exhibit does not, even approximately, indicate the loss of metal due to abrasion ; but I give the information for what it is worth.

Making any reasonable allowance for this loss of metal by turning, let us see what is the tonnage service of steel-tired 33-inch car wheels under passenger cars of known weight and load, and where the only unknown element of wear enters to modify our conclusions, to wit, wear of brake shoes ; and again I am indebted to Mr. Barr for this information :

Coach No. 235, Chicago, Milwaukee and St. Paul Railway, weighing 61 500 pounds; passenger load, 4 500 pounds; total, 66 000 pounds; load per 33-inch wheel, 5 500 pounds—ran 95 110 miles, and suffered an average loss of $\frac{762}{1000}$ of one inch in circumference of wheel. Having the wear as measured on circumference of wheel, the weight in pounds carried by same, and the miles run, the following formula will give closely the tonnage service for each $\frac{1}{8}$ inch tread of 33-inch wheel worn down:

$$\frac{6 \text{ x weight on wheel in pounds x miles run}}{25 \text{ x circumference wear in inches}} = \text{Tons service}$$

for $\frac{1}{8}$-inch direct wear in thickness of tire.

In the case above cited the tonnage service for $\frac{1}{8}$ inch wear was:

$$\frac{6 \text{ x } 5\,500 \text{ x } 95\,110}{25 \text{ x } .762} = 164\,757\,428.$$

In further testimony, I offer the following table of average circumference wear of 33-inch steel tired car wheels of the passenger equipment of the Chicago, Milwaukee and St. Paul Railway, given by Mr. Barr, from which I deduce from the formula stated the tonnage service for wear down of tread for $\frac{1}{8}$ of an inch as shown. The average circumference wear is that of two wheels on the same axle:

TABLE No. 3.

Wear of Steel Tired Wheels, 33-inch Diameter, Chicago, Milwaukee and St. Paul Railway.

Weight of Car with Average load.	No. of Wheels to each.	Weight per Wheel. Pounds.	Miles Run.	Wear in inches, measured on Circumference.	Tons Service to $\frac{1}{8}$-inch wear down of Tread.
65 800	12	5 483	63 702	.600	139 711 226
67 700	12	5 642	77 534	.400	252 468 096
64 000	12	5 333	17 029	.150	145 305 051
80 400	12	6 700	59 493	.700	136 663 920
73 600	12	6 133	36 067	.250	212 350 954
77 500	12	6 458	41 583	.500	128 900 646
54 100	8	6 762	41 449	.625	107 661 256
59 500	8	7 438	80 444	.700	205 145 990
54 100	8	6 762	36 473	.500	118 382 604
64 200	12	5 350	64 872	.650	128 147 150
81 300	12	7 608	30 683	.375	149 399 208
81 300	12	7 608	76 470	1.275	109 543 923
47 000	8	5 875	72 795	.600	171 068 250
63 200	12	5 267	95 446	1.100	109 683 072
79 600	12	6 633	51 259	.550	148 364 049
64 000	12	5 333	47 132	.450	134 055 976
78 100	12	6 508	25 623	.200	200 105 380
59 800	12	4 893	69 653	.800	102 263 638
79 600	12	6 633	55 660	.350	253 160 763
77 900	12	6 492	54 083	.775	108 729 858
50 500	8	6 325	47 997	.700	104 084 922
65 200	12	5 433	60 884	.500	158 775 730
65 700	12	5 475	68 591	.850	106 080 675
				Average.....	149 132 710

From the same source I gather the following data in reference to the performance of 42-inch steel tired wheels with cast iron hubs, and reducing to tonnage service for direct wear of $\frac{1}{8}$ inch the formula,

$$\frac{\text{Mileage x weight in pounds on wheel}}{5\frac{3}{16}\text{ x circumference wear in inches}} = \text{Tons service,}$$

is used.*

Weight of Car and Average Load.	Number of Wheels.	Weight per Wheel.	Miles Run.	Wear in inches, Circumference Measurement.	Tons to $\frac{1}{8}$ inch Wear.
82 000.....	12	6 833	105 439	.900	151 061 524
82 000.....	12	6 833	93 503	.600	204 027 012
				Average.....	177 544 268

*Since this paper was dictated the author has devised the following convenient formula for determining tons service of wheels for a wear down of tread of $\frac{1}{8}$ of an inch, applicable to wheels of any diameter, in which formula

a = reduced circumference of wheel through wear in inches.

b = miles run to produce such wear.

c = weight in pounds on wheel.

d = diameter of wheel in inches while in service.

$$\frac{7.92\ b.\ c.}{a.\ d.} = \text{Tons service.}$$

Through the kindness of our Member, Theodore N. Ely, General Superintendent of Motive Power of the Pennsylvania Railway Company, the following table, showing the weights per wheel carried on that line, is compiled. The freight tariff of that company, however, permits 64 000 pounds paying load, where 60 000 is stated in this table.

TABLE No. 4.

Passenger Equipment Cars.

Kind of Car.	Weight. Pounds.	Load. Pounds.	No. of Wheels.	Weight on each Wheel.*
Passenger	50 000	7 830	8	6 902 pounds.
Baggage	34 700	16 000	8	6 011 "
Express	32 500	14 000	8	5 486 "
Postal	58 000	20 000	12	6 174 "
Combination	39 500	16 000	8	6 611 "
Emigrant	36 500	7 830	8	5 867 "

Weight of passenger car axle..... 326 pounds.
" " " wheel..... 540 "

Freight Equipment Cars.

Kind of Car.	Weight. Pounds.	Load. Pounds.	No. of Wheels.	Weight on each Wheel.†
Box car "Xc."	30 100	60 000	8	10 682 pounds.
Stock car "Kd."	27 800	60 000	8	10 435 "
Coal car "Gd."	23 500	60 000	8	9 797 "

Weight of freight car axle..... 400 pounds.
Weight of freight car wheel..... 540 "

Locomotives.

Class.	Diameter of Driving Wheels.	Diameter of Truck Wheels.	Weight on Drivers.	Weight on Truck.	Weight on each Driver.	Weight on each Truck Wheel.‡
A Passenger	68 inches.	33 inches.	64 000	29 500	16 000	7 375
N "	62 "	33 "	57 700	33 600	14 425	8 400
O "	62 "	33 "	58 300	33 600	14 575	8 400
P "	68 "	33 "	67 800	32 800	16 950	8 200
R Freight	50 "	28 "	100 600	14 025	12 575	7 012
K Express passenger	78 "	33 "	64 900	31 800	16 225	7 950

*Weight on each wheel is exclusive of weight of wheel.

†Weight on each wheel is exclusive of weight of wheel. Weight of box and stock cars includes air brakes fixtures.

‡Weight on each wheel includes weight of wheel.

TENDERS.

KIND.	Weight. Pounds.	Load. Pounds.	No. of Wheels.	Weight on each Wheel.*
Passenger	29 300	35 250	8	7 528 pounds.
Freight	29 000	40 300	8	8 127 "

When we make allowance for wear due to brake shoes and the flow of metal, the tonnage service for abrasion due to rail contact will be largely increased. How it is that there is such disparity between engine drivers and passenger wheel tonnage service will appear, if my premises are correct, further on.

In fact, there are so many varying and conflicting conditions, and so much data wanting, such as loss by corrosion, etc., that it seems about as impossible to determine a fair expression for abrasion due to rolling load alone, as to have the Missouri River act according to mathematical formulas. I submit that the facts presented indicate pretty clearly that failure does not result so much from abrasion as from some other cause.

To secure increased service of wheels and rails, the remedy proposed by the author is indicated by the caption of this paper, to wit, "Cylindrical Wheels on Flat Topped Rails." I do not wish it understood that I mean that the entire width of rail top should be flat, or that if made flat it could continue so for any great length of time. The difference in gauge between wheel flanges and rails permits a lateral movement of, say, $\frac{5}{8}$ of an inch, or more. Owing to difference of wear by reason of wheels running to or from flange, a curve characteristic of this wear is produced, and which will extend along the line of bearing from the flange contact, a distance equal to at least one-half of this lateral movement of wheels on rails, making the origin of the curve about $\frac{3}{8}$ of an inch from the side of the rail head. This will hold true also for both top corners of the rail. The balance of the rail top, I contend, should be flat.

When the subject of rails was under discussion at our last annual convention, our Past President, J. B. Francis, gave us the key to the solution of the problem, and this was, that the material composing wheels and rails should not be subjected to pressures beyond their elastic limit. It is now over a dozen years since, in an evening's discus-

*Weight on each wheel is exclusive of weight of wheel.

sion with the late lamented C. Shaler Smith, M. Am. Soc. C. E., we had under consideration the subject of elasticity. The result of my calculations was, that driving wheels of locomotives, as then loaded, must have a diameter of about 17 feet, to keep the contact between it and the rail within the elastic limit. Calculations since made convince me that this was an under-estimate.

It is hardly necessary for me to direct your attention to the following sketch as illustrating our practice, as shown in Fig. 1, Plate XLIX. The author's proposed arrangement is shown in Fig. 2, without, however, endorsing the section of rail shown below the line, *A B*.

As children we listened with interest to the story of the genius shown in the invention of the coned wheel for railway purposes; and were told that without it curved lines could not be operated, etc. What are the facts? A new wheel, constructed in accordance with the rules of the Master Car Builders' Association, is coned to suit a curve of about 5 000 feet radius, and no other; and after six months' use, as shown by the preliminary report of your Committee, such wheels are either worn hollow at the flange or from the flange; so much so, that in at least one-third the instances, in passing curves, the inner wheel travels on its greatest diameter, and quite often the outer wheel travels on its least diameter—yet such wheels are operated safely. The German investigator Krüger (see Vol. 86, Inst. C. E.) proves by analyses what is apparent to all of us, that rolling friction is largely augmented through this device. I understand that the Association of Master Car Builders, at its meeting some two years ago, decided by a majority of three of the members in attendance at its convention to continue this practice; but that a large majority of those eminently practical men who were not in attendance were vigorously opposed to it.

The form of rail shown in Fig. 1, Plate XLIX, is that lately recommended by Sandberg for 100-pound rail, and which has a curved top of 6 inches radius, and from the sketch it is seen that, in so far as the elasticity wheel and rail will permit, it is about the best arrangement that can be devised to concentrate the stress on a point and have the travel of the wheel on a line. These are conditions that are revolting in a mechanical light—conditions we would not attempt to reproduce in almost any other mechanical device. While this condition prevails with new car wheels, engine drivers are generally turned cylindrical, and the work done by them and the car wheel tends to drive down this arch of the rail as a

wedge, and by flow of metal to make it flatter, at the same time producing characteristic curves of wear on the wheels, often resulting in shattering and piping the rail heads, with but little loss of material from direct wear. This flattening of the rail from an original radius of 10 or 12 inches to over 14 inches is well illustrated in the report of your Committee.

Mr. Octave Chanute, M. Am. Soc. C. E., many years ago gave us the results of his measurements of the area of contact between wheels and rails, and the writer quite recently made measurements in a similar manner, the results of which it may be proper to reproduce here. However crude the manner of taking the impressions may have been, I know of no other convenient method of securing them.

My first test was with an engine having four driving wheels, each carrying 16 000 pounds; diameter of wheels, 70 inches; tires much worn; and on a steel rail five years in constant service. A composite picture of all these apparent areas of contact shows an egg-shaped oval, having a major axis across the rail of $1\frac{48}{100}$ inches, and a minor axis along the rail of 1 inch, inclosing an area of $1\frac{7}{100}$ square inches.

My second test, with an engine having six driving wheels of 62 inches diameter, carrying 13 800 pounds, with tires that had been in service six months, gave a similar figure of contact, with a major axis of 1.27 inches and a minor axis of $\frac{79}{100}$ of an inch, inclosing an area of $\frac{86}{100}$ of a square inch. Through such contacts is transmitted the power exerted by our locomotives, not only in their direct compressive force, but also in their pulling capacity of, say, 4 000 pounds on the lamina of the rail. Is this the one spot in our earthly domain to which we cannot apply reason? The ingenuity of man has been able to formulate the approximate horse-power of the earthquake, finding the factors in shattered towers and dismantled homes; must that ingenuity stand appalled before this point of contact between wheel and rail? If so, we should exclaim with more effectiveness than did *Lady Macbeth*, "Out, damned spot!"

It is on this spot where, if the statement made in a paper now before this Society is construed literally, occurs that miraculous chemical action, "the molecular interlocking of the fibres" of a non-fibrous substance; but I suppose we all know the meaning intended by this sentence.

To me, and doubtless to many other members outside of some of your Committee, the solution of this problem rests in having a flat and

PLATE XLIX.
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WHITTEMORE ON
RAILWAY WHEELS AND RAILS

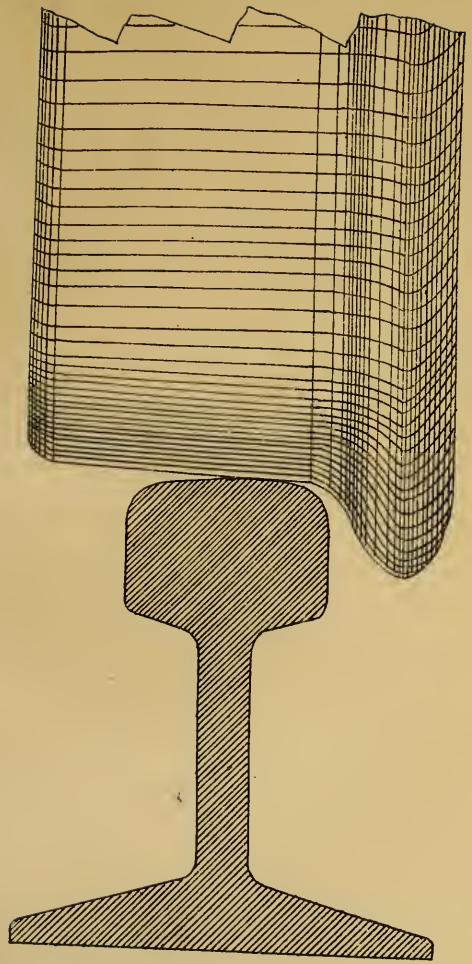


Fig. 1
Showing Present Practice

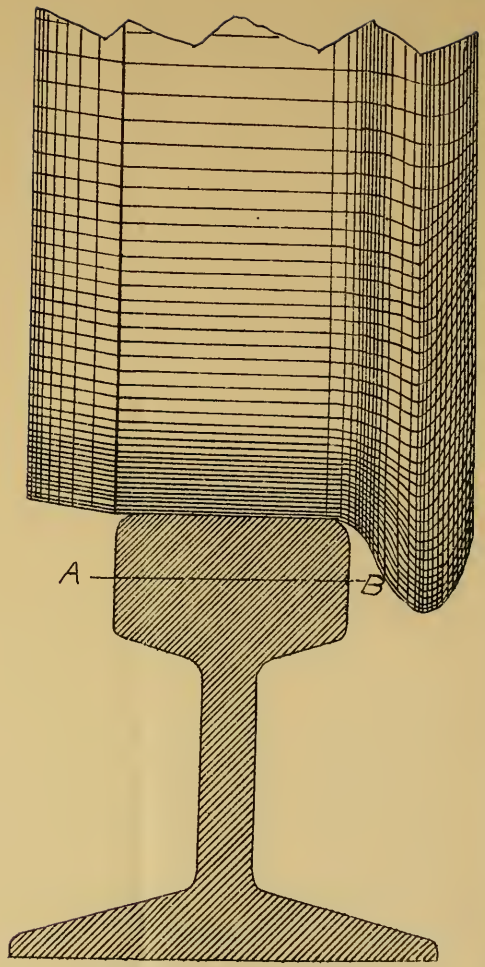


Fig. 2
Showing Suggested Arrangement
Above A. B.

sufficiently broad surface to the rail to support not conical but cylindrical wheels, to keep the pressures within the elastic limit of the metal. Right here we are met by the statement that it is not clear that "increased bearing surface is in itself an advantage;" and when we attempt anything like an analytical inquiry, we are met by *ex cathedra* statements "that to such bearing surfaces the ordinary compression moduli of the text books have no application whatever. * * * To apply these constants to the bearing surfaces of spheres or cylinders laid on their sides, or to draw conclusion that pressures exceeding them are therefore dangerous or destructive, is entirely unwarranted. No attempt has ever been made, nor can there well be, to determine constants for such round bodies, etc., and in good bridge practice it is considered entirely safe to load steel rollers rolling on steel up to the equivalent of 10 000 pounds per lineal inch for a 33-inch wheel."

I heartily wish that all abstruse questions in engineering were as well settled in the minds of engineers as these appear to be in the minds of some of your Committee. Ordinarily it is time wasted to attempt an argument before a jury that has already rendered its verdict on the important questions at issue; and I trust that this Committee will pardon me if I shall overcome my natural modesty, and appeal to those of my readers whose minds are open to conviction, to exercise patience while I call their attention to the labors, text book, and formulas of one more astute than the celebrated character in "Hudibras":

> "Who could distinguish and divide
> A hair twixt south and south-west side,
> And wisely tell what hour o' the day
> The clock does strike, by Algebra."

I mean Professor Frederick Grashof,* of the Polytechnic School at Carlsruhe, one of whose formulas has been extensively used in this country for determining constants for rollers on plane surfaces in nearly every first-class bridge specification for many years; and heretofore referred to by our Member, Mr. Charles Macdonald, with telling effect, in his criticism of C. Shaler Smith's, M. Am. Soc. C. E., paper on drawbridges, published in our Transactions as long ago as 1874, and in the

* Theorie der Elasticitat und Festigkeit mit Bezug auf ihre Anwendungen in der Technik, von Dr. F. Grashof, Grossh. Badischer Geheimer Rath und Professor am Polytechinkum zu Carlsruhe.

same volume containing the report on rails made by our lamented Past President, Ashbel Welch, as Chairman, in which report is repeated six or eight times the necessity of securing greater width of contact between wheel and rail. In making use of these formulas in constructing the following table, the moduli was assumed by me at the values stated, after an examination of the tests made by our Member, Mr. Christie, and reported in our Transactions some years ago. It will be seen in the table of tests of steel in rails of the Chicago, Burlington and Quincy Railway, hereinafter given, that the modulus of resistance is somewhat lower than I have assumed in some instances, and much greater in others. I have utterly failed in my attempt to secure any data relating to the moduli of chilled cast-iron; hence you will consider the results of my calculations in this table as applying to steel wheels on steel rails. The table is not given as a play upon figures, but as an educator of the judgment. It has had that effect with me, and I trust it will not be without interest to others who have not given this subject their particular attention.

TABLE ON OPPOSITE PAGE, showing allowable pressures of wheels on rails with curved tops; also on rails with flat top surface.

Assuming different values of K and in which formulas

K = Elastic limit per square inch and such portion thereof as may be deemed safe; Designated $K_1 K_2 K_3 K_4$.

E = Modulus of elasticity of wheel.

E_1 = Modulus of elasticity of rail.

R = Radius of wheel.

R_1 = Radius of rail top.

P = Allowable pressure of wheel on rail. For rails with curved top the formula is

$$\pi \left(\frac{1}{E} + \frac{1}{E_1}\right) K^2 \sqrt{R R_1} = P.$$

For rails with flat top the formula is :

$$\sqrt[3]{\frac{32}{9} \left(\frac{1}{E} + \frac{1}{E_1}\right) K^3 R} = P = \text{pressure per lineal unit of tread.}$$

In this table the assumed modulus of wheel tread 29 000 000, and for the rail 30 000 000.

TABLE No. 5.

Radii of Wheel and head of rail.		Weight on wheels, according to recent practice.	Assuming the value of K as stated, the weight on wheels can be as follows:				
Wheel.	Rail.		$K = 40\,000$	$K_1 = 35\,000$	$K_2 = 30\,000$	$K_3 = 25\,000$	$K_4 = 20\,000$
5″	8″	From 12 000 to 16 000 pounds, and in some instances 20 000 pounds.	5 722	4 367	3 212	2 225	1 422
5″	10″		6 398	4 882	3 591	2 488	1 590
5″	12″		7 068	5 348	3 934	2 725	1 741
5″	14		7 570	5 777	4 250	2 944	1 881
35″	16″		8 093	6 176	4 543	3 147	2 011
35″	18″		8 584	6 515	4 819	3 338	2 133
30″	8″		5 298	4 043	2 974	2 060	1 316
30″	10″		5 923	4 520	3 325	2 303	1 472
30″	12″		6 489	4 952	3 643	2 523	1 612
30″	14″		7 008	5 348	3 934	2 725	1 741
30″	16″		7 492	5 718	4 206	2 913	1 862
30″	18″		7 947	6 065	4 461	3 090	1 975
27″	8″		5 026	3 835	2 821	1 954	1 249
27″	10″		5 619	4 288	3 154	2 185	1 396
27″	12″		6 156	4 698	3 456	2 394	1 530
27″	14″		6 649	5 074	3 732	2 585	1 652
27″	16″		7 108	5 424	3 990	2 764	1 766
27″	18″		7 539	5 753	4 232	2 931	1 873
24″	8″	Sleeping and dining cars, 6 000 to 6 500 pounds.	4 738	3 616	2 660	1 842	1 177
24″	10″		5 298	4 043	2 974	2 060	1 316
24″	12″		5 804	4 429	3 258	2 257	1 442
24″	14″		6 268	4 784	3 519	2 437	1 558
24″	16″		6 701	5 114	3 762	2 606	1 665
24″	18″		7 108	5 424	3 990	2 764	1 766
21″	8″		4 432	3 382	2 488	1 723	1 101
21″	10″		4 955	3 782	2 782	1 927	1 231
21″	12″		5 429	4 143	3 048	2 111	1 349
21″	14″		5 863	4 475	3 292	2 280	1 457
21″	16″		6 268	4 784	3 519	2 437	1 558
21″	18″		6 649	5 074	3 732	2 585	1 652
16½″	8″	Passenger and sleeping cars, 5 500 to 6 500 lbs. Freight cars from 8 000 to 11 500 lbs., and for rotary snow plows, 16 000 lbs.	3 929	2 998	2 205	1 528	976
16½″	10″		4 392	3 352	2 466	1 708	1 091
16½″	12″		4 812	3 672	2 701	1 871	1 196
16½″	14″		5 198	3 966	2 918	2 021	1 291
16½″	16″		5 556	4 240	3 119	2 160	1 381
16½″	18″		5 894	4 498	3 308	2 292	1 464
35″	Flat top		23 243	19 025	15 097	11 482	8 134
30″	"		21 519	17 614	14 086	10 630	7 530
27″	"		20 415	16 710	13 260	10 085	7 144
24″	"		19 248	15 755	12 502	9 508	6 736
21″	"		18 006	14 738	11 693	8 895	6 301
16½″	"		15 959	13 063	10 366	7 884	5585

These formulas for the values of K, K_1, K_2, K_3 and K_4 can be reduced to expressions as follows, viz.:

For Cylinder on Cylinder:	For Cylinder on Flat Track:
$K = 40\,000.$ $P = 342 \sqrt{R\ R_1}$	$P = 3929 \sqrt{R}$
$K_1 = 35\,000.$ $P = 261 \sqrt{R\ R_1}$	$P = 3216 \sqrt{R}$
$K_2 = 30\,000.$ $P = 192 \sqrt{R\ R_1}$	$P = 2552 \sqrt{R}$
$K_3 = 25\,000.$ $P = 133 \sqrt{R\ R_1}$	$P = 1941 \sqrt{R}$
$K_4 = 20\,000.$ $P = 85 \sqrt{R\ R_1}$	$P = 1375 \sqrt{R}$

The formula I wish to call your attention to first, is that which applies to cylinders in contact and having their axes transversely to each other—precisely the condition that prevails with wheels and rails. It may be claimed that the moduli assumed are not correct. My answer is, assume any other within reason, and then deduce results. It is true that these formulas apply to the cylinder or wheel, but in degree they apply with equal force to the rail; or, to put it in the language of a late editorial in the *Engineering News* on this question: "It is a fair presumption that whatever wears the rail most wears the wheel most."

No words of mine are needed to give weight to the deduction that can be drawn from this table. Of course it is nonsense to contemplate the use of driving wheels of 30 feet and over in diameter to keep within the elastic limit of rails having a rounded head of 12 inches radius. Note what results are given for wheels on flat surfaces. Please bear in mind that these figures apply only to static conditions of loads; what should be the practice when we add that indefinite quantity due to momentum or impact, imperfect wheels and track, and unbalanced locomotives at high speeds? Our wonder is not that rails fail so soon, but that they last so long!

It is my claim that this table at once suggests the remedy—which is, that without the necessity of increasing diameter of wheels, we should provide a contact between wheel and a flat surface of rail of from 2¼ to 2½ inches wide, making the total width of head, after providing for corner curves, from 3¼ to 3½ inches. In arriving at this conclusion I have taken for my factor of safety that given in the use of the value of K_3, although that derived from K_4 corresponds, in my opinion, more nearly with good bridge practice—the statement of some of your Committee to the contrary notwithstanding.

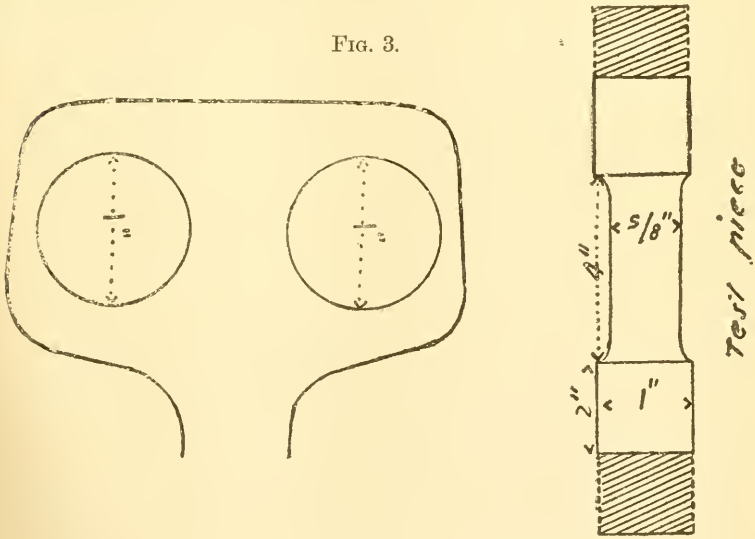
The merit I attach to these formulas rests not alone in their deter-

mination through rigid analytical and logical reasoning, but that they accord with common sense.

It is true, as reported by your Committee, that our rails fail with little material abraded from the top. The fact is, they are crushed, after flow of metal has reached its limit; they fail, like all other crystalline substances when overtaxed, by rapid disintegration and not abrasion, and it is the exception to find in the scrap heap a rail with the head well worn down.

If my premises are correct, that a wider bearing surface is necessary, the question naturally arises, can such forms be rolled? A way is always found to do that which must be done ; but if this width of rail head shall prevail, then the indications are that the Sandberg pattern of T rail—with its sacrifice of from one to two pounds per yard in reinforcing the two cantilevers of its head to accommodate splice bars that, in themselves, restore the rail to only one-half of its strength as a girder where intact—must be a thing of the past.

Fig. 3.



In search of moduli for use in Professor Grashof's formulas, I have secured, again through the kindness of Mr. G. W. Rhodes, of the Chicago, Burlington and Quincy Railway, the results of test specimens, cut from the heads of new and old rails, which are given in the following table:

I have also secured (from George Gibbs, Mechanical Engineer of

the Chicago, Milwaukee and St. Paul Railway Company) tests, chemical analyses and photographic representations of etched rails that have received severe service, which are given in the following table and sketches:

TABLE No. 6.

TEST OF RAIL, CHICAGO, BURLINGTON AND QUINCY RAILWAY.

When Made.	Maker.	Weight per Yard. Pounds.	Tensile Strength in pounds per square inch.	Elastic limit per square inch.	Per cent. Elongation in 4 inches.	Old or New Rail.
1887	F	66	119 000	60 100	10.15	New
1887	F	66	120 800	61 500	9.30	"
1887	F	66	124 100	65 800	9.00	"
1887	F	66	126 700	68 100	6.25	"
1887	F	66	107 022	55 100	12.10	"
1887	F	66	104 400	55 800	6.25	"
1887	F	66	120 437	63 400	9.96	"
1887	F	66	120 100	62 900	10.15	"
1871	F	66	75 350	42 000	16.40	Old
1871	F	66	66 130	38 700	9.40	"
1878	F	66	76 800	45 600	19.50	"
1878	F	66	79 600	50 500	18.75	"
1878	F	66	80 000	50 700	20.00	"
1878	F	66	76 000	47 100	20.00	"
1878	F	66	76 200	44 300	22.26	"
1888	A	66	87 900	40 000	18.00	New
1888	A	66	87 200	39 900	18.35	"
....	G	60	101 500	42 414	6.25	Old
1879	A	66	114 900	52 800	14.18	"
1880	F	66	74 606	35 185	30.00	"
1880	H	66	81 150	36 200	26.56	"
1882	I	66	92 300	46 500	15.60	New
1884	I	66	106 400	78 300	.....	"
1885	C	66	109 800	51 600	12.08	"
1885	C	66	111 200	52 600	14.38	"
1886	A	66	102 700	52 000	15.62	"
1886	J	66	93 300	48 132	21.00	"
1886	J	66	126 200	65 235	10.00	"
1879	K	56	80 285	37 650	22.00	Old
1879	K	56	81 280	38 100	21.50	"
1880	H	66	83 800	.....	21.00	"

It will be observed that these tests do not show any remarkable change through use. I take it as a fact that the cutting of test specimens relieves them from the internal stresses, as is supposed to be the case in reaming punched rivet holes in steel plates.

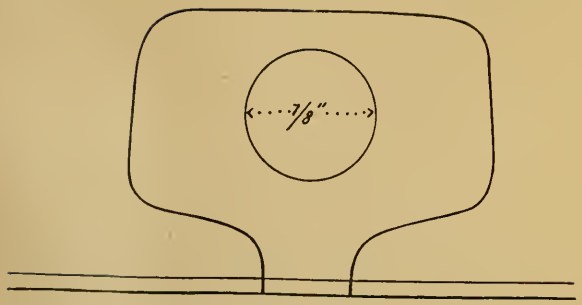
While the appearance of the photographic etchings can be sometimes observed in new rails, owing, as is believed, to faulty manufacture or of design, yet fractures of this rail before use did not show such defects of structure. It is the slow but fatal development of an internal organic disease in a structure that is not organic, if we may borrow an idea from a present report—a sort of tuberculosis disease, with its slow-forming cavities. (Plates LI, LII, LIII, LIV, LV.)

TABLE No. 7.

Tests of Steel Rails, Chicago, Milwaukee and St. Paul Railway Company.

Vol XXI, page 148.

Laboratory Number.	Number on Section.	Material.	Maker.	Per cent. of Carbon.	Per cent. of Manganese.	Per cent. of Silicon.	Per cent. of Phosphorus.	Per cent. of Elongation.	Tensile Strength, pounds per square inch.	When Laid.	When Removed.	Color and Grain of Fracture.	Dr. Dudley's Phosphorus Units.
.....	.....	56-pound steel rail.	....	.35	1.35	.025	.069	.....	.....	.....	.....	.....	....
.....	.....	60 " "	....	.45	1.63	.075	.056	.....	.....	.....	.....	.....	....
63 b	1	67 " "	A	.263	0.35	.050	.123	1.	73 200	Sept., 1884	.....	.....	30.6
64 "	2	"	B	.250	0.56	.019	.068	4.	109 500	1886	.....	.....	26.2
65 "	3	68 " "	B	.283	0.40	.013	.060	3.	89 900	July, 1886	April, 1887	.....	24.5
66 "	4	60 " "	A	.211	0.44	.025	.113	23.	88 600	Sept., 1884	July 15th, 1887	.....	28.4
72 "	9 and 10	"	C	.344	0.83	.198	.095	0.	82 100	Mar., 1883	1887	Marked rail west, bright.	47.5
73 "	11 and 12	"	C	.231	0.55	.013	.077	21.	83 900	.....	Sept., 1887	.....	27.0
76 "	13 and 14	"	C	.278	0.50	.043	.108	9.	83 100	June, 1883	Oct. 4th, 1887	.....	32.8
77 "	15 and 16	"	D	.297	0.87	.047	.068	9.	91 900	June, 1886	Sept. 26th, 1887	.....	36.4
120	17 and 18	60 " "	A	.34	0.64	.063	.146	21.	83 500	Oct., 1878	July, 1887	Dark gray, fine grain.	41.8
121	19 and 20	60 " "	A	.42	0.92	.082	.129	7.	88 000	Oct., 1878	July, 1887	Light gray, coarse grain.	49.4
122	21 and 22	60 " "	A	.46	0.97	.105	.071	15.	109 200	Oct., 1878	July, 1887	Darker than 121, coarser.	47.0
123	23 and 24	60 " "	A	.48	0.99	.065	.146	2.	80 400	Oct., 1876	June, 1887	Light gray, rather coarse.	53.6
124	25 and 26	60 " "	A	.35	0.84	.119	.140	17.	97 700	Oct., 1876	June, 1887	" fine grain.	58.3
125	27 and 28	60 " "	A	.39	0.72	.097	.167	21.	91 700	Oct., 1876	June, 1887	" "	47.9
161	31	"	A	.39	1.02	.070	.149	.....	.....	.....	.....	Short end.	51.8
162	29 and 30	"	A	.39	1.08	.103	.160	19.	99 500	.....	.....	Dark color, fine grain.	54.7
197	32 and 33	67 " "	A	.45	1.35	.098	.181	1.	86 900	Sept., 1886	Sept., 1887	Bright fracture, fine grain.	65.0
303	34	67 " "	C	.41	0.69	.034	.055	.....	.....	June 7th, 1883	Feb. 9th, 1888	.....	34.6
526	35	75 " new "	E	.28	0.67	.274	.108	23.	88 400	.....	.....	Dark gray, fine grain.	47.2
534	36	75 " " "	A	.36	1.55	.030	.067	7.	114 900	.....	.....	White and rather coarse.	51.2
547	37	60 " "	F	.39	1.52	.046	.066	14.	108 500	.....	.....	Gray, fine grain.	52.3
548	38	60 " "	C	.52	1.97	.065	.074	5.	120 200	.....	.....	White and rather coarse.	49.3
549	39	75 " new "	E	.27	0.68	.227	.088	21.	87 700	.....	.....	Fine grain, small bead.	42.7
555	40	"	D	.48	1.00	.020	.076	17.	102 300	.....	.....	" gray.	44.6
556	41	67 " "	D	.50	0.94	.021	.069	2.	84 600	.....	.....	White, coarse grain.	43.3
558	42	60 " "	B	.50	1.15	.026	.068	15.	121 700	.....	.....	Almost white, fine grain.	47.7
807	43	75 " "	E	.326	0.499	.150	.136	.....	.....	.....	.....	Short end.	42.0
808	44	75 " "	E	.266	0.536	.256	.146	.....	.....	.....	.....	"	46.9
809	45	75 " "	E	.332	0.504	.172	.149	.....	.....	.....	.....	"	44.7
810	46	75 " "	E	.288	0.543	.232	.139	.....	.....	.....	.....	"	45.9



Note —Nos. 123, 124 and 125 were between Langdon and Newport, Minn., laid on a 26-foot grade, oldest and best steel on River Division, and best when taken out.

Nos. 120, 121 and 122 were between Read's Landing and Wabasha, laid on about a level.

The following analyses of three samples of 75-pound rails were made at Laboratories at "A" and "E":

Analysis No. 1, made from a piece of 75-pound rail that came from "E," was: C., 0.30 per cent.; Mn., 0.74 per cent.; Si., 0.227 per cent.; P., 0.132 per cent.; S., 0.053 per cent. 49.3

Analysis No. 2, made by "A" this year, was: C., 0.391 per cent.; Mn., 1.31 per cent.; Si., 0.045 per cent.; P., 0.114 per cent.; S., 0.071 per cent. 52.8

Analysis No. 3, an average analysis made at "E," was: C., 0.29 per cent.; Mn., 0.40 per cent.; Si., 0.165 per cent.; P., 0.09 per cent.; S., 0.03 per cent. 34.8

Test specimens were cut from head, as shown, turned down to ¾ inch diameter 5 inches long.

George Gibbs,
Mechanical Engineer.

December 29th, 1888.

PLATE L. No. 1.

60-Pound Steel Rail.

Laid September, 1884. Removed July 15th, 1887.

Carbon	0.21	per cent.
Manganese	0.44	"
Silicon	0.025	"
Phosphorus	0.113	"
Sulphur	0.025	"

T. S. ..... 88 600 pounds per square inch.
Elongation ..... 23 per cent. in 5 inches.

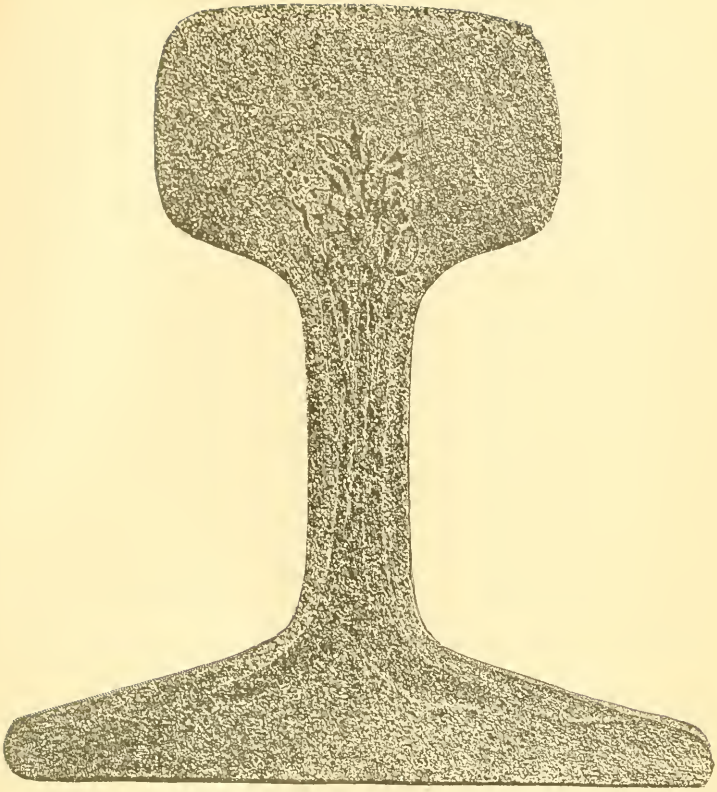


PLATE LI. No. 2.

60-Pound Steel Rail.

Laid October, 1878. Removed July, 1887.

Carbon	0.42	per cent.
Manganese	0.92	"
Silicon	0.082	"
Phosphorus	0.129	"

T. S. ..... 88 000 pounds per square inch.
Elongation ..... 7 per cent. in 5 inches.

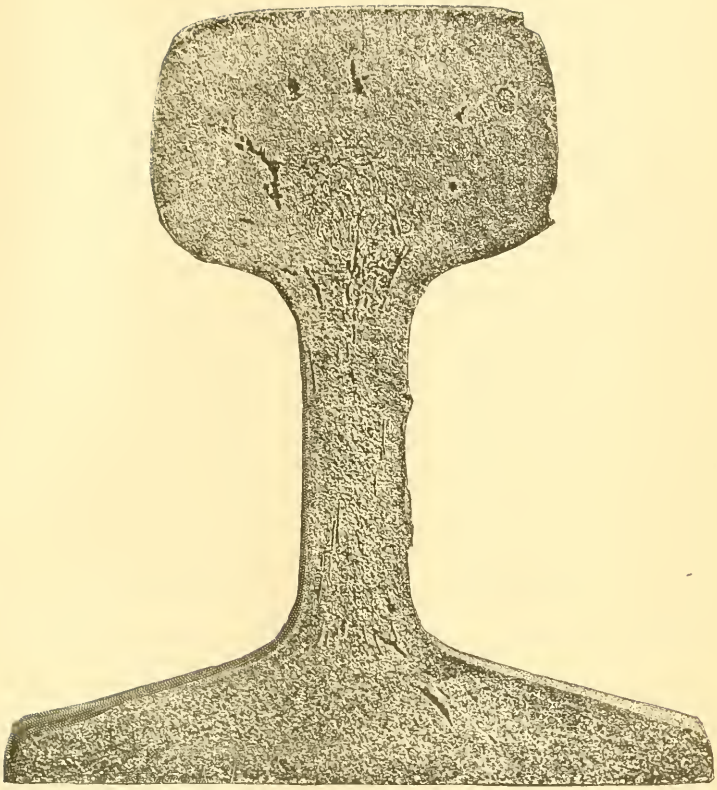


PLATE LII. No. 3.

60-Pound Steel Rail.

Laid October, 1876. Removed June, 1887.

Carbon	0.48	per cent.
Manganese	0.99	"
Silicon	0.065	"
Phosphorus	0.146	"

T. S. ..... 80 400 pounds per square inch.
Elongation ..... 2 per cent. in 5 inches.

PLATE LIII. No. 4.

60-Pound Steel Rail.

Laid October, 1876. Removed June, 1887.

Carbon	0.35	per cent.
Manganese	0.84	"
Silicon	0.119	"
Phosphorus	0.140	"

T. S. ..... 97 700 pounds per square inch.
Elongation ..... 17 per cent. in 5 inches.

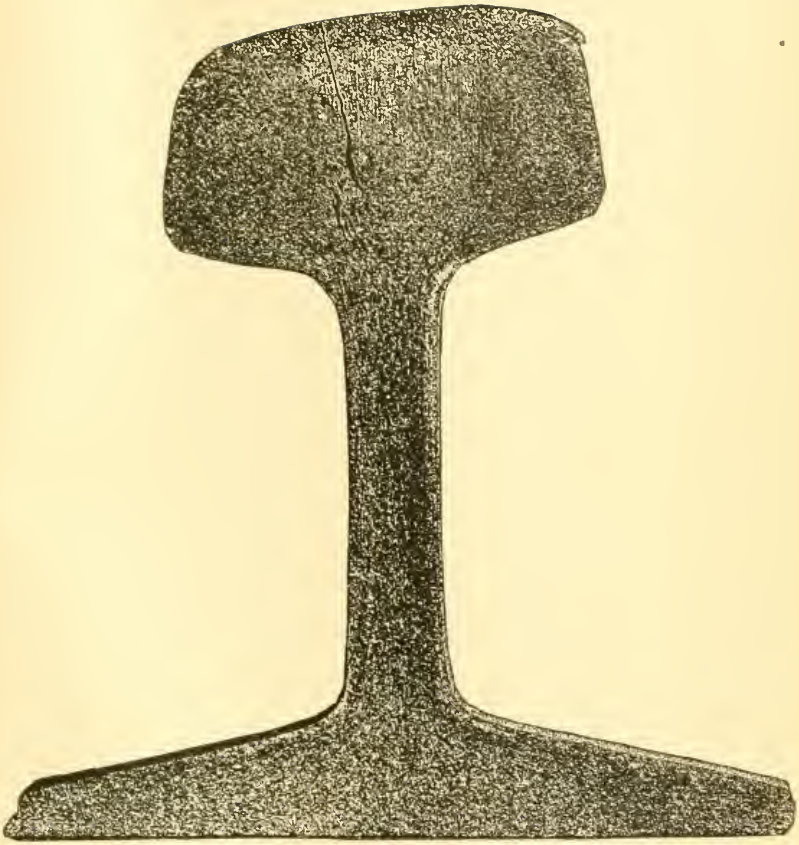


PLATE LIV. No. 5.

Caused Wreck at Rosemount.

Carbon	0.39	per cent.
Manganese	1.02	"
Silicon	0.070	"
Phosphorus	0.149	"

It is beyond the intended scope of this paper to treat of the chemical composition of steel rails; yet it may be proper for me to incorporate an extract from a letter just received from the able manager of the Union Steel Company of Chicago, Mr. Robert Forsyth, M. Am. Soc. C. E., as follows: "In regard to your inquiry as to the analyses of steel, I will say that we aim to make rail steel with: Carbon, .32 to .40 per cent.; silicon, .04 to .06; phosphorus, .09 to .105; manganese, 1.00 to 1.50 per cent. The amount of manganese is varied from time to time to suit the character of the metal we may happen to be using. We have made a great deal of steel with phosphorus lower than .09, but we cannot, as a rule, do better, nor do I think it necessary for rails."

As a matter of curiosity he sends me two analyses of rails, made not long ago, in which he determined a large number of substances not ordinarily looked for in steel. These analyses should not be considered typical of his manufacture, however. They are as follows:

ANALYSES OF TWO SAMPLES OF RAIL STEEL.

	No. 1. Per cent.	No. 2. Per cent.
Carbon	.410	.390
Manganese	1.45	1.500
Silicon	.069	.063
Phosphorus	.098	.092
Sulphur	.046	.055
Copper	.007	.022
Titanium	.000	.000
Calcium	.004	.001
Magnesium	.001	Trace
Aluminium	.010	.008
Arsenic	Trace	Trace
Antimony	Trace	Trace
Cobalt } Nickel }	.0013	.0011
Specific Gravity	7.83972	7.84395

It is justly claimed by the managers of rail mills that the engineer of permanent way is deficient in knowledge of what practicable forms can be rolled with good results; and it is also natural to suppose that the proprietors of these works desire to limit their output to the amount of metal that can be squirted through their rolls in the shortest possible time, and have the same accepted.

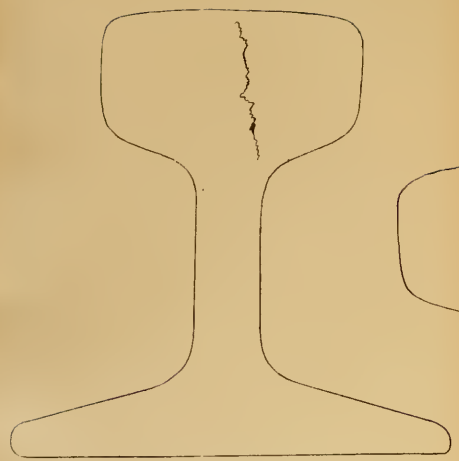
We find, however, from an examination of forms sent us by rolling mill managers, that they can roll, with uniformly good results, as great

a diversity of disposition of metal as the engineer of permanent way offers in his plans. This, however, is a branch of the subject of which it is not the purpose of this paper to treat. To such as wish light on this branch I refer to the paper on steel rails, by our Member, Robert W. Hunt, read before the American Institute of Mining Engineers in October last, in which it will appear that all the ills the rail is heir to cannot be attributed to the error of its curved top; neither do I so claim, but I do assert that whether the material be good or bad, better service will be secured when this error is eliminated.

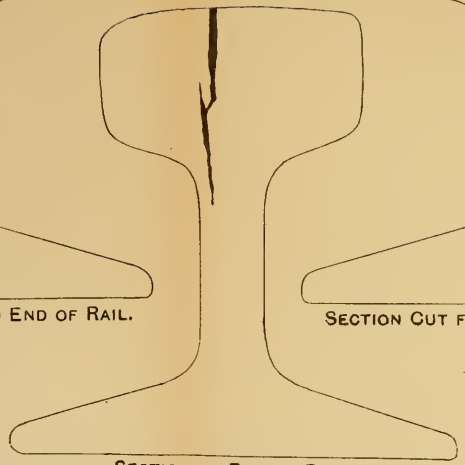
On Plate LV are shown sketches of sections of rail taken from a scrap heap showing how rails fail with little abrasion.

These rails were of the very best quality and had been in service for nine years. Measurements taken of twenty of them show a wear of less than $\frac{1}{32}$ of an inch, and the loss of weight about 1 pound per yard. How much of this was due to corrosion and how much to abrasion we do not know, but I venture to say that there was twice as much from corrosion as from abrasion. Giving these sections the curve in head that they originally possessed, and with the web projecting into it, can we devise a better form for splitting the rail from the weight imposed by the wheel than is presented ? Yet this is typical of universal practice. If my premises are true, we now see why 33 and 42-inch wheels, under our passenger equipment, render so much greater service than we have from the rail, which is subjected to pressures far beyond the elastic limit, through freight car wheels and engine drivers. The wear of engine drivers shows better results than that of rails ; but it must be remembered that there is from two to three times the metal in their cross-section than in the rail ; that there is a greater field for flow and disposition of stresses in the former than in the latter ; and yet it is not an unheard-of thing for driver tires to pipe and split. In comparing wheel with rail service, it should be borne in mind that the estimates given of tonnage service of rail apply to the two parallel rails, and not to one rail only.

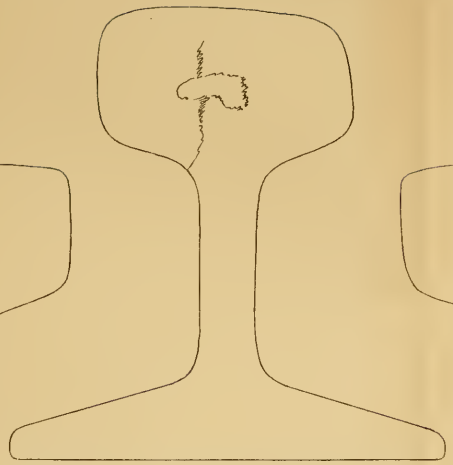
The fact is that there is hardly a wheel turning under our freight cars when loaded to their scheduled capacity, or a driver under our locomotives, that does not strain the metal in the rail beyond its modulus of resistance. From the tests given we can safely assume that for the majority of our rails this modulus can be taken at 45 000 pounds. In many instances it falls below 40 000. With a modulus of 45 000



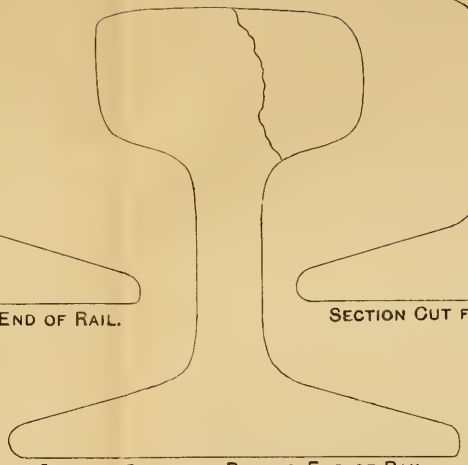
SECTION CUT FROM BEYOND END OF RAIL.



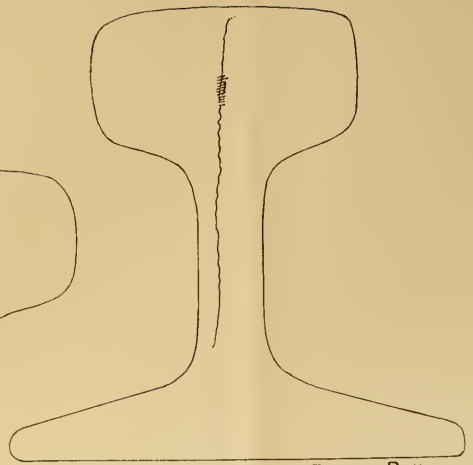
SECTION AT END OF RAIL.



SECTION CUT FROM BEYOND END OF RAIL.



SECTION CUT FROM BEYOND END OF RAIL.



SECTION CUT FROM BEYOND END OF RAIL.

pounds the car wheel can sustain about 6 000 pounds within the elastic limit, and this approximates to the weight on passenger equipment; hence the high service shown for the steel tired wheel.

Undoubtedly, many of our older members have seen the iron rail of twenty-five or thirty years ago wear down nearly one-half inch; many of them have observed, and I have measured the wear of steel rails when first introduced, and when lighter loads prevailed, a wear of $\frac{3}{8}$ of an inch before they found a resting place in that charnel-house, the scrap heap; now, under present loads, their usefulness ends with from ten to forty or fifty million tons' service. Some one spot on our overloaded engine tires wears down $\frac{1}{8}$ of an inch after a service of from twenty to forty million tons, while the steel tired passenger car wheel, which is loaded to about its elastic limit, gives a tonnage service of from one hundred and fifty to two hundred million for the same wear, nearly the whole of which wear we can reasonably attribute to the action of brake shoes and slipping of wheels, and not to abrasion due to normal conditions of rail and wheel contact.

In regard to the fillet curve of wheel flange, it is my belief that the decision of your Committee is correct—that it should not be the same as the curve of rail corner; that flange pressures should be confined, as near as possible, to the origin of the curve, with the tread of wheel; and where abrasion caused by increased periphery is the least, and also where leverage to resist stress is the most effective.

We should seriously consider the propriety of adding in any way whatever to the duty of the flange—a projection of only a trifle over 1 inch—the agent between us and disaster as we rush through space at fearful velocity. I venture to say that there is not a flange on a cast-iron wheel that cannot be knocked off, without excessive effort, by a hammer in the hands of any person in this room.

To me it appears self-evident that rolling resistance is decreased with wheels running within the elastic limit; that once wheels are of equal diameter, they will longer remain so than by the present practice; and that, therefore, the tendency of wheels to run to flange will not be nearly so great as now. The larger wheel always drags the smaller when on the same axle.

It strikes the writer that heretofore the engineer of permanent way has had just enough of the æsthetic in his nature to desire to top out his work by a graceful curve, leaving the purely mechanical engineer

to meet the condition it induces as best he could; and I take it as the duty of the civil engineer, *per se*, the real conservator of power and matter, to meet this condition.

Looking at the rail on end, in cross-section, we find it a very small affair, 6 or 8 square inches in area, and my matter treats of but a few hundredths of this area; but when looked at in longitude, we find that it has a length of over 600 000 miles, every inch of which contains, as I believe, a blunder in design.

If this opinion is a correct one, then it is the duty of our profession to hasten the slow processes of evolution and eradicate this evil in the shortest practicable time. This cannot be done at once—rails and wheels now in service will remain until their usefulness is past. The flat topped rail adopted by any single line will receive the imprint of wheels worn hollow in tread by service on round-topped rails; but I believe if those responsible agents of lines forming any one of our great transcontinental routes make earnest effort in this direction, the evil will soon be practically overcome. Already an earnest effort is being made by several managers and engineers of several important corporations to secure uniformity of rail sections.

It is believed that the change of form of rail contemplated in this paper will not involve the use of any more material than is now used on many of our railways. That dear old lady, Mrs. Partington, said that when the horse ran away with her, she trusted in Providence until the breeching broke, and then she jumped. The traces of our railways are now at their fullest tension, possessing no factor of safety within the elastic resistance. Have we not trusted in Providence too long in our violation of a law of nature? Is it not our duty to be prepared to quickly leap, not into the dark, but into the light of reason, to avoid disaster that will, that does, result from present practice?

If a change is necessary, why not have our form of rail a type of that noblest of all creations, a level-headed man with two legs and feet to correspond? Why not come back to the design of the American Strickland of 1834, re-designed by Brunel in 1835, and still in use on the Great Western Railway of England? While I am not prepared to express the opinion that the rail of the future will have this similarity, it goes without saying that disaster surely follows any attempt to work material substances, as well as our mental faculties, beyond their elastic limit.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

424.

(Vol. XXI.—October, 1889.)

DISCUSSION

ON

CYLINDRICAL WHEELS AND FLAT TOPPED RAILS FOR RAILWAYS.*

JAMES B. FRANCIS, Past President Am. Soc. C. E.—I have had little or no practical experience in the wear of rails and the tires of wheels rolling on them, but there are some general ideas that occur to me bearing on the matter.

Assuming that the top of the rail is curved in section, and the tire conical as represented by Fig. 1, Plate XLIX,† if the forms are perfect and the material of both incompressible, they would touch only in a point, where the pressure per square inch would be infinite. They are, however, both compressible, the effect of which is to form depressions on both surfaces, and to distribute the pressure over an area, the extent of which depends on the degree of compressibility. The pressure per square inch would be very unequal at different parts of this area, being greatest at the first point of contact, and diminishing from this point to the edges of the depressions, where it would be nothing.

If the forms of the rail and tire are as represented by Fig. 2, Plate XLIX, the contact, if the material is incompressible, would be a line, on every point of which the pressure per square inch would be infinite, and if compressible, depressions would be made, having this line for

* Cylindrical Wheels and Flat Topped Rails for Railways, by D. J. Whittemore, Trans. Am. Soc. C. E., Vol. XXI, p. 133, September, 1889.

† Transactions, Vol. XXI, p. 142, September, 1889.

an axis, where the pressure per square inch would be the greatest, and at the edges of the depression it would be nothing.

The area of the depression would be greater, and its maximum depth less, in the case represented by Fig. 2 than by Fig. 1, and the total pressure being the same in both cases, the mean pressure per square inch of the depression would be proportionally less in Fig. 2.

I think the destructive effect of the pressure on the rail and tire is substantially proportional to the maximum pressures which would be obviously greater in Fig. 1 than in Fig. 2.

The above applies only to the case where the surfaces are unworn, and the motion uniform both in velocity and direction; practically these conditions are far from being fulfilled. The effect of use on Fig. 1, I think, must be to approximate to Fig. 2, and it would appear to be best to adopt that form, or something near it, to begin with.

On page 146 of the paper, after describing the great variations in the forces, it is remarked: "Our wonder is, not that rails fail so soon, but that they last so long." My idea is that the destructive effects of pressure, impact, etc., are largely modified by their distribution through the mass of the head of the rail; the application of the pressure or impact would usually be on a small area of the top of the rail, and the pressure would be sufficient to crush an isolated prism with a base of that area; to be crushed, however, it must spread laterally, which is prevented to a great extent by the adjacent parts of the head of the rail, and instead of crushing the prism, by the elasticity of the metal a large part of the effect is transferred to other parts of the head of the rail. If the point of contact is near one side of the head of the rail, the prism would have less support from adjacent parts, and these parts of the head would be the weakest.

M. J. Becker, President Am. Soc. C. E.—The statements contained in the introductory sentences of Mr. D. J. Whittemore's paper on "Cylindrical Wheels and Flat Topped Rails for Railways" must certainly be accepted without controversy. It is true that the wheel-loads are larger than ever before, and it is not at all likely that they will ever be reduced; it is equally true that the rail sections have not been increased in proportion to the heavier loads imposed, and that the rails are therefore subjected to stresses somewhat beyond their proper resisting capacity.

While I agree with Mr. Whittemore thus far, I am not prepared to admit either the full extent of the consequences due to these admitted facts, nor do I quite agree with his views regarding the efficiency of the remedies he suggests.

In the first place, the wheels and rails which are annually consigned to the scrap heap, and the value of which he estimates at a billion of dollars every few years, are by no means a total loss. As far as the old rails

are concerned, they bear, at present prices, a proportion to the value of new rails, of about 22 to 28.

Assuming the total mileage of all tracks (main, second and yard tracks) in this country at 300 000 miles, and counting 100 tons per mile, we would have an aggregate of 30 000 000 tons of rail. Assuming, further, the average life of rail in all these tracks at fifteen years, the annual renewal would amount to 2 000 000 tons, and the absolute loss, resulting from the difference in value between old and new rail, at say $6 per ton, would be $12 000 000 per annum.

The number of wheels attached to our combined rolling stock and motive power is about 8 000 000, and their aggregate weight is about 2 500 000 tons; the wheels annually broken and condemned for all causes amount to about 300 000 tons, and the difference in value between scrap and new wheels is nearly $13 per ton, so that the actual annual loss in wheels would foot up to about $3 900 000. This, together with the $12 000 000 for rails, gives us $15 900 000; an amount very far from Mr. Whittemore's billion. Indeed, the total value of all tracks and wheels in the country does not amount to anything like a billion. Still the value of the annual scrap heap amounts to quite a large sum, and if it can be reduced, it ought certainly to be done.

But let us take a look at the other side of the picture and see what has been gained by the adoption of heavier power and larger carriages.

Comparative Statement of Engine and Car Mileage, Tonnage and Ton Mileage, for Years 1873, 1878, 1883 and 1888, and Percentage of Increase or Decrease Compared with 1873.

Pittsburgh, Fort Wayne and Chicago Railway.

	1873.	1878.	Per cent.	1883.	Per cent.	1888.	Per cent.
Engine mileage ...	5 751 670	4 415 830	— 23.2	5 202 559	— 9.5	3 961 950	— 31.1
Car mileage.....	70 654 926	92 533 418	+ 30.9	116 958 485	+ 65.0	116 112 930	+ 64.3
Tonnage	2 292 644	3 026 250	+ 32.0	5 076 311	+121.5	6 210 816	+170.9
Ton mileage	479 917 429	637 470 506	+ 32.8	944 563 376	+ 96.8	1 004 657 034	+109.3

Pittsburgh, Chicago and St. Louis Railway.

	1873.	1878.	Per cent.	1883.	Per cent.	1888.	Per cent.
Engine mileage ...	2 312 780	2 037 633	— 11.8	2 672 601	+ 15.5	2 803 974	+ 21.2
Car mileage	36 189 869	40 376 875	+ 11.5	48 437 368	+ 33.8	66 332 026	+ 83.2
Tonnage	1 472 709	2 142 155	+ 45.4	3 466 544	+ 67.4	5 239 796	+255.8
Ton mileage.....	205 508 887	287 757 418	+ 40.0	428 293 199	+108.5	622 997 128	+203.1

+ Increase. — Decrease.

The above statements give the result upon the Pittsburgh, Fort Wayne and Chicago Railway, and the Pittsburgh, Chicago and St.

Louis Railway, and they show, that while on the former road, between 1873 and 1888, the engine mileage has decreased 31 per cent., the car mileage has increased 64 per cent.; the tonnage has increased 171 per cent. and the ton mileage has increased 109 per cent. On the Pittsburgh, Cincinnati and St. Louis Railway, the engine mileage during the same period has increased 21 per cent.; the car mileage has increased 83 per cent.; the tonnage increased 256 per cent.; and the ton mileage increased 203 per cent.

Similar results are obtained on all lines where the motive power capacity has been judiciously increased with the enlarged freight car capacity.

Now, of course, it would not be reasonable to expect such favorable results entirely without a sacrifice of some sort.

But this is not all. Look at the chart of rail-prices and note the reduction in cost from $120 in 1873 to $30 in 1888 for steel rail; and also glance at the price of $70 per ton for iron rail in 1873 and remember that this rail, under the much lighter wheel-loads of that time, regularly wore out in from six months to two years' time.

And then summarize the situation in 1873, and compare it with 1888. You will find that our present engines, with a greatly reduced mileage, transport much larger tonnages, that our rails cost 75 per cent. less than in 1873, and wear six times as long; so that really our condition appears satisfactory and encouraging, rather than gloomy and disheartening as Mr. Whittemore would make us believe.

Rails have been worn in the past under much lighter loads, and no doubt they will continue to wear out in the future, and so will the wheels; in fact, there is nothing upon a railroad that does not wear out; even the chief engineers, tough as they generally are, wear out in course of time.

Mr. Whittemore admits the difficulty of determining the amount of abrasion due to rolling load alone, and I agree with him fully; and I also confess that I have precious little faith in the development of a formula which gives the theoretical ton service for $\frac{1}{8}$-inch wear of wheel-tread deduced from results varying between 107 000 000 tons and 252 000 000 tons under nearly equal wheel-loads. No doubt it would seriously complicate the solution of an equation which would contain variable values for corrosion, brake shoes, sanded rails, slipping drivers, sliding wheels, abrasion by torpedoes, flow of metal, and internal strains due to imperfect manufacture; and I suggest, that the quantity remaining after deducting all these enumerated values, and which remainder would represent the legitimate wear due to the tonnage, would be anything but constant or practically reliable.

To remedy the effect of excessive loads upon the rails, Mr. Whittemore proposes to abolish coned wheels, make the rail head flat, and by increasing the contact area and bearing surface, reduce the unit pressure upon rails and wheels alike.

PLATE LVI
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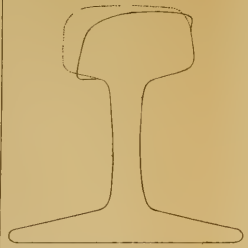
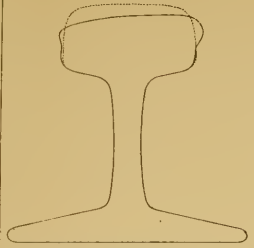
WHEELS AND RAILS.

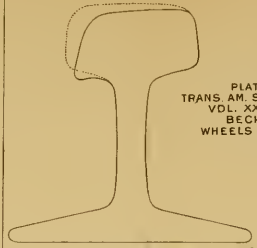
4°15' Curve west of Br. 7, west bound track. OUTSIDE Rail. CAMBRIA, 11-'77. 60" steel.
Grade + 7.02



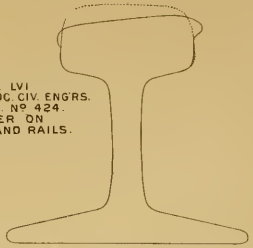
6° Curve west of "FORT PITT" west bound track OUTSIDE Rail CAMBRIA, '78. 60" steel.
Grade + 25.0



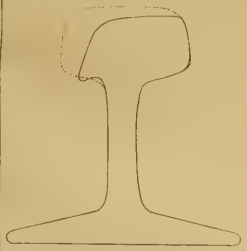
6° Curve west of "FORT PITT" west bound track. INSIDE Rail, CAMBRIA 1878. 60" steel.
Grade + 25 0



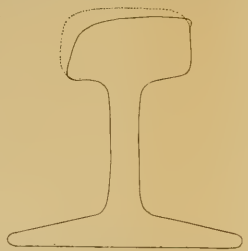
3° Curve at "WALKER'S MILL" Sta., west bound track, OUTSIDE Rail, CAMBRIA 1878. 60 steel.
Grade + 42 24



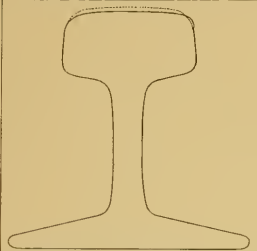
5° Curve 1st west of Br. 14, east bound track, INSIDE Rail, CAMBRIA 1878 60" steel.
Grade. 0 00.



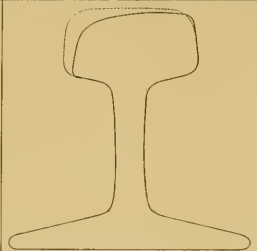
6° Curve over Br. 14, east bound track. OUTSIDE Rail, CAMBRIA 1878. 60" steel.
Grade. 0.00



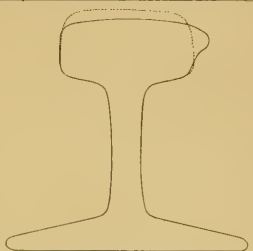
5° Curve at "OAKDALE" telegraph office. OUTSIDE Rail, CAMBRIA 1878. 60" steel.
Grade + 26 +



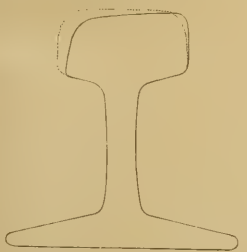
TANGENT, just east of Br. 15, S. rail, east bound track, CAMBRIA 1878. 60" steel.
Grade + 2 64.



3° Curve at "NOBLESTOWN" station, W. bound tk., OUTSIDE Rail, CAMBRIA 1878. 60" steel.
Grade + 52.8

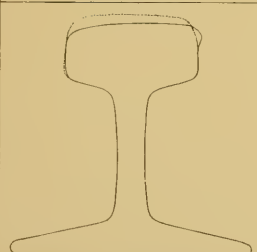


3° Curve at "NOBLESTOWN" station, E. bound track, INSIDE Rail, CAMBRIA 1878. 60" steel.
Grade + 52 8

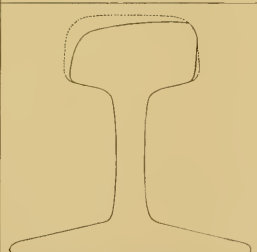


3° Curve, 1st east of "LAUREL HILL" E. bound tk., OUTSIDE Rail, CAMBRIA 1875. 60 steel.
Grade + 5.28

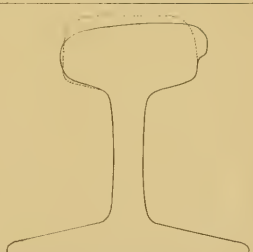
3° Curve, 1st east of "LAUREL HILL" W. bound tk., OUTSIDE Rail, CAMBRIA 1875, 60 steel.
Grade + 5.28



TANGENT, E. of "LAUREL HILL" mines, W. bound tk. N. Rail, CAMBRIA 1875. 60" steel.
Grade + 5.28



2° Curve, 1st east of "PRIMROSE" west bound tk., OUTSIDE Rail, CAMBRIA 1875. 60" steel.
Grade + 26.4



4° Curve, 2nd E. of "MIDWAY" W. bound track. INSIDE Rail, opposite telegraph tower. "CAMBRIA" 1875. 60" steel.
Grade + 52.8

PLATE LVII
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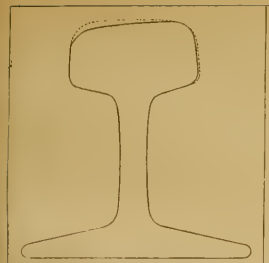
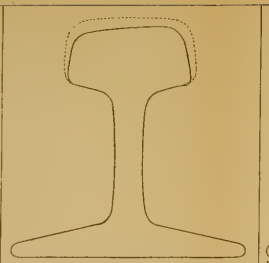
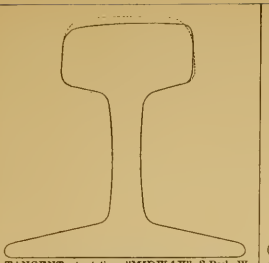


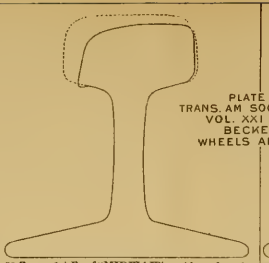
TANGENT, 2nd E. of "MIDWAY" W. bound tk., N. Rail, CAMBRIA 1879. 60' steel.
Grade + 52.8



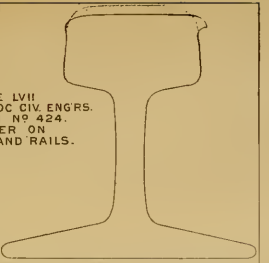
TANGENT, 2nd E. of "MIDWAY" W. bound tk., N. Rail, CAMBRIA 1876. 60' steel.
Grade + 52.8



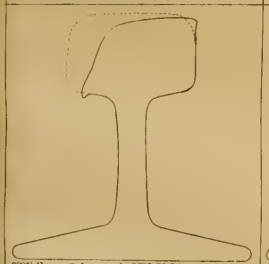
TANGENT at station, "MIDWAY" S. Rail, W. bound tk. at Xover east of Midway, CAMBRIA 1875. 60' steel.
Grade + 52.8



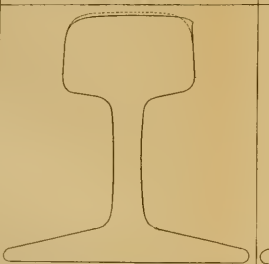
3° Curve, 1st E. of "MIDWAY" west bound track, OUTSIDE Rail. CAMBRIA 1875, 60' steel.
Grade + 52.8



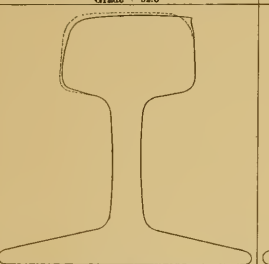
6°25' Curve, 3rd west of "BULGER" tunnel, S. tk., used as single tk. till 12-23-'88, E.T.7-82, INSIDE Rail, 67' steel. Grade - 52.8



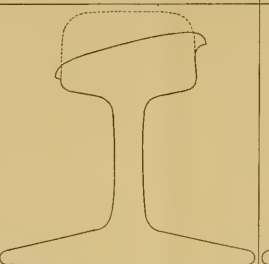
6°25' Curve, 3rd west of "BULGER" tunnel, S. tk., used as a single track till 12-23-'88. E.T.7-82, OUTSIDE Rail, 67' steel. Grade - 52.8



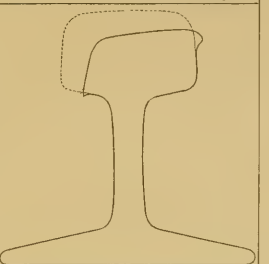
TANGENT, W. of "Springer's Dump" S. track, used as single track till 12-23-'88. E.T.7-82, S Rail 67' steel. Grade - 52.8



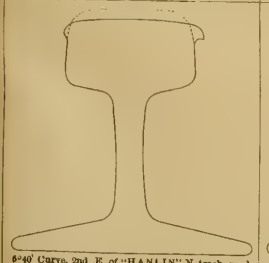
TANGENT, W. of "Springer's Dump" S. track used as single track till 12-23-'88. E.T.7-82, N. Rail 67' steel. Grade - 52.8



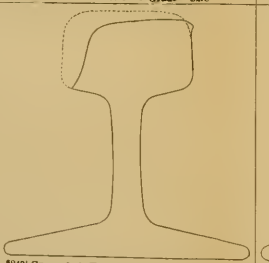
8° Curve at W. end of "DINSMORE" tunnel, single track, INSIDE Rail, E. T. 3-82, S. Rail, 67' steel.
Grade - 52.8



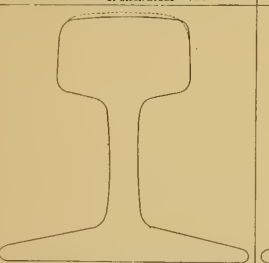
8° Curve at W. end of "DINSMORE" tunnel, single track, OUTSIDE Rail, E.T. 3-82, N. Rail, 67' steel.
Grade - 52.8



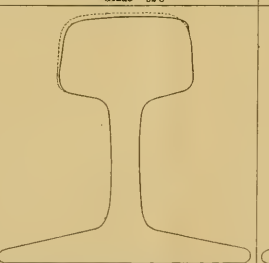
6°40' Curve, 2nd E. of "HANLIN" N. track, used as single track till 8-2-'88, E.T. 67' steel 2-'81, S. rail
Grade - 51.42



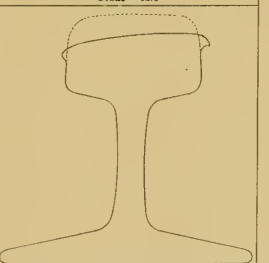
6°40' Curve, 2nd E. of "HANLIN" N. track, used as single track till 8-2-'88, E.T. 2-'81, 67' steel N. rail
Grade - 51.42



TANGENT, E. of "HANLINS" N. track, used as single track until Aug. 2, '88, E.T. '81, 67' steel, S. rail. Grade - 51.42



TANGENT, E. of "HANLINS" N. track, used as single track until Aug. 2, '88, E.T. 2-'88, 67' steel, N. rail. Grade - 51.42



6°20' Curve W. of "HANLIN" station, S. tk., used as single tk. till Oct. '67, E.T. 2-'81, 67' steel, S. rail
Grade - 51.42

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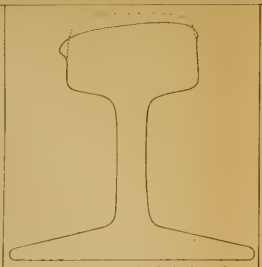
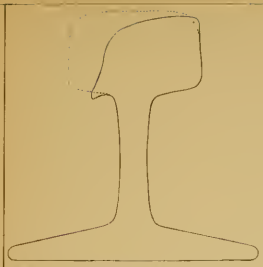
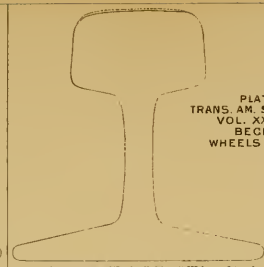
6°20' Curve W. of "HANLIN" station, S. tk., used as single tk. till Oct. '87, E.T.2-'81, 67# steel, N rail. Grade - 51.42



8°0 Curve, E. of Br. 18. South track., used as single track prior to Oct. '87. E.T.2-'81, 67# steel, S. rail. Grade - 51.42

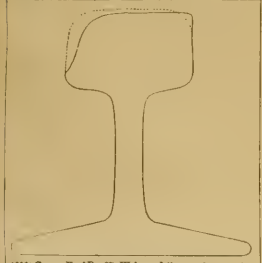


6°0 Curve, E. of Br. 18. S. or E. bound tk., used as single track prior to Oct. '87. E.T.2-'81, 67# steel, N. rail. Grade - 51.42

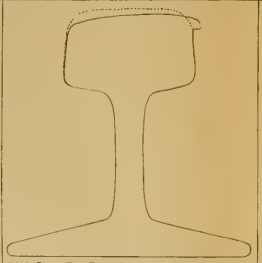


Long Tangent, 2m W. of "Colliers" W. bound track used as single track until Oct. '86, E.T.7-'80 67# steel S. rail Grade - 32.21

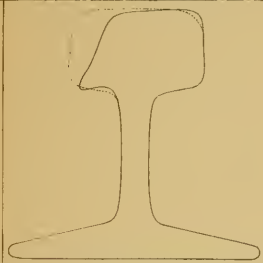
Long Tangent, 2m W. of "Colliers" W. bound track used as single track until Oct. '86, E.T.7-'80 67# steel S. rail. Grade - 32.21



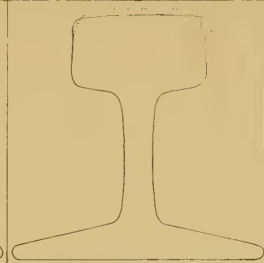
6°10' Curve E. of Br. 27, W. bound tk., used as single tk. until Oct. '86, E.T.7-'80, 67# steel, S. rail OUTSIDE rail. Grade - 32.21



6°10 Curve, E. of Br. 27, W. bound tk., used as single tk. until Oct. '86, E.T.7-'80, 67# steel, N. rail INSIDE rail. Grade - 32.21

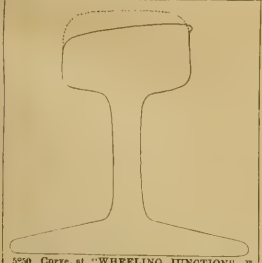


3°0' Curve, 2nd W. of "New Cumberland Junction" E. bound tk, in use as single tk. until 1887, E.T.7-'80 67# steel. OUTSIDE [S] rail Grade + 42.24

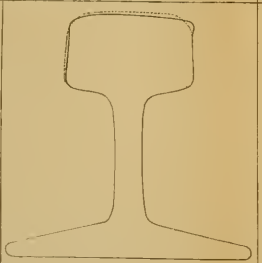


3°0' Curve, 2nd W. of "New Cumberland Junction" E. bound tk, in use as single tk. until 1887, E.T.7-'80 67# steel, INSIDE [N] rail. Grade + 42.24

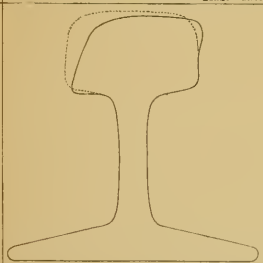
5°50 Curve at "WHEELING JUNCTION" E. bound tk., used as single tk. until '87 and again during summer of '88, E.T.7-'80, 67# steel, OUTSIDE [S] rail Grade + 42.24



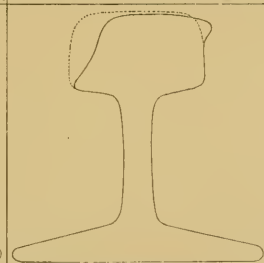
5°50 Curve, at "WHEELING JUNCTION" E. bound tk., used as single tk. until '87 and again during summer of '88, E.T.7-'80, 67# steel, INSIDE [N] rail Grade - 42.24



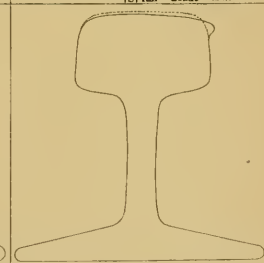
3° Curve at "IRONDALE" INSIDE rail, E.T.'83 Grade + 11.09



3° Curve at "IRONDALE" OUTSIDE rail, E.T.'83 Grade + 11.09



About 12m E. of "FERNWOOD" 2°0' Curve to left E.T.'83 OUTSIDE Rail. Grade + 11.09



2° Curve 1st E of "FERNWOOD" INSIDE Rail. E.T.'83 Grade + 11.09

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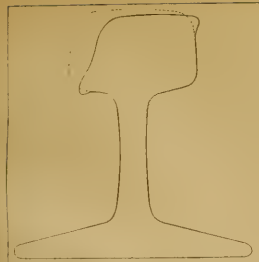
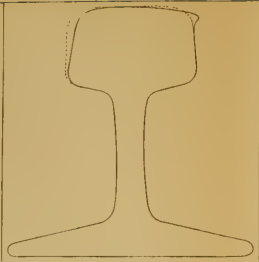
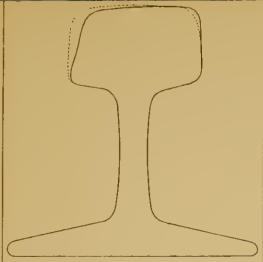
WHEELS AND RAILS.



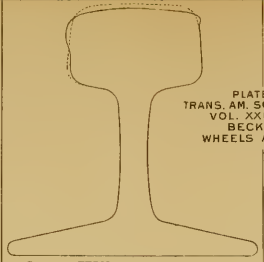
2° Curve, 1st east of FERNWOOD OUTER Rail E. T. '83. 67# steel.
Grade + 13.73



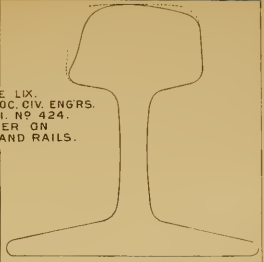
TANGENT E. of FERNWOOD S rail, E.T '83
67# steel
Grade + 13.73



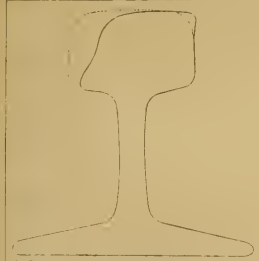
2° at FERNWOOD OUTER rail. E.T.'83,
67# steel.
Grade + 13 73



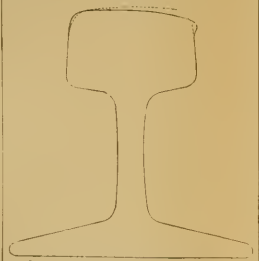
2° Curve at FERNWOOD INSIDE rail, E T '83
67# steel.
Grade + 13.73



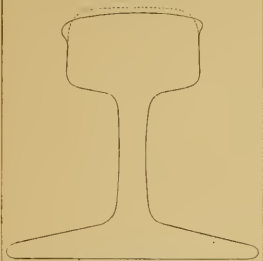
3° Curve 1st east of Br. 36, OUTER rail, E.T.'83.
67# steel.
Grade + 39.6



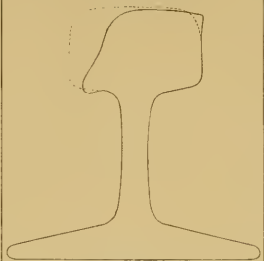
3° Curve, 1st E. of No. 6 TUNNEL, OUTSIDE Rail, E.T.83. 67# steel.
Grade + 39.6



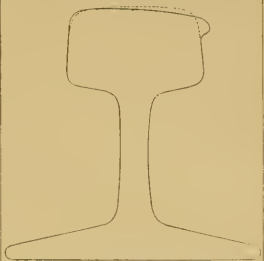
3° Curve, 1st. E. of No. 6 TUNNEL, INSIDE Rail, E.T.83, 67# steel.
Grade + 21.12



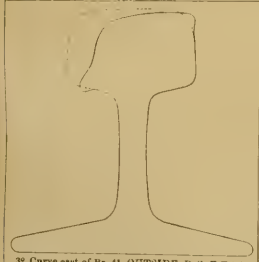
3° Curve over Br. 39, INSIDE Rail, E.T.'82,
67# steel.
Grade 0.00



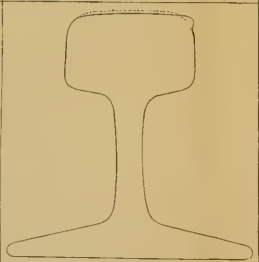
3° Curve over Br. 39, OUTSIDE Rail, E.T.'82.
67# steel.
Grade. 0.00



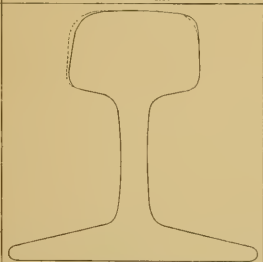
3° Curve east of Br. 41, INSIDE Rail, E.T. '82.
67# steel.
Grade 0.00



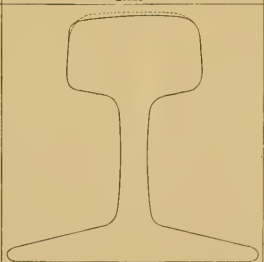
3° Curve east of Br. 41, OUTSIDE Rail, E.T. '82.
67# steel.
Grade. 0.00



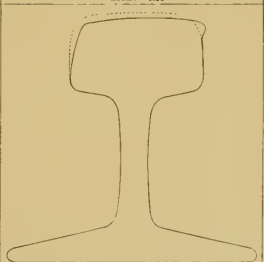
2° Curve, 1st. east of SKELLEY'S, INSIDE Rail, E T. '82, 67 steel.
Grade + 15.34



2° Curve, 1st, east of SKELLEY'S, OUTSIDE Rail, E T. '82, 67 steel.
Grade + 15.34



1st TANGENT E. of "SKELLEY'S," N. Rail, E T. 82, 67# steel.
Grade + 39.60



TANGENT, east of No. 7 Tunnel, CAMBRIA. 61. 67# steel.
Grade + 39.60

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3° Curve at Br. 43, OUTSIDE Rail, CAMBRIA '81, 67' steel.
Grade + 13.2

3° Curve at Br. 43, INSIDE Rail, CAMBRIA '81, 67' steel.
Grade + 13.2

3° Curve, at "BLOOMFIELD" INSIDE Rail, CAMBRIA, '81. 67' steel.
Grade + 39.6

3° Curve, at "BLOOMFIELD" OUTSIDE Rail, CAMBRIA, '81. 67' steel.
Grade + 39.6

2°14' Curve 1st east of Br. 44, OUTSIDE Rail, CAMBRIA '81, 67' steel.
Grade + 39.6

2°14' Curve 1st east of Br. 44, INSIDE Rail, CAMBRIA '81, 67' steel.
Grade + 39.6

TANGENT, east of No. 8 Tunnel, S. Rail, CAMBRIA '81. 67' steel.
Grade + 39.6

Between Br. 45 and No. 8 Tunnel. INSIDE Rail. CAMBRIA '81, 67' steel. 3° Curve.
Grade + 39.6

Between Br. 45 and No. 8 Tunnel, OUTSIDE Rail. CAMBRIA '81, 67' steel, 3° Curve.
Grade + 39.6

Between Br. 45 and No. 8 Tunnel, INSIDE Rail, E T 11-'82, 67' steel, 3° Curve.
Grade + 39.6

3° Curve Br. 45, OUTSIDE Rail, E T. 11-'82, 67' steel
Grade + 39.6

TANGENT, at "CONOTTON" N. Rail, E T '76 60' steel.
Grade - 3.17

3° Curve, 1st. W. of "CONOTTON" INSIDE Rail E T. '76, 60' steel.
Grade - 3.17

3° Curve, 1st. W. of "CONOTTON" OUTSIDE Rail E T. '76, 60' steel.
Grade - 3.17

1° 40' Curve at Br. 59, OUTSIDE Rail, E. T. '76, 60' steel.
Grade + 39.6.

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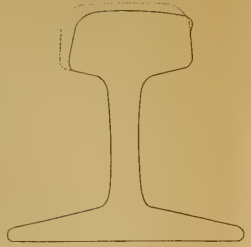
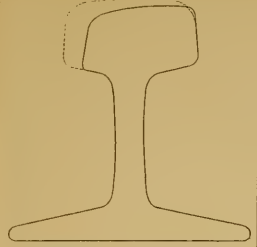
BECKER ON
WHEELS AND RAILS

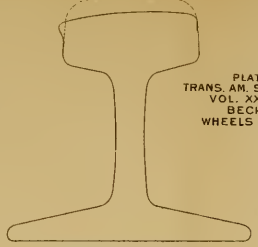
1º 40' Curve at Br. 59, INSIDE Rail, E.T.'76, 60' steel.
Grade + 39.6



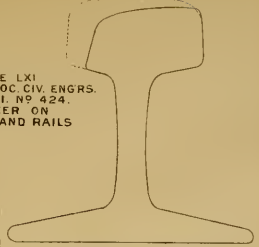
1º Curve at "BOWERSTOWN" OUTSIDE Rail, E.T.'76, 60' steel.
Grade + 39.6



2º Curve; 3rd E of "PHILADELPHIA ROAD" OUTSIDE Rail. E.T.'76, 60' steel.
Grade - 39.6

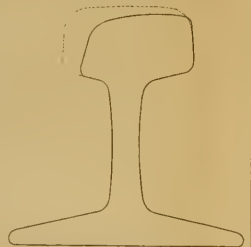


2º Curve; 3rd E of "PHILADELPHIA ROAD" INSIDE Rail, E.T.'76, 60' steel.
Grade - 39.6

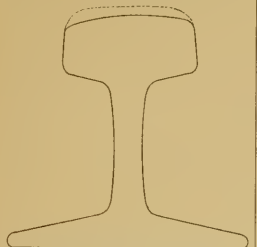


2º Curve; 3rd E. of "PHILADELPHIA ROAD" OUTSIDE Rail, E.T.'76, 60' steel.
Grade - 39.6

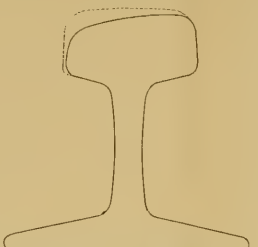
2º Curve; 3rd E of "PHILADELPHIA ROAD" INSIDE Rail, E.T.'76, 60' steel
Grade - 39.6



2º Curve; 1st W. of M. P. '87, OUTSIDE Rail, E T.'76, 60' steel.
Grade - 39.6



1º Curve about 6m E of "PHILADELPHIA ROAD" INSIDE Rail, E.T.'76, 60' steel.
Grade - 39.6



2º Curve 1st East of "PHILADELPHIA ROAD" OUTSIDE Rail, E.T.'76, 60 steel.
Grade - 39.6

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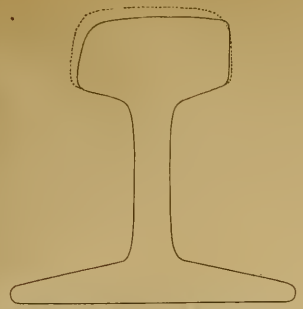
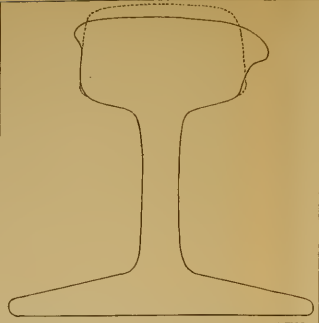
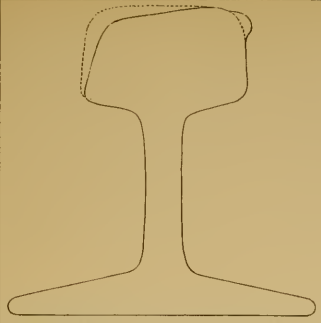


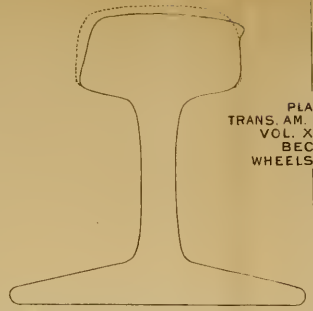
East of "UHRICHSVILLE JUNCTION" E bound track, "CAMBRIA" 1876, TANGENT, 60* steel.
Grade 0.00



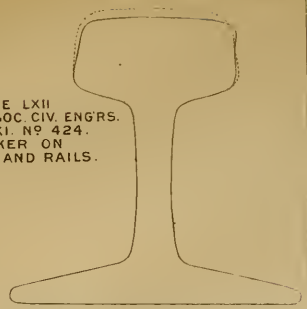
INSIDE Rail on 3° Curve, 1st Curve E. "TRENTON" E.bound tk, near Joint, E.T.1883, 67 steel.
Grade - 36.96



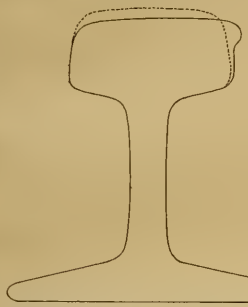
OUTSIDE Rail on 3° Curve, 1st Curve east of "TRENTON" E.bound track, E.T.1883, 67*steel.
Grade - 36.96



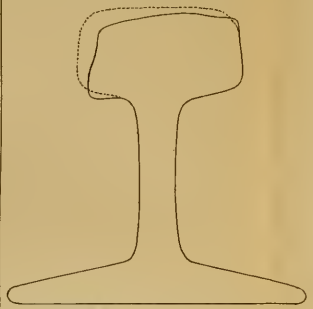
South side, main track, 1m.W. of "PORT WASHINGTON" TANGENT, CAMBRIA '78, 60*steel.
Grade - 2.64



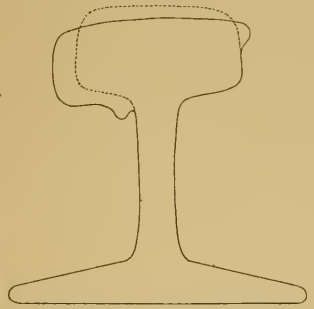
North Rail, main track, 1m.W. of "PORT WASHINGTON" TANGENT, CAMBRIA '78, 60*steel
Grade - 2.64



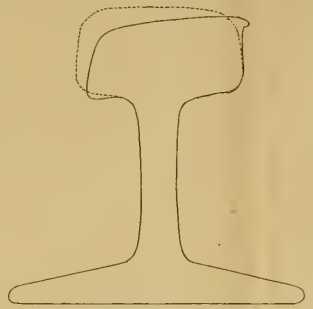
INSIDE Rail on 1° Curve, "GLASGOW" N.Rail, at Joint, CAMBRIA '78. 60* steel.
Grade - 1.58



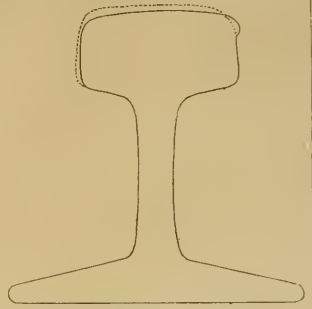
OUTSIDE Rail on 1° Curve, "GLASGOW" S.rail CAMBRIA '78, 60* steel,
Grade - 1.58



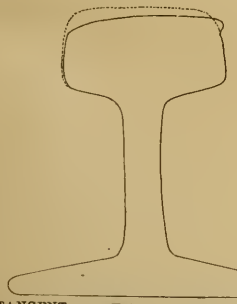
INSIDE Rail on 1° Curve, "GLASGOW" near joint, tie mashed, CAMBRIA '78, 60*steel, N.rail
Grade - 1.58.



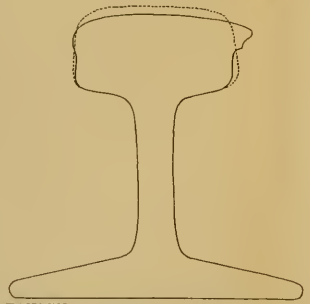
OUTSIDE Rail on 1° Curve, "GLASGOW" S.Rail, CAMBRIA '78. 60*steel.
Grade - 1.58



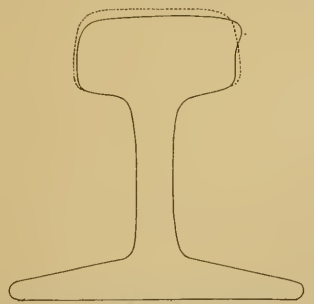
North [INSIDE] rail on Curve [1°] at "GLASGOW" CAMBRIA " '78, 60*steel.
Grade - 1.58



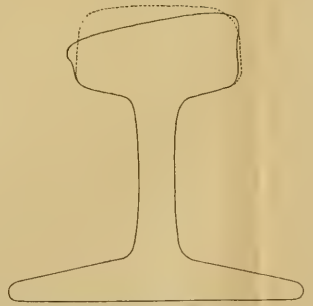
TANGENT, near E Sw. "New Comerstown" N. rail. CAMBRIA '78, 60*steel.
Grade - 4.22.



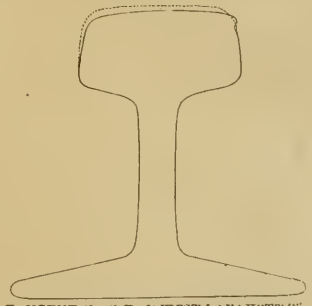
TANGENT, near E Sw. "New Comerstown" S. rail. CAMBRIA '78. 60*steel.
Grade - 4.22.



Section of best rail near E Sw. "New Comerstown" TANGENT, S. rail, CAMBRIA '78, 60*steel,
Grade - 4.22.

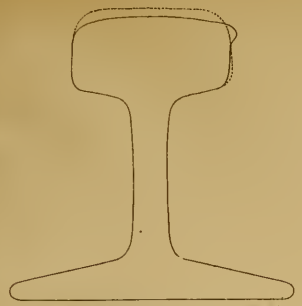


TANGENT opposite "New Comerstown" station, N.rail, CAMBRIA '78, 60*steel.
Grade - 4.22.

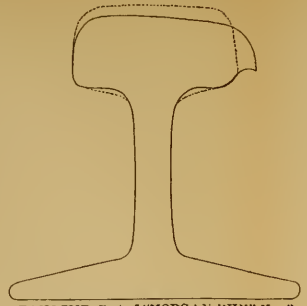


TANGENT, N rail, E. of "WEST LAFAYETTE" station, CAMBRIA '77, 60*steel,
Grade. 0.00

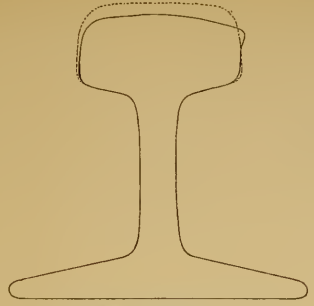
PLATE LXIII.
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XXI. Nº 424.
BECKER ON
WHEELS AND RAILS.



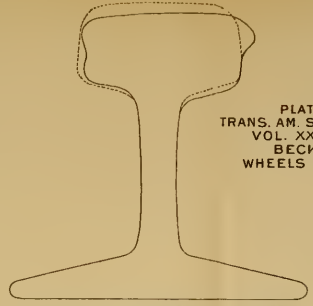
TANGENT, East of "MORGAN RUN" S. rail CAMBRIA '77. 60' steel.
Grade – 2.64



TANGENT, East of "MORGAN RUN" N. rail at Joint, CAMBRIA '77. 60' steel.
Grade – 2.64

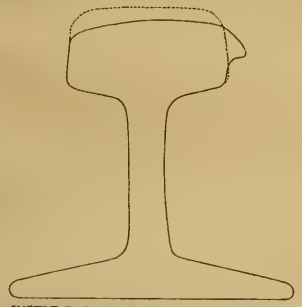


TANGENT, East of "MORGAN RUN" N. rail at Joint, CAMBRIA '77. 60' steel.
Grade – 2.64

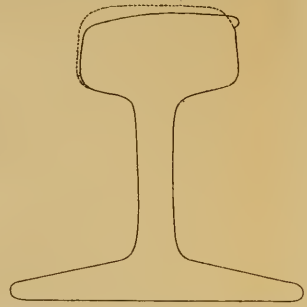


0°30' Curve, INSIDE rail at Joint, "ADAMS' MILL" N. Rail, CAMBRIA '77. 60" steel.
Grade. 0.00

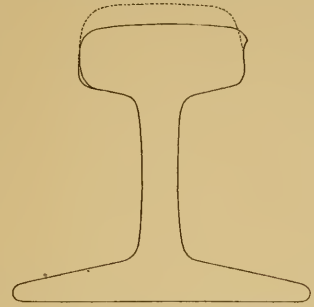
0°30' Curve, OUTSIDE rail "ADAMS" MILL" S. Rail, CAMBRIA '77. 60' steel.
Grade. 0.00



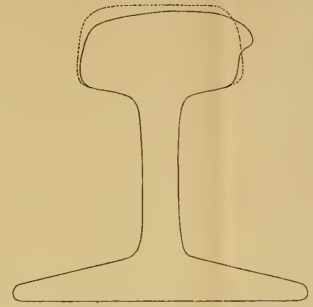
INSIDE Rail, on 0°30' Curve, "ADAMS' MILL, N. rail. CAMBRIA '77. 60' steel.
Grade. 0.00



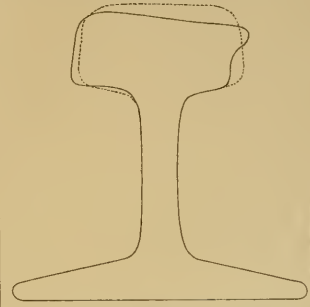
South rail on TANGENT, 1m. E. of "DRESDEN JUNC." CAMBRIA '77. 60 steel.
Grade – 0.528



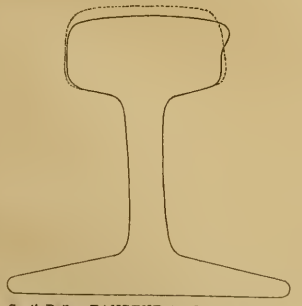
South rail on TANGENT, 1m. W. of "DRESDEN JUNC." E.T. '75. 60' steel.
Grade – 0.528



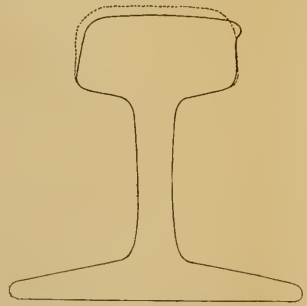
North rail on TANGENT, 1m. E. of "DRESDEN JUNC." CAMBRIA " '77. 60" steel,
Grade – 0.528



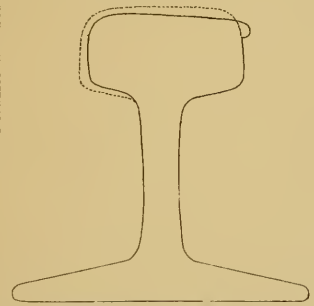
North rail on TANGENT, 1m. W. of "DRESDEN JUNC." CAMBRIA '77. 60" steel,
Grade – 0.528



South Rail on TANGENT, 1m. W. of "WAKA-TOMIKA" CAMBRIA '77. 60 steel.
Grade. 39.5–



North Rail on TANGENT, west of "FRAZEYS-BURG" telegraph office, CAMBRIA '77. 60" steel.
Grade. 0.00



North Rail on TANGENT, near E. Sw. at "BLACK RUN," CAMBRIA '77. 60" steel.
Grade. 0.00

If this would satisfactorily accomplish the purpose, it certainly ought to be done; but I would apprehend that the wheels would not maintain their perfectly cylindrical shape, nor the rails preserve their flat horizontal heads after a short period of actual use. Between the track gauge and the wheel gauge there is a difference of ¾ of an inch, which admits of a continuous oscillation corresponding to that space, and unless the wheels can be made of a metal wholly indestructible, a curved groove will be worn into their treads in course of time, which will either cause the wheels to run upon the outer edges of the flat rail head, and thereby abrade the corners; or, more likely, will wear the entire rail head to a curve corresponding to the groove in the tread.

I attach hereto prints of 112 rail sections (see Plates LVI–LXIII), carefully measured at various places in our tracks between Pittsburgh and Columbus, showing the sizes and conditions of the rails, and if any lesson is to be learned from this exhibit, it is to the effect, that under ordinarily favorable conditions, the rails wear reasonably well; upon sharp curves they naturally wear faster; a slight flow or spreading of the metal is indicated upon the outer edges of the rails. No doubt the same flow occurs on the inner edges as well, but is constantly ground off by the wheel flanges; but this flow does not, in my opinion, hasten the destruction of the rails to any marked extent; they fail upon tangents from vertical wear and upon curves by the abrasion of the inner sides of the heads. As long as this kind of wear continues uniform, there can be no special cause for complaint. It is the irregular destruction of individual rails from defective manufacture which gives just cause for dissatisfaction and alarm.

These defects have been very clearly pointed out by Mr. R. W. Hunt, by Mr. Mattes, and by Mr. Hawks; their causes are being generally understood and admitted and the remedies will no doubt be promptly applied. Wider and shallower heads with flatter tops and sharper corners will avoid the defects caused by internal strains due to injudicious distribution of metal, and afford greater resistance against wheel pressure.

While we are engaged in these efforts of reforming the rail sections, we should begin by reducing the endless variety of patterns to a few standards of normal shape; this would not only save much outlay to the manufacturer in making new rolls and constantly changing them for every trifling order, but would avoid much confusion and simplify the labors of all concerned. The catalogue of the Cambria Iron Company alone contains forty-five different patterns for sixteen different weights. The 60-pound pattern alone has seventeen different shapes. Of all these numbers there must be one that is better than all the rest, and if we can determine which one it is, by all means let us have it.

D. J. Whittemore, Past President Am. Soc. C. E.—In partial answer to the discussion of the author's paper on "Cylindrical Wheels and Flat

Topped Rails". by our worthy President, I feel impelled to say that his deductions fortify me in the statement I made that "car wheels and rails to the value of nearly a billion of dollars pass into the scrap heap every few years on this continent alone." It was this fact that in the main prompted me to dictate my paper.

By the words "every few years" my evident meaning was to embrace in this period their service as wheels and rails before they went to the scrap heap, be the same five or fifteen years. At the time my paper was written wheels and rails were worth $30 per ton in Chicago, and this would make the value of wheels and rails on this continent, as estimated by our President, $975 000 000, approximating closely to the author's statement. I did not endeavor to show annual depreciation or the value of the scrap heap, but left that for each member to infer what it was as his judgment might warrant. This the honorable President has done in a manner satisfactory to himself; whether it be so to our members remains for them to determine.

The author is pleased to learn that our President advocates wider heads and flatter top.

I have no doubt that the President will agree with me that many of our rails of forty years ago had a top of from 4 to 5 inches radius. When they were worn out this curve approximated a radius of 6 inches, many of the second generation of rails were then formed with a 6-inch radius, the third came out of the track having a radius of 8 inches, the fourth with about ten, and now the fifth is generally designed with a 12-inch radius. Many exceptions, of course, can be cited, but this has been the general rule. By referring to the diagrams of tangent rail wear contained in the Preliminary Report of the Committee on the relation of wheels and rails to each other, it will be seen that the average radius of worn rail tops of the Pennsylvania Railway have been beaten down from a radius of not more than 12 inches to over 14. Do these facts mean anything? With the constant tendency to flatten tops, why not make it flat at once, and then demonstrate whether a large portion of the rail head will remain so or not?

The amplitude of the oscillation of wheels and rails will, of course, demand corner curves, and it is plain to see that these curves will be of a spiral nature, as is well shown by the diagrams I have above cited, and which curve can be approximately described by commencing about $\frac{4}{10}$ths of an inch from the perpendicular of the side of rail with a curve of $\frac{3}{4}$-inch radius extending through 12 or 15 degrees, thence compounding to a curve of $\frac{1}{4}$-inch radius, through the balance to the perpendicular.

Of course, with wheel-treads already worn hollow on round-headed rails, the change to a flat head, and to maintain it practically flat, becomes difficult, but not in my opinion entirely impracticable in time through general adoption.

In regard to tire wear, our President evidently attaches but little or

no value to the facts the author presents. That it is reasonable to suppose the material in steel tires may vary somewhat, as to its elastic limit, as is shown to be the case in rails, may well account for the great variation they give in tonnage service, but it seems to the author that the general averages he gives are worthy of consideration in support of the opinion he advances. Many of the sections of rails shown by the President indicate, and he unqualifiedly admits, flow of metal, the sure indication that the elastic limit is exceeded. In what other device does he or we attempt to subject metal to such strains, and if it is not proper, how can a change of practice be effected, presupposing, of course, that the wheel-loads will never be less than they are now.

Again, some of the wheels referred to in the table were undoubtedly such as had never been subject to brake-wear, as, for instance, the middle wheels of a six-wheel truck; and this fact may well account for varying results—upon which our President says he has precious little faith in the development of the author's formula giving the theoretical tonnage service of wheels.

In answer I desire to say that the formula is purely an arithmetical one, not empirical in any sense of the word, and while I believe I am the first to express wheel wear on a tonnage basis, I also feel prepared to say that the formula is as correct as that which is used to ascertain the tonnage service of rails. I presume, however, that the President means to take exceptions to the determinations derived from the formula, that the same are such as to leave him but precious little faith as to their value.

It is strange how differently we esteem these values and determinations. My argument is that the table in question shows that wheels running at or near the limit of elasticity and at the same time subject to the contingent wear from brake-shoes, slipping, etc., give us a tonnage service, for a wear of one-eighth of an inch, of from two and one-half to six and one-half times the tonnage service we secure from the two parallel rails of good quality as now designed, before they go to the scrap heap.

The decreased engine mileage compared with car mileage, which is cited, is directly attributable to heavy engine service, resulting in wonderfully decreased cost of transportation. There is no question about this. From this cause and fierce rival interest combined, lower rates have prévailed, dividends to stockholders are less, or are passed, and managers are now demanding of the engineer how this economy of train service can be perpetuated. To this end much will be accomplished when the rapid deterioration of rail is prevented.

In this connection we should bear in mind that from thirty to sixty overloaded freight car wheels pass over our rails to one engine-driver, hence the injury to rails can be attributed largely to freight service ; our

permitted loads on freight wheels being about double that of English and French practice.

A. M. Wellington, M. Am. Soc. C. E.—I have listened with very great interest and instruction to the paper by Mr. Whittemore. In regard to one subject I think that there can be no doubt that he is correct; that is, the enormous disproportion between the rail wear caused by the tires of the locomotives and by car wheels. According to the best available evidence, something like three-fourths of the rail wear results from the locomotive and only one-fourth from the much larger tonnage of the cars. That has been heretofore an unproven hypothesis, because it can only be determined in an indirect way; but the data which Mr. Whittemore has collated is exactly in accordance with what engineers who have studied the matter have concluded on that subject.

One point of difficulty that has occurred to me, among others, in listening to the paper, leads me to feel considerable doubts as to Mr. Whittemore's conclusions, viz.: the modulus of elasticity in a mass of metal loaded in the center by a weight which does not extend over its entire surface. I have myself, with a view of determining whether there was any permanent set from the locomotive, arranged to have a locomotive, the heaviest I could get, run over some rails and to be left there as long as possible. Now, if the quick passage of a locomotive over a rail produces permanent set, it seems logical that one whose weight continued to rest on one spot for from one hundred to two hundred hours would produce still more permanent set; but the most careful effort to find some sign of indentation failed. If it is true that the weight of a locomotive, in quickly passing over, produces in the rail large permanent set, then standing on the rail from a day to a week should produce more permanent set, sufficient at least to be detected by the reflection of a ray of light; but such is not the case.

On the other hand, it is entirely feasible to explain that there is, from a locomotive running at speed, a great deal of rubbing friction; from lateral motion or the lurching of the train from one side to the other, as also from unequal diameters of wheels, etc. I think it has been practically proven by comparison with the wear which takes place on curves and on tangents, that all the tangent wear that occurs can be accounted for as caused by abrasion only. If it were true that the instantaneously applied weights produced permanent set, some of our rails which stand a great many such loads a minute would be worn away and could not stand for any length of time, whereas some rails do last fifteen or twenty years.

Mr. Whittemore.—In my investigations of this subject some twelve years ago, I did not have the benefit of Dr. Grashof's analysis; but pursuing nearly the same method of reasoning that he does, I arrived at nearly the same results as his formulas give. From the area of the con-

tact of a 70-inch tire under a load of from 13 000 to 16 000 pounds, the compression of both tire and rail combined did not show over about $\frac{2}{1000}$ of an inch; but the more I have thought of the matter, the more I am convinced that the material of rail and wheel were pressed to beyond their elastic limit.

Relative to the modulus of elasticity used in the application of Dr. Grashof's formula, I wish to remark that the greatest variation we can allow from the knowledge we have, when applied to the rails of which tests are given, will be between 27 000 000 and 31 000 000, and by taking either one of these values, but little difference will be found in the results given in my table.

Mr. Wellington.—Has Mr. Whittemore ever made the experiment of taking a block of Bessemer steel, planing it off flat, and then taking a smaller rounded block, like the tread of a wheel, and applying successive pressures to it until a permanent set is produced, and then determining what pressure per square inch was necessary to produce it?

Mr. Whittemore.—I have not made such tests.

Mr. Wellington.—In questions of such moment it seems to me that we cannot rely on mathematical analysis for conclusions; we all know how liable purely mathematical analyses are to fail under test. We can deduce general results from specific experiments by their aid, but it is somewhat unsafe, if the formulas do not rest on experiment, to attempt any generalization on their evidence alone. Do Professor Grashof's deductions rest on experiment?

Mr. Whittemore.—Dr. Grashof distinctly says the modulus of elasticity and of resistance must be first determined by experiment, and this having been determined, his formulas apply to their several cases. In the absence of these values, I have applied moduli as usually determined, as the best testimony we can offer in the absence of further tests.

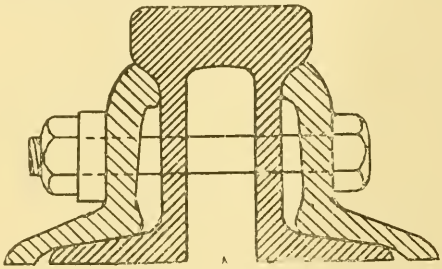
I wish to say a word in regard to aluminum. The refined analysis of steel given in my paper shows that it contains $\frac{1}{1000}$ per cent. of that metal, and if at moderate cost we can add about nine times this amount, it would be of great advantage, provided its benefit is anything like what is claimed for alloys of that kind. I feel sure that aluminum steels of this grade can be furnished for from $5 to $6 per ton over the price of steel, in the belief that we will soon be able to supply aluminum for $3 or a trifle less per pound.

G. B. Hazlehurst, M. Am. Soc. C. E.—I have for sometime past held identical views with Mr. Whittemore on this subject, which he has so fully and forcibly brought to the attention of our engineers.

Any engineer who has had any thing to do with "Strength of Materials" can not but see the correctness of the statements brought forward by Mr. Whittemore, and why they should have been ignored so long by sensible men, it is hard to see.

I am decidedly in favor of a flat top rail of such proportions that the bearing area will only produce stress within the elastic resistance of the material used.

I have advocated this theory on our road for two years past, whenever the question of a new rail section has been discussed; and I go so far as to believe that the day is not far distant, when, to secure a rail of sufficient bearing area, some such section as shown by the sketch below will come into use for the two-fold purpose of increased bearing between wheel and rail, and between tie and rail.



Additional splice of convenient form to be used.

Mr. J. T. Richards, C. E.—It is hardly within the province of a civil engineer to remark very much on the shape of wheels.

As for the rail, it is my opinion that the top should be nearly flat. I very much doubt, however, if an absolutely flat top rail can be rolled satisfactorily.

I am firmly convinced that there should not be more than a slight convexity—say a radius of 12 inches.

Mr. W. S. G. Baker (Prest. Balto. Car Wheel Co.)—When we began the manufacture of cast chilled car wheels, from an engineering standpoint we believed a cylindrical wheel the best form adapted for steam railway service, and so made all such wheels. As there seemed to be a prejudice on the part of railway officials to such treads, and while they favored those with cone shape, of necessity we were compelled to meet the prevailing idea.

Having no practical way to form an opinion as to relative value of the two forms of tread in their relation to rail heads, we cannot give an opinion based upon experience, but should, however, as engineers, pronounce in favor of the cylindrical tread and flat head to rail, as most likely to give increased life to both.

The first five lines of Mr. Whittemore's paper are interesting, and, no doubt, true as far as the engineers are concerned, but while they are exercising their genius to secure safety and make economy of train service possible, may it not be that their efforts are counteracted by the purchasing agent, who, from his point of view, believes he also is

accomplishing the same economy; who, from a commercial standpoint, buys cheaply—in fact he purchases his wheels per ton for about what proper metal to make such articles costs—thus causing the wheel-maker to undo what the engineer endeavors to accomplish, as his genius is taxed to produce a commercial wheel and not an engineering one—a wheel that can be made without pecuniary loss to himself, which will be accepted.

Within the last few years some of the railroads have formulated specifications based upon a physical test, and have increased the weight of chilled wheels to meet increased weight of cars and carrying capacity —in some cases as much as 100 pounds.

It would seem that this increased weight would insure increased strength, but in many cases it does not. Having recently tested wheels from eighteen different makers, we were amazed to find of what poor material wheels which will stand the specified requirements can be made, showing conclusively that the tests are not what they should be to secure proper work. Our test also demonstrated that the additional weight allowed in structure has not increased its value for strength, notably so with one maker's work, whose recently-made wheels, weighing from 555 to 575 pounds, stood an average of $1\frac{1}{2}$ blows to crack, and $4\frac{3}{4}$ blows to break under drop, while old wheels, by same maker, weighing less than 500 pounds, made some eight or ten years ago, and after many years service under cars, stood under same drop an average of $7\frac{1}{2}$ blows to crack and 25 blows to break. From our experience it is reasonable to claim that had the same stock been in the heavier structures as was in the lighter ones, with additional weight properly distributed, the heavier wheels would have stood from 40 to 50 blows to break them, and could have been made with deeper and better chill, prolonging their life, and probably giving less wear upon rail heads, as they would retain their contour and better preserve their relations with the rail heads.

If the good offices of the civil engineers could be utilized in bringing wheel-making to a higher standard of excellence, I have no doubt railroad shareholders would be benefited, and the road secure a safer and cheaper service.

Thomas Doane, M. Am. Soc. C. E.—A car wheel must necessarily have some lateral motion. It is given to it, to a greater or less extent, by more or less loose gauge, by all engineers, in construction. If not given in the beginning, it will acquire it very soon, by wear of rails and flanges.

The central portion of the wheel tread, whether cylindrical or conical, will get the most wear, and the tread will be worn hollow. I do not see how this can be avoided, even if running only upon flat top rails.

The consequence will be, with a hollow tread wheel, to carry the

weight of the load upon the inner and outer edges of an absolutely flat top rail, or which is far worse, the whole weight first upon the inner and then upon the outer edge of the rail. The heads of rails must be very strong to stand up under such treatment.

Such a consequence will also rock the rail upon its tie, and loosen it there, and also have an injurious effect upon the stem of the rail, bending it back and forth, as wheels carry their loads alternately on the inside and outside of the rails.

My own preference is for a rail top of such contour as to fit the average hollow tread wheel. Possibly this cannot be ascertained except by wearing wheels upon flat top rails. I think, however, it would result in a rail top very considerably removed from a flat top.

W. Howard White, M. Am. Soc. C. E.—On reading over Mr. Whittemore's paper I notice the following, which seems to need comment:

On page 150 of Mr. Whittemore's discussion of the rail head question he says: "Giving these sections the curve in the head that they originally possessed, and with the web projecting into it, can we devise a better form for splitting the rail from the weight imposed by the wheel than is here presented?"

It would seem as if Mr. Whittemore had forgotten a practical illustration of this particular action, with which he must have been familiar a good many years ago.

If in those days he had been told to make a stake for driving into hard ground, I hardly think he would have made the stake flat-topped, as it is a matter of such notoriety that the stake with the tapered head is the one that stands driving best.

Although I quite agree with Mr. Whittemore as to the desirability of a wide bearing for the wheel, and believe that the flat head is about the only practical way to attain that object, the argument adduced seems to me to make in favor of the other side of the question.

With a round-headed rail the overhanging sides of the head are less exposed to splitting off than with any other form.

We have only to imagine a rail with a concave head to see how much the splitting-off action of the wheel would be intensified by any flattening of the head.

Lest this should be considered a fanciful presentation of the case, it is necessary to call to mind the fact that every wheel tends to wear more or less double flanged.

Now, even if the rails were originally wide and flat, there must arise from side wear more or less narrowing of the head, to which the wheel wear will conform itself.

When a wheel so worn runs over a new unworn flat headed rail, the bearing would be on the outer edges of the rail head and the effect would be the same as with the supposed concave head.

Mr. Whittemore is evidently quite sure that there can be no such thing as fibrous interlocking of two iron surfaces, but I cannot share his skepticism until experiments on friction accompanied by microscopic examinations of the surfaces have shown that there is not a cellular formation in iron, the walls of which, when worn, may form microscopic ridges and points, bent one way or the other by the rubbing of another surface.

I have already suggested such an action as this as a possible solution of what has been ascertained by observation on German railroads, namely, the greater wear per ton of traffic in rails run over both ways as compared with those upon which the business is all one way.

It is to be hoped that experiments will be made upon this point in some of the physical laboratories of the country, and also that some great road with both single and double track lines will institute observations to confirm or disprove the German results.

Mr. Whittemore.—Mr. White quite mistakes my reference to that singular sentence in the Preliminary Report of the Committee on the relation of wheels and rails to each other; to wit: "Molecular interlocking of the fibers." The committee evidently refer to cast-iron or steel surfaces in contact and not iron surfaces as Mr. White asserts. Molecular, I need not say, is a chemical term, and any expression that implies molecular change certainly implies a chemical one also. Cast-iron and steel are generally considered crystalline in structure; hence I conclude that both the terms molecular and fibrous were misused in the sentence referred to.

Wilson Crosby, M. Am. Soc. C. E.—With reference to the form of the rail head, I approve of the proposed flat top with small radius at the corners.

Instead of having the head so broad, as proposed, I should prefer a somewhat narrower one both for the purpose of diminishing the train resistance in cases when there was any tendency of the wheel to run either to or from flange, and also to thereby save metal to be added to the depth of the head, and thus, by its greater depth and the lessened length of the cantilevers, strengthen the top against the tendency of the sides to break off as indicated in the paper, also to oppose a greater amount of metal to be worn down.

In the wheel, I approve of the general form of tread shown in Fig. 2, Plate XLIX of the paper under discussion, but would limit the length of the flat portion to somewhat less than the width of the rail head in order that as the tread of the wheel wore it might not develop a groove near the flange.

F. M. Wilder, M. Am. Soc. C. E.—As a whole, this subject is one which should be very fully considered by those responsible for the engineering and mechanical departments of our railroads.

I believe that the tendency in railroad construction is toward too heavy weights upon the track at a single point. While Superintendent of the Motive Power of the New York, Lake Erie & Western Railroad, our limit was 13 000 pounds upon any one wheel, and in order to get greater adhesion and consequently greater tractive power, we built "Mogul" passenger locomotives, in order to get a better distribution of the weight of the locomotives and still have it on the driving wheels.

Mr. Whittemore very clearly states his proposition that we should use cylindrical wheels and flat topped rails and I fully agree with him.

In reference to the coning of the wheel having any considerable effect in assisting the truck to curve, I would say, that I do not believe it has, but that when wheels are held rigidly upon an axle and their axis is held parallel with another pair, as in a truck to a car or locomotive, the truck will roll forward on a tangent even though one wheel is much larger than the other, unless forced to an angle by some obstruction like the flange pressing against the rail, and if the axles are radial to each other, the wheel will endeavor to follow a curve proportioned to the angle of the radii.

I have experimented with a small truck on a plane and demonstrated this fact, even showing that the small wheel will take the outside of the curve if they are set wider apart than the larger ones. This fully shows that the coning of the wheel does not cut any figure in cars curving easily, but that having the trucks built so that the axles are exactly parallel is a matter of the greatest importance.

The coned wheel on a round topped rail only has a point for a bearing, consequently the only way to increase the bearing surface is to lengthen that point to a line.

The experiments mentioned on page 142 gives $1\frac{7}{100}$ square inches as the area of the points of contact as shown between coned wheels and round top of rail for 72-inch wheel, and $\frac{86}{100}$ths as the area of contact for 62-inch wheel.

The distribution of weight on this surface of contact is unequal, being greater in the center. Had the experiments or measurements been made with new wheels and rail instead of these which were "much worn," the area of contact would have been an oval with major axis much longer and the minor axis not more than three-eighths of an inch instead of nearly an inch, thus raising the rate of pressure and showing greater distress to the rail at the center of the point of contact. The effect of cylindrical wheels and flat topped rails, would be to widen the theoretical point of contact to a line and the point of actual contact would be shortened lengthwise with the rail, and would probably show a greater area of bearing than by the experiments mentioned.

Mr. C. P. Sandberg.—Allow me first to refer to my own paper, just published by the Institution of Civil Engineers here, on "The Use of

Heavier Rails,"* in which it is said that my Goliath rail, adopted on the Belgian State Railways, is laid perpendicularly, and I may add that this was done at the recommendation of the engineer-in-chief of the rolling stock, Mr. Belpaire, who actually favors cylindrical wheels, but with the large rolling stock of the Belgian State Railways, conversion would take some time. I am not aware that the conversion has even been begun, and nowhere else do I know of cylindrical wheels, or flat top rails being used in Europe, but they get flat enough in time, and I shall have no difficulty in proving why rails are not made flat from the commencement.

First, from the producer's point of view. Any one who is acquainted with the manufacture of rails will agree that the flat top rail necessitates less pressure or work in the last groove than a round top rail, and the more round it is, the more pressure or work or closer grain in the head will be the result. The joint of top and bottom rolls, being in the middle of the head with a small opening, if the pressure or work is excessive, would leave a seam along the head in the joint between the top and bottom roll; besides, the more round the head of the section is, the better delivery would the rail have from the rolls, or the less number of wasters. Thus, it follows, that the flat topped rail would have a more porous head than the round one, and more wasters, and as a very short time in the track will make it flat enough, it would be an absolute loss, both to the consumer and the producer, to start with the flat top. See drawing attached, Plate LXIV.

Again, from the consumer's point of view. The flat top rail can never be laid in a track absolutely correctly so as to have the tire of the wheel bear on all parts of the flat surface, but it will bear on the left-hand corner on one rail, and on the right-hand corner on the next; and after a few trains have passed the line will look like a snake; therefore, the evil from the round topped rail of small bearing surface would be still worse in practice on the flat topped rail before it is worn down sufficiently to bear on the neighboring rails alike. Thus, in practice, Mr. Whittemore's suggested remedy would bring matters from bad to worse both for makers and engineers.

Now, as regards the radius. I have kept to a simple radius of 6 inches for the above reasons, but I am perfectly willing to increase it to 10 or 12, since steel rolls so much better than iron, and the delivery would be good enough with even 12 inches. After all it is of very little importance whether it is 6, 10 or 12 inch radius, as long as the top is not flat, and Mr. Whittemore's prediction that my sections would be a thing of the past is entirely premature; but I believe his flat topped rail will never become a standard section, nothwithstanding the elaborate theories of German professors, and his own poetry; besides, the thing is not new.

* Proceedings Institution of Civil Engineers, Vol. XCV.

Plate LXIV shows a section which I had to inspect in 1869, at the Dowlais Works, for the New Orleans, Mobile & Chattanooga Railway, 16 000 tons rolled thus in iron twenty years ago, and I well remember the heavy rejection we had on account of the flatness of the rail top, and I am very glad to say that I never had one like it afterwards. I am sure no one could have wished more than I that the complaints generally heard now that the rails are not as good as those made twenty years ago, could have been remedied by such a trifling alteration in the section as making the top flat, but I think the above facts show the reason that that will not do it, but simply make matters worse. The increased pressure of the heavier engines necessitates harder steel both physically and chemically, and in this direction a remedy must be looked for. Now, as the physical hardness or what is produced by cold rolling (same as cold hammering) is not obtainable in the fast going mills used at the present day, chemical hardness or excess of carbon must be looked to as the simplest remedy, and as such hardness involves greater liability to fracture, it shows an absolute necessity of increasing the weight and the section at the same time as the hardness, if accident from broken rails should be avoided. This has been the motive of designing my Goliath rail, and my work in this direction. I am very glad to see that it is fast being adopted on both sides of the Atlantic. The reproduction of my 100-pound section in Mr. Whittemore's paper is somewhat disfigured, and to correct this in the Transaction I inclose a correct drawing of it.*

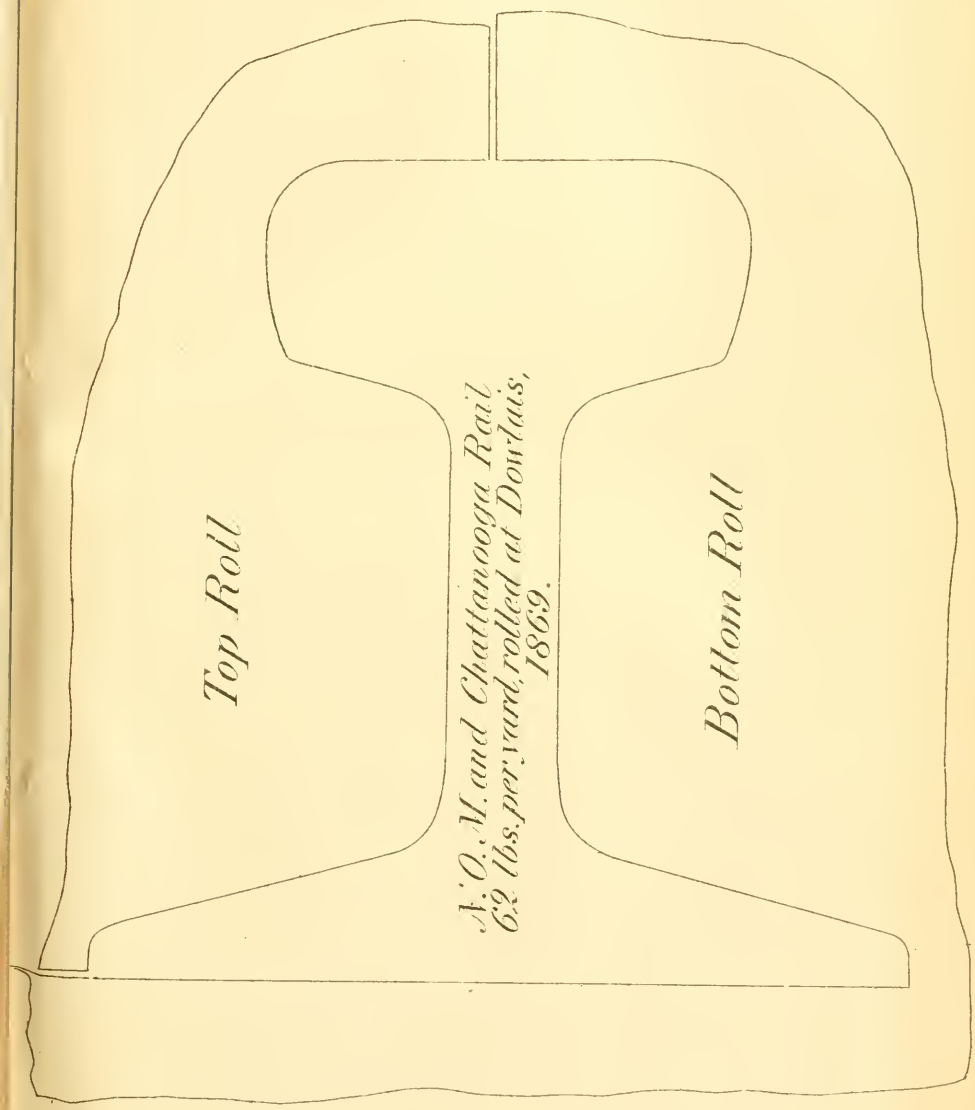
Fortunately the price of steel is being gradually reduced, which will facilitate such change with the object of securing perfect safety with more economy, and a possibility of the maker making what is wanted.

Mr. Whittemore's paper having been so ably dealt with in the *Engineering News* by Mr. A. M. Wellington, M. Am. Soc. C. E., I have nothing more to add except that I believe that it would lead to great advantage if makers had a voice in the discussion of the rail question, and I find a total absence of such, not only in this but in the general treating of the rail subject in the United States. For, after all, the possibility of making a rail is the first, and the use of it the second question; and I believe much ink might be saved if practical makers would come forward and give their opinion frankly upon the engineer's scheme.

Mr. Whittemore.—When the author dictated his paper he expected his position would be opposed by many who had much to answer for in giving us the forms of rails in general use in this and other countries, and therefore he is not at all surprised at the position taken by Mr. Sandberg.

In no other device, except in the use of rails, wheels (and of the

* Plate LXV is a reproduction of this drawing. The top line, however, is much too flat for the indicated radius of 6 inches. [Ed.]


Top Roll
N. O. M. and Chattanooga Rail,
62 lbs. per yard, rolled at Dowlais,
1869.
Bottom Roll

50 tons Engine
50 miles pr. hour.

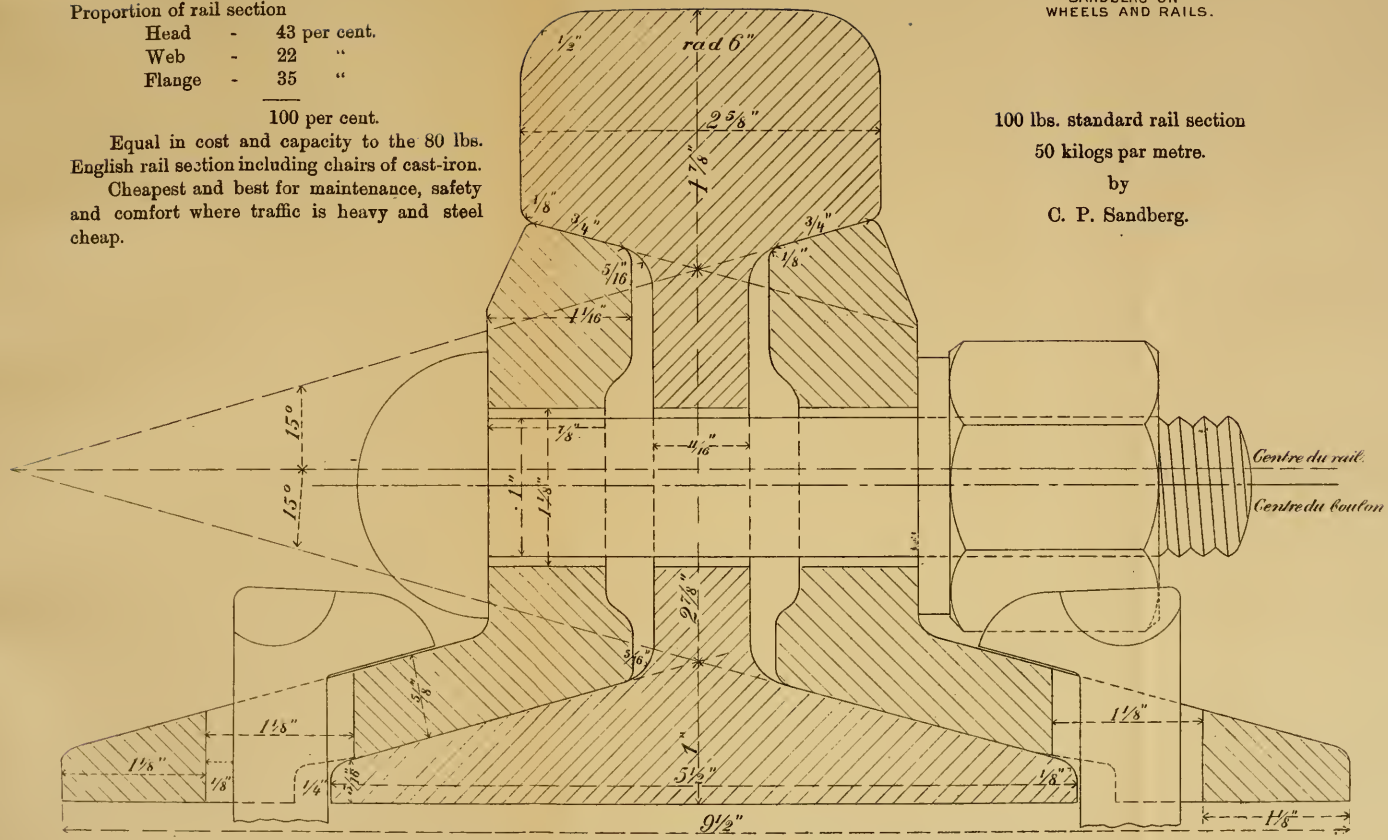
Proportion of rail section

Head	-	43 per cent.
Web	-	22 "
Flange	-	35 "
		100 per cent.

Equal in cost and capacity to the 80 lbs. English rail section including chairs of cast-iron.

Cheapest and best for maintenance, safety and comfort where traffic is heavy and steel cheap.

100 lbs. standard rail section
50 kilogs par metre.
by
C. P. Sandberg.



Extracted from the proceedings of the Institution of Civil Engineers.

grindstone, and in the emery wheel and like appliances), does the civil or mechanical engineer subject material to much beyond one-half of its elastic limit, and I respectfully ask, where, in his discussion, he in any way attempts to state how this can be done, provided we must impose the loads we do on wheels, except by providing a broad contact between cylindrical wheels and flat topped rails. As to the difficulty of rolling such rails, that is a matter that must be accomplished by the rolling mill managers. As I have said before, a way can always be found to do that which must be done.

The difficulty he has experienced in having rails lie properly on ties is an easy matter to overcome, and at very slight expense.

In building the first railway with which the author was connected, all the ties were machine-dressed at the spot where the ties came in contact with the rail. It would be economy to do this now, whether the rails be flat or curved on the top.

Mr. Sandberg refers to a flat topped rail, the manufacture of which he inspected some twenty years ago, and he is happy to say that he has had none of the pattern to inspect since.

The author is not surprised to learn that a flat topped rail of the Brunel pattern was laid on the Great Southern and Western Railway of Ireland, so long ago as 1843–44. Some of this is still in sidings on that line and capable of standing considerable tonnage service.

The author is pleasingly surprised and gratified to learn through his friend, M. Le Rond, Engineer of the Northern Department of France, that a rail is now adopted embracing precisely the claim of the author; not, however, in degree as regards width of head, as that is not necessary since their carriage wheel-loads never exceed 6 000 pounds.

A sketch of this rail in cross-section and the specifications under which it is manufactured are so pertinent to the subject that a copy of the former, Plate LXX, and a translation of the latter are printed as an addenda to this discussion.

We must bear in mind that from forty to sixty overloaded car wheels pass over the rails to one locomotive driver, hence, the great injury resulting to rails can be attributed to car wheels.

The slighting notice given to Professor Grashof's deductions by Mr. Sandberg, I shall not attempt to answer, other than to state that I conceive his formula is proved safe by experience when the limit of elasticity, as usually determined, is entered in his formula at one-half its value. With a less margin of safety I have knowledge of repeated failures to bridge rollers and wheels under draw bridges. Mr. Sandberg says that it is immaterial whether the rail has a curved top of 6, 8 or 12 inch radius—that it will get flat enough in time, etc. Of course, the rails do get flattened through the struggle they have with the wheel, and in this warfare the wheel is being made hollow in the tread, both of which disastrous operations it is the object of my paper to correct.

In designing all our structures (except perhaps rails) the engineer is expected to be guided by reason as to size and forms of their different parts; but in this instance Mr. Sandberg asserts that it is immaterial whether a radius of 6 or 12 inches is used in forming a surface over which the bulk of the land tonnage service of the civilized world passes. Is this true engineering?

In regard to the statement that my representation of the Sandberg pattern of rail is not correct, I have only to say that it was intended to reproduce it from the sketch and dimensions given by Mr. Sandberg in his paper as published in Vol. 84, page 395, Transactions of the Institute of Civil Engineers.

With Mr. Sandberg, "I believe much ink might be saved if practical makers would come forward and give their opinion frankly upon the engineer's scheme." It seems to me that I have burned a deal of powder to blow over a feather; that when real thought, logical reasoning and common sense, all lead directly in one direction, there can be no question of what is necessary, and if we have not the appliances or methods for meeting the conditions the problem engenders, it is the duty of the engineer, whether he be a theorist or a manufacturer, to meet and solve the problem at once.

I am not one to willingly admit that the engineer, in his profession, should be considered the impracticable factor behind or before the manufacturer. The one should and will supplement the other, and what true engineering demands will be met by the manufacturer in so far as human ingenuity will permit.

E. T. D. Myers, M. Am. Soc. C. E.—I have read with much interest Mr. Whittemore's paper on wheels and rail heads. Although I have been forty years engaged in engineering, most of the time upon railroads, and have seen the fashions of rail sections vary almost as much as those of apparel, I am not competent to say much upon the subject. First rate material will, I think, give satisfactory results in almost any of the forms which have prevailed. Yet I will go so far as to say that I prefer a top broad and nearly flat, if not absolutely so, and I do not adhere to the inclined sides of the rail head.

G. Bouscaren, M. Am. Soc. C. E.—Using without abusing is a dictate of common sense, it is the exercise or application of a power within certain defined limits, beyond which it loses the faculty of being repeated or re-used in the same manner; going beyond these limits being tantamount to destruction or waste.

Hence, if economy of power is the first object of the engineer, no greater professional sin can be committed by him than to tax materials beyond their limit of elasticity.

We often err by force of habit, dating from early days, when perhaps the same position was perfectly legitimate; by the process of evolution,

use may become abuse; such seems to be the case with regard to the present relations of rails to wheel-loads.

As long as the loads were kept within the limits of 2½ tons for car wheels and 5 tons for engine driving wheels, the rail heads appeared to be well enough adapted to the purpose for which they were intended; although the fact, that the round shape of the head was productive of increased wear of rails and wheels, was even then recognized, is shown by the gradual changes observable both in Europe and America toward a flatter top; but now that we have been led, through the necessity of reducing the cost of transportation, to increase these loads to 5 tons for car wheels and 10 tons for engine driving wheels, with a tendency to a still greater increase, it is evident, from the facts presented by our honorable Past President:

First.—That we are abusing the rail head and wheel-tread by subjecting the metals thereof to repeated pressures beyond their limits of elasticity.

Second.—That in so doing we are decreasing the length of rail and wheel services, which are both important factors in the cost of transportation; that an increase in wear means an increase in frictional resistances, which is also an important factor in the cost of transportation.

Third.—That as a necessary corollary of the above, we are losing through the increased wear and tear of rails and wheels and loss of power, a considerable part of the advantages sought to be gained by the increase in the carrying capacity of our cars and in the hauling capacity of our engines.

If these premises are correct, we cannot apply the remedy too promptly, and if the metals of the rails and wheels are to remain the same, the only remedy is obviously an increase in the bearing surface between the two.

I would not say absolutely with Mr. Whittemore "cylindrical wheels on flat topped rails," because, if by "flat topped" is meant (as illustrated in Fig. 2, Plate XLIX, showing the suggested arrangement) a plane surface, it will not give in normal running conditions the greatest bearing surface; this maximum surface of contact can only be secured by giving to the rail top the shape that it will take ultimately after a prolonged service; this shape must necessarily vary on different roads, according to the depth of grooves allowable on the engine driving wheels and car wheels, but will always be a slightly curved surface, until the general use of equalizing brake shoes, which prevents the formation of the grooves and maintains the cylindrical shape of the wheel-tread, shall gradually cause the flattening of the rail top into a plane surface in normal running conditions.

Mr. Whittemore has specifically limited his inquiry to the influence of the shape of the head; although I cheerfully recognize the impor-

tance of the shape and size in that most indispensable of all organs, it is, I think, also generally acknowledged that the quality of the substance inclosed therein should not be overlooked, and it may perhaps be best in a study of the survival of the fittest, not to start out by separating the head from the body.

If the question before us is how to design and construct a steel rail better adapted to the altered conditions of American railway traffic than the short lived rails now so extensively used in the United States, I think the shape of the rail, its weight and the quality of the metal are three factors of about equal importance, and so closely related and co-dependent on each other as to be inseparable in any consideration of the subject aiming to practical results.

If it is true that the bearing surfaces between rail and wheel are entirely inadequate to the present wheel loads, it is, I think, equally true that the steel of the rail is generally too soft and open-grained through an insufficiency of carbon and mechanical work. In the early days of steel rails they were made much harder in imitation of what was being done in Europe and they gave much better service. A very large proportion of the 52-pound rail, laid on the Cincinnati Southern Railway, when it was constructed in 1876, is still in the track and shows less sign of "flowing" and crushing in the head than some of the 60-pound steel by which it has been partly replaced, although the head of the 60-pound rail is fully ⅛ of an inch wider; manifestly this result can only be ascribed to the superior hardness of the 52-pound rail.

It may appear singular that as the wheel-load has increased, the metal of the rail should have been made softer instead of harder; this is explained by the widespread confidence placed in Dr. Dudley's theory as to the superior wearing quality of soft rails, for several years after the publication of his paper, and also by the fact that the early hard rails were found to be too brittle, many of them breaking in the track and causing innumerable accidents; this was the case with the 52-pound steel laid on the Cincinnati Southern Railway; hence the remedy sought in the use of a softer metal containing from 0.30 to 0.35 of carbon instead of 0.40 to 0.45.

To make matters worse, contemporaneous to this change, happened several "improvements" in the manufacture of rails, whereby a less number of passages through the rolls and a high temperature of rolling so reduced the cost of production as to place it within the reach of the most fastidious railway manager whose practical economy prefers pot metal at $30 to good steel at $33.

Although a strong partisan of the larger bearing surface, advocated by Mr. Whittemore, I earnestly believe that the flowing and disintegrating of metal, and the coring and splitting of heads observed by him and many others, myself included, is principally attributable to the bad quality of the steel. I think that the remedy is to be found, not only

in cylindrical wheels on flat topped rails, but in a return also to a harder steel, sufficiently rolled at the proper temperature.

To avoid brittleness, as well as for a due regard to economy in maintenance, the rails should be made much heavier.

My experience is, that, with very few exceptions, the breaking of steel rails in the track begins at the flanges, this is an indication that the flanges should be made narrower and thicker.

In the paper of Mr. Caillé (*Notes sur l'acier à rails et sur la durée des rails d'acier—Comptesrendus de la Société des Ingénieurs Civils, Année,* 1886, 2me Vol.) on the bearing qualities of two different makes of rail on the Paris and Orleans Railway, the analyses given of these rails, show the proportion of carbon to have been 0.6 to 0.7; the wear had been 8 millimeters in twenty-one years' service, and the author estimated that they were good for ten years more; these rails weighed 76 pounds per yard and did not break in the track, but they were double-headed rails.

I do not think that it would be advisable to use steel of this grade in a flange rail of the same weight, but I believe it is entirely practicable to make a heavier flange rail quite as durable and safe.

In a late paper read before the Institution of Civil Engineers, Mr. Sandberg points out the necessity for narrower and thicker flanges, and proposes the use of tie-plates to increase the bearing area of the flanges on the tie; I think well of this suggestion and believe that the increased life of the rails and ties would well repay for the cost of the tie-plates.

In conclusion, I would sum up as follows, the requirements considered to be necessary for a safe and durable rail, under the present conditions of wheel-loads and speed on American trunk lines:

First.—Weight—Not less than 80 pounds per yard; 100 pounds preferred.

Second.—Shape—Head—Flat topped (shape taken by the top surface of the head after a prolonged service) with vertical sides and sharp rounded corners.

Flanges—Not less than $\frac{3}{8}$ inches thick on edges. Quantity of metal in flanges equal to that in head, less a wear of $\frac{3}{8}$ or $\frac{1}{2}$ inch. Slant of flanges and head equal.

Third.—Metal—Close-grained, uniform and tough; well rolled, and finished at a dark red heat. Tensile strength not less than 105 000 pounds, not more than 125 000 pounds. Probable carbon 0.45 to 0.55.

Such rail should stand a traffic of not less than 20 000 000 tons for a wear of $\frac{1}{16}$ inch, and should give a life service of from 120 to 160 millions of tons.

The rail mills have always claimed the privilege of determining for the railway companies the quality of steel most suitable for their rails; I think this should be entirely satisfactory, provided the mills would also be willing to guarantee a rate of wear under service, below a reasonable maximum, say $\frac{1}{16}$ inch for 20 000 000 tons; but as long as they decline

this responsibility, it is nothing more than fair and just that they should work under specifications prepared by engineers.

Mr. George Gibbs, Mech. Engineer.—Mr. Whittemore's very suggestive paper cannot fail to call attention—and none too soon, I believe—to the effect of the steadily increasing wheel-loads of modern cars and locomotives upon the wear of rails; by "wear," meaning the sum total of defects developed in service and not abrasion alone. The portion of the problem connected with car design is now receiving very careful attention from master car builders throughout the country, and the necessary strengthening process from the roof down to the wheel-tread is going on. But right here we are met by inflexible conditions, the contact surface offered us to-day by the modern rail head is substantially what it was fifteen years ago; the radius of the upper corner and slope to sides have increased or diminished according to varying dictates of fashion, now appearing to be on the return cycle towards small radii and vertical sides, but the available width of rail tops has undergone slight change.

It seems to be high time that rail and rolling stock designers considered this subject from a common point of view. Mr. Whittemore's figures show very clearly that the effect of increased load upon tires is not to be ignored, and there is no reason to suppose that rails enjoy immunity from punishment from the same cause. Some car designers are seriously considering the advisability of using a six-wheel truck for the high capacity freight cars coming in use, very largely on account of the impaired life of the wheels under the increased pressures entailed. I believe it is a demonstrable proposition that the life of our wheels is being appreciably shortened from this cause. Does not Mr. Whittemore's suggestion, then, offer a more direct solution of the problem?

I say, it can be proven that increased pressures cause increased wear, and I also believe we have proven that increased contact surface diminishes wear. In the discussion of your Committee's report on rail forms will be found a form of wheel designed by Mr. J. N. Barr, M. Am. Soc. C. E., and now largely in use on this road (Chicago, Milwaukee and St. Paul), in which the available contact surface of rail head is more fully utilized than in the usual form of conical tread. In this tread, it will be noticed that besides altering radii at throat, the main bearing portion is formed to a curve having the same radius as rail head—10 inches. In Mr. Barr's absence, I have not at hand figures for relative performance of these wheels with those having tread of usual design, but can state generally that results have been most satisfactory, which is confirmatory evidence in the direction we desired.

Now, in regard to Mr. Whittemore's proposition definitely; looking at it from the direction of its effect upon rolling stock, I heartily second the attempt to provide increased contact surface between wheels and rails by increased width and parallelism of surfaces, but am doubtful of

the effect of the particular method of securing it by flat tops and cylindrical treads having their contact in a horizontal plane, upon flange wear of our wheels. Car trucks are seldom found with perfect parallelism of frame and axles, so that the tendency in all trucks is to run more or less to one side. This is appreciably counteracted by either coning wheels, inclining the rails, or the form given throat of wheel, tending to equalize wear over whole tread by causing end oscillation of axle. Do we not remove a large factor in this desirable action by surfaces so disposed as to be in equilibrium in any position assumed? I am here supposing the extreme case of cylindrical wheels with the usual sharp fillet, ½ to ⅝-inch radius, at the throat. In Mr. Barr's wheel, coning is dispensed with, but an intermediate curve of 1½-inch radius is formed between throat and tread, which I believe supplies the needed condition for distribution of wear.

From an examination of a considerable number of plaster casts taken of rails in track and tracings of wheel-treads in all stages of wear, I am inclined to think that the contact area between the two very rapidly approximates the maximum allowed by width of rail head after the wheels are put into service, so the question of parallelism of surfaces very quickly solves itself.

I am, therefore, of the opinion that we should consider a rail having sharp corner curve, as wide a head as the weight of the section and safe rolling conditions will allow, and radius of top at least as great as 10 inches; wheel-tread to be modified in throat in some such manner as Mr. Barr suggests, the remainder of tread to be of same curve as rail top. In this manner we will preserve uniformity of wearing conditions under varying conditions of track surfacing, curvature, etc., in the best possible manner.

It may hardly be the place here to discuss the best section of rail throughout, but I believe that we can with advantage considerably modify the present proportions without adding to weight, if so desired, by adopting Mr. Whittemore's suggestion as to width of head, increasing height of rail and its strength consequently as a girder, adding little or nothing to web thickness, but equalizing amount of metal in head and foot by adding to foot, narrowing same to obtain good distribution of metal for rolling and favorable shrinkage conditions. Here we would have a rail which would leave the mill in the most perfect physical condition, and in the track would offer the least contact pressures with ample material for abrasion and would have sufficient stiffness as a girder to lessen the difficulty of keeping track in surface. With better surface the "hammer blow" would become a less important factor, harder material would be allowable and the percentage of rails removed for battered ends, less.

In the table Mr. Whittemore has given (page 148), showing results we have obtained from some analyses and physical tests of rails, the

first ten samples on list were from rails removed from track after only two to three years of service for excessive wear or crushing. In every case the photograph of etched section showed the undoubted cause of the wear; thus, in Plate L, an etching of sample 66*b*, will be noticed a zone of flattened blow-holes enveloping the entire section about ⅛ inch below the surface; these appear beautifully clear in the photograph, but not so well defined in the engraving. Sample 72*b* shows spongy section throughout, apparently from a frothy ingot; the high silicon would here appear to indicate imperfect conversion, as the physical test of rail shows cold-short rotten, rather than hard and brittle material. Almost the identical amount and distribution of hardeners in a different make of rail, and one made, I believe, by a different process, shown by 52*b*, produced most excellent physical results, and from the etched section and behavior of the metal under the tool in machine shop, I should predict excellent results in track.

Not to follow analysis of table further, it is apparent that the most variable chemical and physical characteristics prevail in steel rails, and that failures are many times to be explained by undoubted physical defects, defects certainly not traceable to design largely, but rather to some undetermined relation between chemical composition and physical treatment in the mill, a relation not clearly revealed to us by the ordinary chemical and physical tests which give reliable indication of the quality of other structural material.

Mr. Whittemore.—In regard to the form of wheel tread as designed by our member, Mr. J. N. Barr, cited by Mr. Gibbs, I have it from Mr. Barr that he so formed the tread of his wheels to best meet the blunder of the curved top rail; it was in his power to control the one and not the other in his own field of work, and it is my opinion that he has met the fault he did not control as thoroughly as he could. I have it from Mr. Barr, personally, that could we have flat topped rails he would have his wheels cylindrical.

L. L. Buck, M. Am. Soc. C. E.—If we do not see any tests of the efficacy of using cylindrical wheels and broad flat topped rail heads until there is a general movement of railroad companies in that direction, it is probable that we shall have a long time to wait. There is still a strong prejudice in favor of the coned wheel, and so long as coning continues in general use, a large part of the advantage of using the flat topped rail would be lost. Still it would prevent the destructive effect upon the rail head caused by the cylindrical driving wheels as illustrated in Mr. Whittemore's paper. Of course, a new coned wheel would not have much bearing upon it, nor would a badly worn wheel, and the consequence would be that they would, in a short time, wear broader bearing surfaces for themselves, and that in time they would round the top of the rail. Were the wheels cylindrical and the

heads of the rails broad and flat, the same result would take place, but not nearly as rapidly as with the coned wheels. There would always be a portion of the surface of the wheel which would wear more rapidly than any other part, thus forming a more or less shallow groove, which, in turn, would round the top of the rail. The amount of such wear would depend upon the proportion of such worn wheels to the new ones in use and upon the extent to which the wheels were permitted to wear before they were "trued up" or replaced. But it would be absolutely impossible to keep the tops of the rails perfectly flat.

If now there is a chance that the life of the rail would be increased by making the greater portion of the top of the head flat, there seems to be no reason why the experiment could not be tried now as well as at some future date. If some railroad company having occasion to order a large lot of rails, should ask for enough flat topped ones to lay several miles continuously, followed or preceded by the present form of head, I do not believe they would be taking any risk. The rails could not wear out any more rapidly, and it is reasonable to suppose they would not wear out as rapidly, for the reason that after having become rounded on top they would still be as good as the ordinary rail, and we should have the time it required to wear them to that condition as a nearly net gain.

One of the objections that I have heard used against the flat surface of the head is, that in rolling such a rail pieces of cinder or scale are liable to get between the rail head and the shoulders of the grooves of the rolls and cause cycloidal scratches on the top of the rail. I do not think, however, that this would be difficult to overcome.

Should the experimental piece of track show satisfactory results, it might encourage somebody to experiment on cylindrical wheels, and ascertain whether the loss due to the resistance on curves (if such resistance is thereby increased) was not more than compensated by increased life of rails and wheels.

Moses Burpee, M. Am. Soc. C. E.—I consider a wheel of which the greater portion of the tread, or say about 2½ inches, is cylindrical, a better one than the ordinary conical wheel. That the latter does not give entire satisfaction is evident from the present discussion. To discover the faults which underlie its failure to do so and to present a plan by which these faults may be eliminated is a duty which no engineer should shirk.

The benefits expected from the coning of wheels are in practice seldom realized, I believe, even in new wheels on good track, and still more seldom with worn wheels on either good or indifferent track, probably never when both wheels and track are in poor condition.

The wear which is certain to occur after a few months use is either to reduce the diameter near the flange to at least as small, if not a smaller one than that near the edge of the tread, in which case the tendency on

curves to press to the outer rail results in bringing as large, if not a larger, diameter on the inside and shorter rail, or the reverse of what is supposed to be obtained by coning the wheels.

While it is one thing to demonstrate that the practice of coning wheels is probably useless, it is, of course, another thing to show that there may be something gained by adopting the cylindrical wheel proposed by Mr. Whittemore.

I think it has been plainly shown by the diagrams made of worn wheels, with which all are familiar, that the object of the practice of coning is seldom attained and never for a long time held.

There can probably be no doubt that cylindrical wheels are much more liable to run in a straight line with less lateral pressure on either rail than conical ones are. To illustrate, suppose we have a set of new conical wheels on good track, just leaving a curve and with the flanges against the outer rail. The tendency is, as soon as it gets on tangent to run toward the other rail in order to bring the equal diameters of the pair in bearing. The momentum as well as the direction of the wheels will carry it beyond that point and reverse the order, which will also in turn reverse the motion, and a series of vibrations will be kept up for a length of time which depends much on the condition of track. I believe this motion exists on the best track in a slight degree, and on indifferent track to a greater degree, and is aggravated by every imperfection of line or level. If this is so, there must be a good deal of consequent flange friction and wear, as also a tendency to spread the gauge of the road, especially where soft wood ties are used. I believe, therefore, that there is a constant and growing tendency for conical wheels to injure the track they run upon, and that this tendency would be almost, if not entirely, eliminated by the use of cylindrical wheels, in so far as the lateral vibrations are concerned.

The objections to cylindrical wheels on account of inadaptability to curves, has, I think, not been shown to be greater than actually exists with conical wheels, that are even slightly worn, and, inasmuch, as by far the greater proportion of the mileage of the railways of the country is straight line, it must be better economy to provide for circumstances of the most frequent occurrence by what can be shown therein to work well, and fairly well at least in less favorable circumstances, than to sacrifice a good thing for the sake of one which has been supposed to work well in the less frequent, and tolerably in the more frequent, cases of ordinary practice, but which has been found wanting in either.

While I am satisfied that a cylindrical wheel as proposed by Mr. Whittemore would be an improvement, even on the ordinary rail now used, I believe its chief merit lies in the opening it gives, or would give, for the adoption of a flat top rail—thus securing much better conditions for wear, of both wheel and rail, as well as for adhesion.

I presume there would be but little difficulty in rolling such a section,

as it would be but little more than half the width of the flange, which is at least nearly, if not quite, flat, and would favor as large a radius for the crown of rail as possible, should a perfectly flat one be impracticable in manufacture.

WATERMAN STONE, Assoc. Am. Soc. C. E.—I have not quite outgrown the effects of my early education as to the coning of wheels, and I am of the opinion that rails rolled with a top radius of 11 or 12 inches are to be preferred to those with a flat top. While I deprecate the multiplication of rail patterns, I hope that some road will be found with sufficient courage to give Mr. Whittemore's plans the test of experience.

E. PONTZEN, Cor. M. Am. Soc. C. E.—In some remarks on the report of the Committee on form of rails and wheels I said: "I would admit the 12-inch radius or even a flatter top, but only if the tread of the wheels is cylindrical."

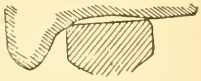
I said further "the flatter top reduces the chances of producing grooves in the tread."

On these points I am of the opinion of Mr. Whittemore as regards the top of the rail. Like him, I am afraid of the "out-squeezing" of the top of rails, what he named the outflow of the metal; and for that reason I had wished to avoid bringing the bearing of the wheel too near to the lateral face of the rail. But I agree with Mr. Whittemore in considering the increased surface of contact between wheel and rail as very useful in this respect.

The efficiency of conical wheel treads, and, as a consequence of it, of a rail top not flat, for the passage of wheels through curves, is absolute as long as we consider a single axle; but when two axles are connected, the advantages of the cones are diminished, and this the more as the distance of the axles increases.

The wear and tear of rails and wheels results from two causes: the rolling motion under too high pressures and the friction. With flat rail tops and cylindrical wheels the first will be much reduced, but at once the second will increase, as in curves cylinders of the same diameter will have to make longer runs on the outside rail than on the inside rail. The axles of the wheels will have to support the increased torsion stress, which changes direction with the direction of the curves.

When the wheel treads are grooved, the contact face between rail and wheel is increased and the advantages of conical wheel tread disappear, the first cause of wear will diminish, the second increase. But the conditions will not be then quite the same as when the wheels should be cylindrical and the rail top flat. The groove will come in aid to the flanges; but it is of no great use, and I mention it only.



Another difference is of more importance, as long as the flat top rails

are not of general use, and as long as grooved wheels will therefore run on the lines which will have introduced the flat top rail, the bearing of the grooved rails will be entirely on the edges of the rail head of the flat top. This drawback would be avoided with rails not having a flat-top. I am also afraid that the rolling of flat tops would present some difficulties.

In conclusion I would say that cylindrical wheels and rails with curved top of more than 12 inches radius, say 15 inches or more, would prepare for the future a good way and prevent for the present the danger to which flat top rails are exposed by grooved wheels.

I believe that Mr. Whittemore proposed only the Strickland or Brunel rail, in view of the bad effect of grooved wheels on the Stevens or other single flange rail. In my mind we will never return to the U rail.

A. Gottlieb, M. Am. Soc. C. E.—I fully agree with the author that a larger surface contact between wheels and rail would reduce the excessive crushing strains, per unit of surface, on both, and thereby enhance the life of both rolling stock and track. That this can be achieved by cylindrical wheels and flat topped rails, as long as they are new, or can retain their original shape, is equally true; but, can this be done?

It seems to me that such shape of wheel and rail top would answer best, which through continuous use and abrasion would bring more and more surface in contact, instead of having such contact from the start with new material, and diminishing the same through actual service. Rails with curved tops and sharp corners, and conical wheels with obtuse angles between rim of wheel and flange, seem to me to be approaching this condition better than those suggested by the author.

That the rails do not render service until they are worn out, but deteriorate a great deal faster, is, in my opinion, not exclusively due to the heavy pressures imposed upon them by the locomotive driving wheels, but more so to the high speed, imperfect tracks and faulty joints. In this respect the shapes suggested by the author would not remedy the evil. The style of joints mostly used at present, angle bars bearing under the head of the rail as well as on the foot of the rail, seems to me a barbarous abuse of the rail; it reminds me of a slugger of studied cruelty, who uses an ingenious device to hit the corn on the toe with the same blow he deals to the head of his victim.

The imperfections above mentioned cause the life to be pounded out of the rail, before it has time to be rendered worthless by wearing. The same is experienced with good sound oak cross-ties, that naturally ought to do service for about seven years, while on roads with heavy traffic and high speed of trains they do not last more than two or three years. The life is pounded out of them, said an experienced engineer to me some time ago.

On page 148 Mr. Whittemore states as follows, with reference to the test made by the Chicago, Burlington and Quincy Railroad with new and old rails: "It will be observed that these tests do not show any remarkable change through use;" and again, "While the appearance of the photographic etchings can be sometimes observed in new rails, owing, as is believed, to faulty manufacture or of design, yet fractures of this rail before use did not show such defects of structure.

"It is the slow but fatal development of an internal organic disease in a structure that is not organic, if we may borrow an idea from a present report, a sort of tuberculosis disease, with its slow-forming cavities."

Now, while I fully agree with Mr. Whittemore as to the latter statement, I beg to differ materially with him as to the showing of the test results above referred to in the first part.

Considering the first fifteen tests of the table, made with rails manufactured by "F," which presumably are of the same material, it will be noticed, that while in the new rail the ultimate strength is high, it is low in the specimens from old rails. Again, while the elastic limit for the new rails shows an average of 52 per cent. of the ultimate strength, it is in the old rail 60 per cent, and the elongation in new and old rails $9\frac{2}{10}$ and 18 per cent. respectively.

The larger elastic limit and elongation, combined with lower ultimate strength, in the old rail specimens, tells, I believe, the whole story, and explains also the cavities shown on the etchings. The life has been pounded out of the rail before it had time to wear out.

These test-results are perfectly in accord with the experience of testing laboratories, or of engineers having experience in testing eye bars for bridges. If a bar is tested and is strained slightly beyond its elastic limit, then removed from the testing machine and allowed to rest for a time, say a few weeks, and then again the test continued, it will be found that the so tested bar will show new life, and the elastic limit will be higher than the first time; the elongation will be greater, but the bar will ultimately fail suddenly with greatly reduced ultimate strength from what might have been expected. It reminds one of the last effort of ebbing life in a sick and dying person; the remaining spark of life has concentrated its efforts to a last resistance, but after that the collapse is sudden.

The few tests referred to are scarcely sufficient to warrant positive conclusions, but, in my opinion, point in the direction indicated. It would be very desirable to have more tests with new and old rails of the same material, to determine the facts.

If, however, my views, here expressed, are not erroneous, the remedy is not so much in the shape of rail-tops and wheels, but in the proper weight of rails, with reference to the traffic and speed of trains, the better distribution of the weight over all the wheels of the locomotive, perfect track and sound couplings at rail joints.

J. Foster Crowell, M. Am. Soc. C. E.—In this able paper Mr. Whittemore has definitely described and located one great evil, and suggested a remedy; indirectly he has referred to others in the same field hardly less great; and it would seem that we cannot proceed very far in discussing the form of the top of the rail and of the wheel before we encounter positive and insistent questions of the form of the rest of the rail, of its process of manufacture, of its support and fastening in the track, and of its maintenance in the planes in which it is placed.

Probably there is nothing else in the whole range of railroad appliances for which so little is done and of which so much is expected as the modern rail. It is conceived, in the converter, in sin, and born from the rolls in iniquity; it is tortured and beaten to correct the faults of its pre-natal days, and put out of shape so that it may sit up straight; it is then taken out with its fellows, thrown violently down a steep place, put into an imperfect track, with yielding, shifting, shrinking support, subjected to the weight and impact and pushing of loads that far exceed its strength; at length it expires, still in early life, and when the learned doctors come to decide why it did not live, they take the shape of what is left of its poor head and consult phrenologically. It is not too much to say that important as the relation is of the top of the rail (and the tread of the wheel), it is no more so than the relations which concern its footing.

The merits of Mr. Whittemore's suggested arrangement seem to me evident; it would certainly appear to be the natural arrangement for rails on straight lines.

There was at one time a belief that the coning of wheels caused them, when on straight track, to take and maintain through gravity a central position between the rails. This tendency is the only advantage of coning; with partly worn wheels it diminishes or altogether disappears. On curves, especially where sufficient elevation is not secured, the advantage of the flat topped rail is not so marked, for the reason that in taking position against the outer rail the wheel will ride up on its fillet, and all the weight will come upon the rail corner. This, it is true, is a condition no worse in its extreme case than exists with the curved top rail, but the fact calls for attention, and suggests the query whether we should not have a special type of rail for outsides of curves; those rails aggregate in this country about 12 per cent. of the entire service—a very small minority to govern when their requirements are so much greater.

It is probable that if flat topped rails are introduced it will be before all coned wheels have disappeared, and it would in that case be necessary to adapt the outer curve rail for coned wheels, by giving inclination to the top.

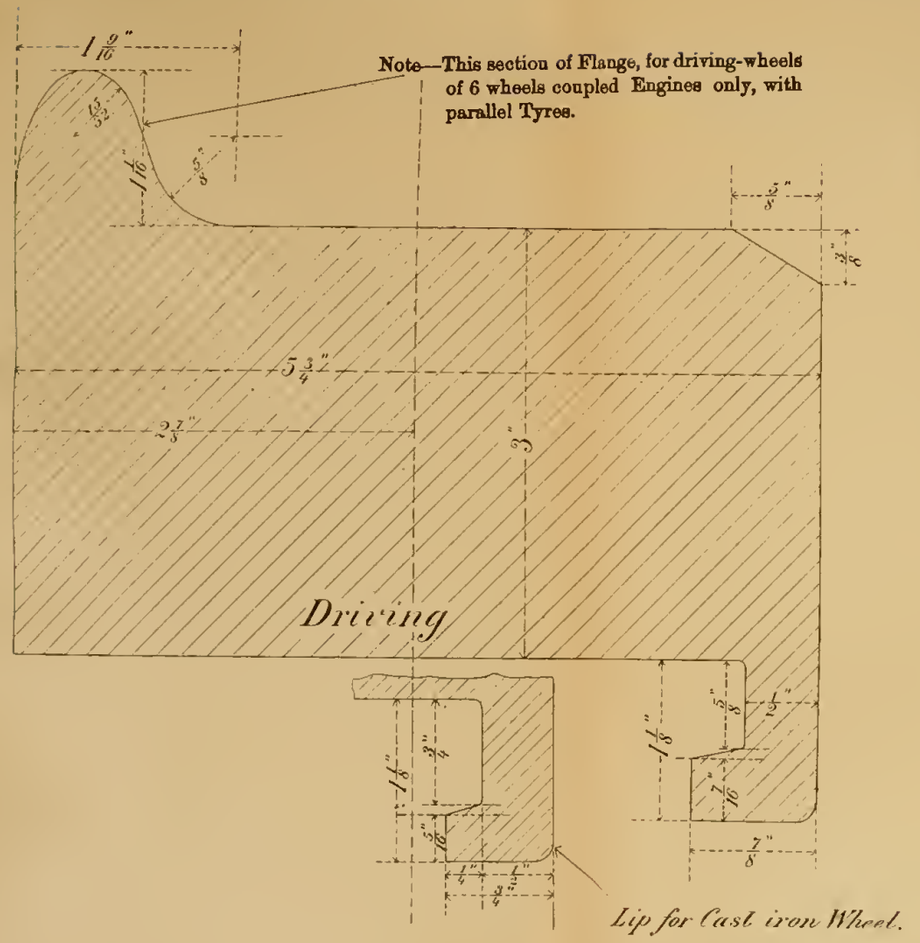
In regard to the relative wear of flat topped and curved top rails the deductions of Mr. Whittemore seem to me convincing.

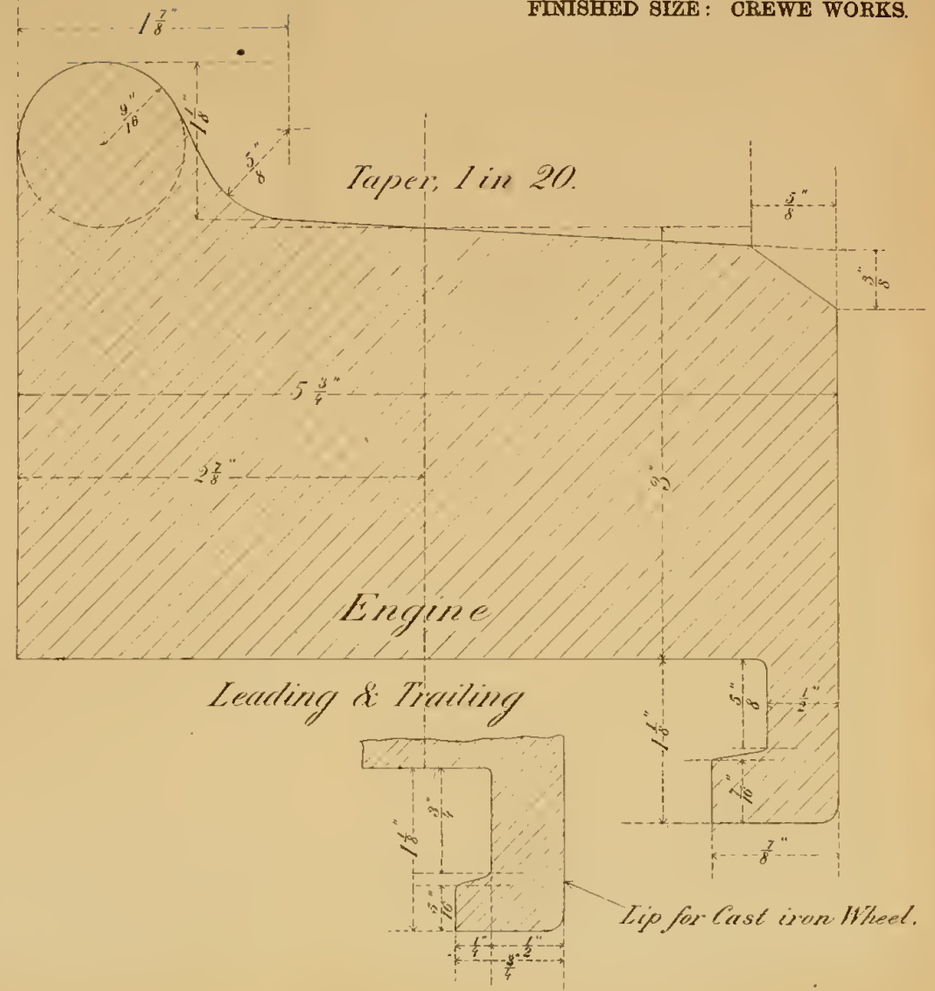
LONDON AND NORTH-WESTERN RAILWAY. GOODS ENGINES.

SECTIONS OF TYRES.

FOR ENGINE, CARRIAGE & WAGON WHEELS.

FINISHED SIZE: CREWE WORKS.





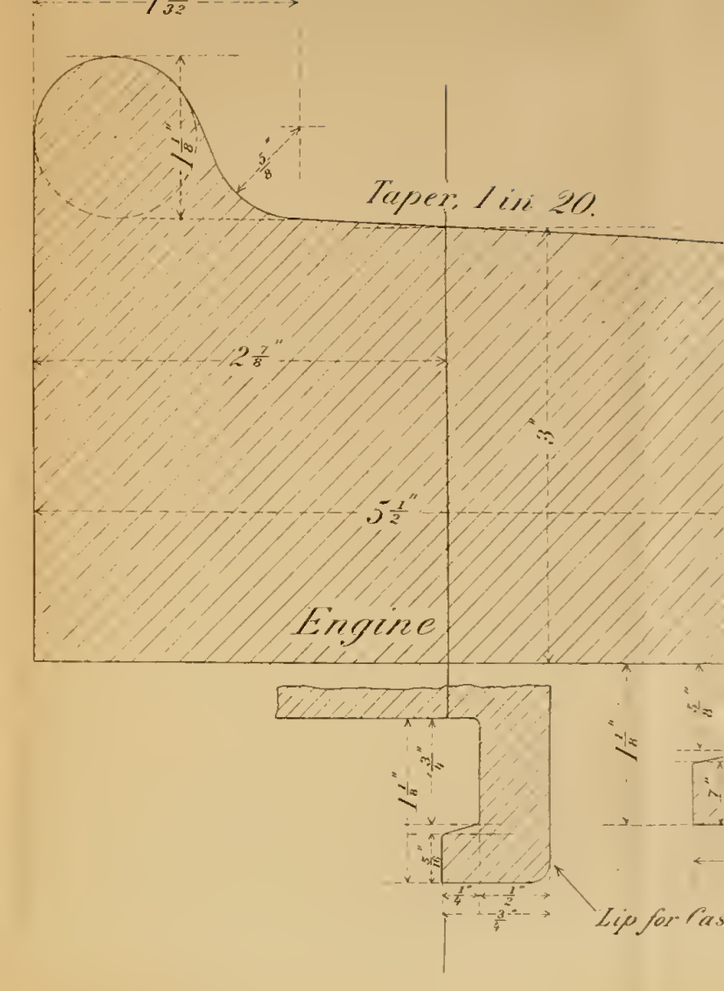
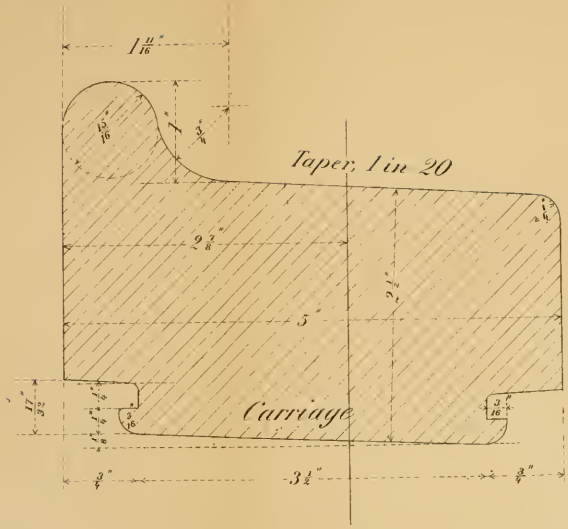
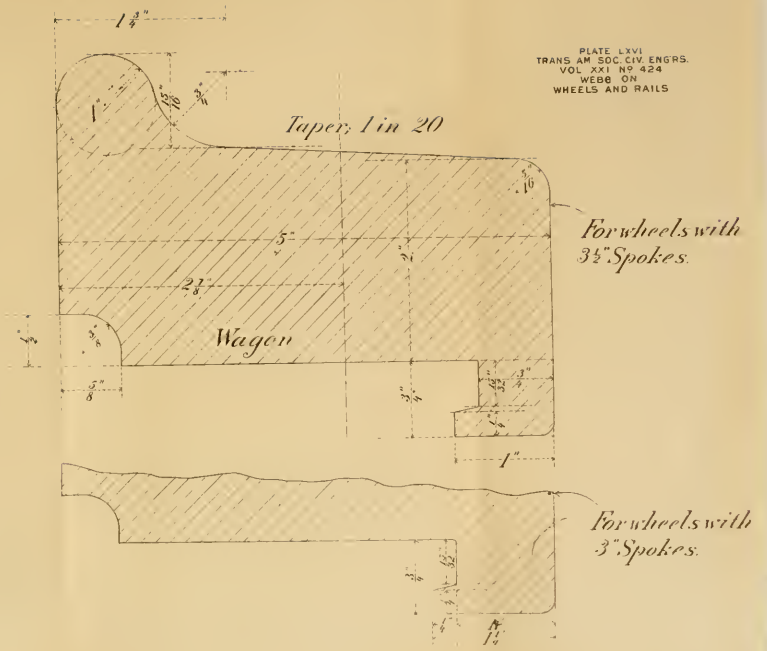


PLATE LXVI
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XXI. No. 424
WEBB ON
WHEELS AND RAILS

Tip for Cast iron Wheel.



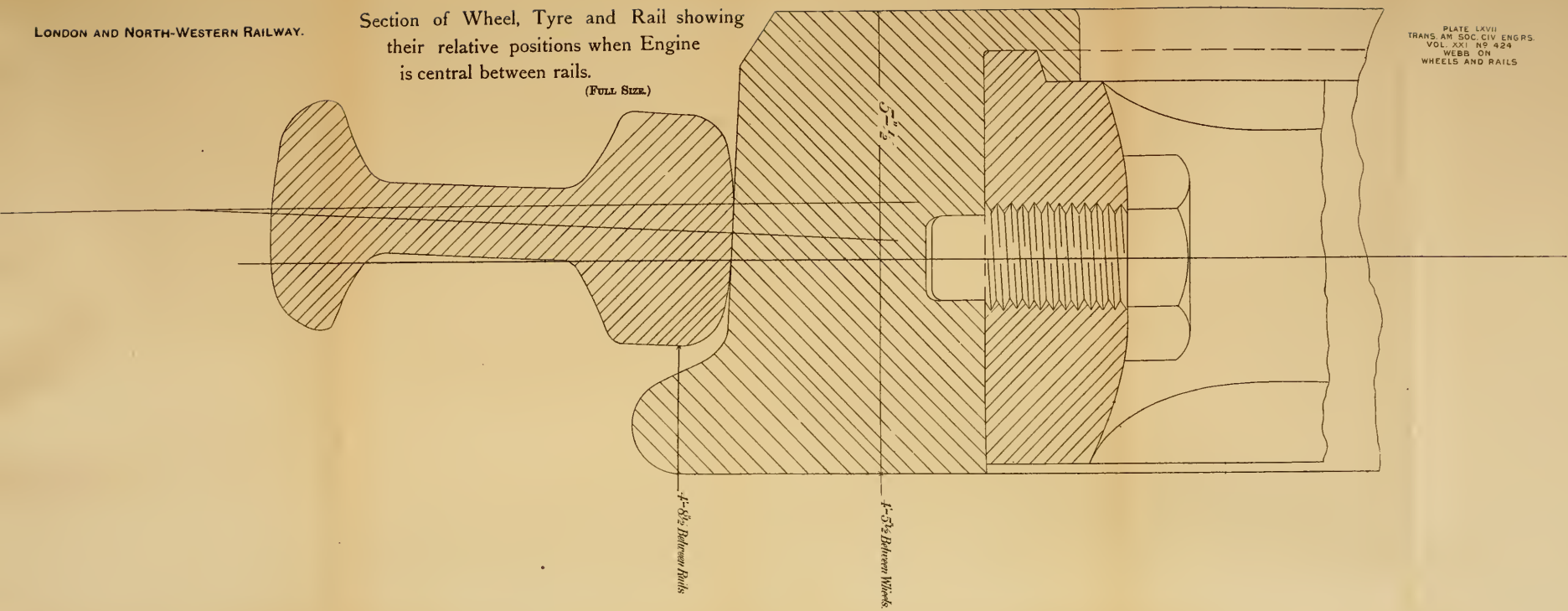


NOTE—The distance between Tyres, viz. 4"—5½" is the same for the whole of the Stock.

LONDON AND NORTH-WESTERN RAILWAY.

Section of Wheel, Tyre and Rail showing their relative positions when Engine is central between rails.

(FULL SIZE.)



A limited amount of the first wear of new rails is due to abrasion. This continues until the scale is removed from the part of the head that is in contact, leaving it smooth and polished; if now the traffic is light and the metal in the rail uniform, the abrasion continues, but at a much slower rate, until the head is worn to its limit, retaining its smooth appearance; but under heavy traffic the rail shows altogether different signs of wear—signs that show that abrasion is being exceeded. And soon there are evidences of the actual failure of some part of the rail, although the surface of most of it may be still fit for a long service. It seems, then, to go without saying, that if the effect of the major cause can be reduced by increasing the area of contact to limits that will not allow the disintegration to take place, it is a matter of small concern if in so doing we somewhat, but very slightly, increase the total amount of abrasion. As far as the wheel is concerned it will only affect that portion of the tread not previously in contact; and as for the rail, it is its weakest foe.

Mr. F. W. Webb, Mech. Eng. London and North Western Railway.—I am afraid that if the rails were made as Mr. Whittemore suggests, in a very short time they would shape their own form for themselves, owing to the wear of the tires. In this country (England) we do not appear to have the same difficulty in rails wearing sideways as you have in the States, and when we decided upon the present form of the top of our bull head rail, we had templates taken over several miles of railway of rails partially worn, and we then made the top of our rail as near as possible the average of the templates we had taken in the way described. I believe we get over a good deal of the difficulty you experience by greater elevation of the outer rail on curves than you have. I know of no rule by which this can be ascertained theoretically, owing to the various speeds of trains passing over various portions of the road; but our practice has been when there are indications of wear on the side of the outer rail to elevate that rail until the wearing disappears.

The only tires we turn cylindrical are in our six-wheeled coupled engine stock, the middle wheels being turned cylindrical, so as to ease them in going round curves; the diameter being the same as the mean diameter of the cone over the center of the rail in leading and trailing wheels. The inclosed blue prints will show you our practice. (Plates LXVI and LXVII.) Our standard gauge is 4 feet 8½ inches, but in going round sharp curves we widen out the rails up to 4 feet 8¾ inches.

J. D. Hawks, M. Am. Soc. C. E.—The conclusion which Mr. D. J. Whittemore draws from his experience with steel rails is that a great many of them split, owing to the metal being strained beyond its elastic limit. On this conclusion he founds his paper, with the title given above.

If this conclusion is incorrect, then the paper is not correct.

I think the conclusion is wrong; and, consequently, think that flat topped rails, instead of being an advantage, would be a disadvantage.

Flat topped rails would not come from the mills straight, but winding. They would not be laid perfectly level on the ties, and would not stay level if they were so laid. Hence, the wear of the wheels would come on the inside or outside of top, and aggravate the evil intended to be cured. I do not think the evil is as great as Mr. Whittemore infers. We frequently change rails that have been worn down on one side of the rail on a curve to the straight line, or even to the inside of the curve, and this throws the bearing all on one side of the rail, until that side is worn down sufficiently to give an even bearing on the top. I never have noticed any rails so used that have split and cracked.

I have put new rails in track when the ends showed splits, and have watched them carefully, in order to see how long they would wear before the splits fully developed, and sometimes it would be years before a split could be noticed, except by a person who knew that the rail was split. I have also left rails in track for years after the rail showed a decided split. I do not recommend such a course, however, and I now have all split rails removed every fall from track, as there is no tendency, of course, in split rails to mend themselves, and I do not consider it worth while to see the length of time they will wear, when an accident may result from leaving them in a little too long.

Splits can frequently be detected by black spots or black streaks along the head of the rail before the split has opened. I have cut a great many rails showing these black streaks, and have always found the split that I was looking for. I have also frequently found splits that I was not looking for, while cutting rails, but never have found one yet that I did not believe resulted from a defect in the ingot, and not from over-straining the rail.

Splits develop more frequently in the ends of the rail than in the middle, but are common enough in the middle. Why should a split show in one part of a rail and not in the whole length of it, if the split came from over-straining the rail, and not from a mechanical defect? I have frequently cut off the end of a rail on account of a split, and worn out the remainder of the rail in a track without any further trouble from splitting. Of course, the end of the rail gets the heaviest blow and the hardest usage; but this is not the reason why splits show more frequently at the end of a rail than in the middle. The real reason is that the end of the rail is nearer the end of the ingot. The heavy blows on the ends of soft or "pewter" rails, that we had so much trouble with a few years ago, did not split the ends, even when the ends were worn or battered down half an inch. Of course, the blow under these circumstances was very much more severe than it ever is in a properly kept up track.

It is frequently the case that rails are split in the main track and taken out and put into sidings or switching tracks with half the heads

split off, and will stand all right for a long time if it is the outside half of the head that is gone.

There is no part of engineering where more care is needed in drawing conclusions than in track work. Only last week the Vice-President of a large steel rail mill was telling me that a western road was very much pleased over an experiment they had tried of putting long six-bolt splices on some steel rail that was considerably battered at the joints. The result of the experiment was that the joints got very much better, and the conclusion was that it was owing to the long splices. While I am an advocate of the long splice, I still was under the necessity of relating a little experience of my own, where a long strip of Troy steel, that had been battered so much at the joints that I feared the whole lot of it would have to come out and be condemned as soft rail, improved from year to year, and is very much better at the joints now than it was five years ago; and still it has on it the four-hole short splices with which it was laid.

Our sales of scrap steel from 2 100 miles of steel rail track average about 900 tons per year, and a large part of this comes from worn out steel rail frogs and ends from rails which are cut off by our rail-saw.

I attach our steel-rail report for the last four years, showing the number of broken, split and worn out rails removed from track. I know it is not customary to give the makers' names in such a report, but do not think there can be any fault found with it in this case. (Tables Nos. 1 and 2.)

It will be noticed that the record for steel made during the last four years is very good indeed. The record is deceptive, in some respects, without explanation. Many of the breaks in the older rails were caused by punching bolt-holes. The record of the bruised or battered rails would have been very much larger, except for the fact that long pieces of track laid with the "pewter" rails showed up in such bad shape that they were taken up, and the rails sent to branches or used in sidings after having been in use only a few months on main line.

We received the last of the soft rails in 1884, and the report of rails taken out of track, of recent rails, is very good indeed, and shows no increase in the tendency of rails to split on account of over-straining of the metal, as all rails laid since 1884, until this year, are 65 pounds per yard. Our standard is now 80 pounds per yard, and I shall be disappointed if this heavy rail does not show fewer bruises and fewer broken rails than the 60 and 65 pound rail, besides giving us a much better riding track, and one that can be maintained at much less expense.

We get considerable wear out of defective rails after they have broken at the end or split at the end, as we saw off such rails and use them on branches. Of course, it costs money to do this, and I would very much prefer that they would not split, or break, or bruise, but have very little fault to find with such a record as that of the last four years.

I think the rail-makers are agreed that they can now make a rail from 65 pounds upwards, of any of the recent shapes, that will carry any traffic in this country, without any undue tendency to split or crush. I do not think that the present discussion in relation to flat top rails should be construed as showing a tendency to give the rail men any more advantages than they have at present. We have had a great deal of trouble, of course, from poor rails, but I think the railroads are more responsible for a good share of this trouble than the rail-makers, and from my experience with rails during the last few years, I think we have every reason to suppose that the worst of our trouble with poor rails is over.

TABLE No. 1.

STEEL RAILS MICHIGAN CENTRAL RAILROAD.

BRAND.		Number Rails Received.	Broken.	Bruised.	Split.	No Fault.	Total.	Number Rails in Track April 1st, 1889.	REMARKS.
Cammell, Sheffield,	1885..	7 002	23	..	5	..	28	6 974	
" "	1888..	3 494	..	..	..	..	..	3 494	
Edgar Thomson,	1885..	1 847	3	..	..	..	3	1 844	
" "	1887..	36 438	39	..	7	2	48	36 390	(No fault.) Caused by broken wheel.
N. C. R. M.,	1885..	14 359	13	..	8	..	21	14 338	
"	1886..	1 756	1	..	1	..	2	1 754	
"	1888..	26 872	4	..	1	2	7	26 865	(No fault.) Caused by engine slipping.
Barrow,	1886..	6 975	1	..	2	..	3	6 972	
"	1887..	5 240	4	..	9	..	13	5 227	
Scranton,	1886..	20 440	3	..	9	..	12	20 428	
Totals.....		124 423	91	..	42	4	137	124 286	

Mr. WHITTEMORE.—Mr. Hawks states my position quite correctly, in that rails split and fail through straining them beyond their limit of elasticity. He states further, that, if this position is incorrect, then my paper is incorrect, and then proceeds to prove to his own satisfaction that it is so, knocking me out in the first round, and now the question in my mind is whether his blow is not delivered below the belt. I think it is.

Mr. Hawks will not contend for one moment that rails with all the defects he mentions will disintegrate and fly to pieces without being subject to extraneous strains, and these strains must be of sufficient magnitude to exceed the limit of elasticity, be the rail defective or not so far as manufacture is concerned. These strains will search out and

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TABLE No. 2.—Steel Rails, Michigan Central Railroad Company.

Brand.	Tons in Track June 1st, 1884.	Received, 1884	Taken out during year 1884. Broken.	Bruised.	Split.	Worn Out.	No Fault.	Tons in Track Jany. 1st, 1885.	Received, 1885.	Taken out during 1885. Broken.	Bruised.	Split.	Worn Out.	No Fault.	Tons in Track Jany. 1st, 1886.	Received, 1886.	Taken out during 1886. Broken.	Bruised.	Split.	Worn Out.	No Fault.	Tons in Track Jany. 1st, 1887.	Received, 1887.	Taken out during 1887. Broken.	Bruised.	Split.	Worn Out.	No Fault.	Tons in Track Jany. 1st, 1888.	Received, 1888.	Taken out during 1888. Broken.	Bruised.	Split.	Worn Out.	No Fault.	Tons in Track Jany. 1st, 1889.	Recapitulation. Broken.	Bruised.	Split.	Worn Out.	No Fault.	Total Tons.
John Brown	8 146	.....	4½	7½	2	½	....	8 131½	.....	16	6	3½	4	½	8 108½	.....	10½	4	2½	.....	1	8 086½	.....	12	½	½	.....	....	8 073½	.....	36½	25½	24	7½	2	7 978½	79½	43½	33½	8½	3	167½
Cammell Sheffield	29 082	2 510	3	8	4	....	....	31 577	2 003	3½	74	16	½	....	33 473½	.....	20½	26	4½	.....	3	33 424½	.....	4½	16½	8½	.....	1	33 358	1 000	34	51	45½	½	....	34 223	101½	174	92½	1½	10½	377
Landers	6 318	.....	....	1	....	....	....	5 217	.....	2½	5	32	½	....	5 266	.....	1½	3½	....	.....	....	5 214	.....	1½	1	½	.....	....	5 201	.....	8½	31	5½	....	....	5 184½	13½	13½	87½	.....	½	33
Landers	2 446	.....	4½	20	12½	½	....	2 409	.....	12	32	20	2½	2	2 340½	.....	8	10½	12½	.....	....	2 305½	.....	7	13	7½	2	....	2 273½	.....	33	37½	35½	2½	½	2 165	65	118½	87½	7½	2½	281
C. L. & Co.	4 923	.....	2½	6	4½	½	....	4 910	.....	6½	13	7½	½	1½	4 885½	.....	5½	2	9	.....	....	4 867	.....	2	4½	4½	.....	....	4 846	.....	17½	19	8½	½	½	4 723	34	51	111½	1½	2	200
Fox & Co.	3 467	.....	3	11½	4½	1½	....	3 446½	.....	9½	41	7	1½	½	3 386	.....	8½	16	31	½	....	3 353½	.....	6½	14	5	.....	....	3 325	.....	25	25½	53	....	4	3 217	55	109	77	3½	4	249½
Griswold	10 395	.....	....	37½	....	....	....	10 357½	528	8	122	13	....	2	10 740½	.....	6	89	4½	.....	....	10 671	10 936	4½	8½	½	.....	....	21 356	.....	29	30	14	....	½	21 191½	38½	234	32½	....	2½	327½
Edgar Thomson	7 053	.....	....	1	1½	....	....	7 051	.....	1½	3	3½	½	....	7 045½	.....	2½	1½	2½	.....	....	7 039½	.....	3	3	2	.....	....	19 084	.....	8½	101	31½	....	....	6 981½	15	133	40	½	....	68½
Lackawanna Iron and Coal Co.	10 995	.....	3½	7	1½	....	....	10 994½	.....	21	18	12	....	....	10 943½	.....	19	11	16½	.....	....	10 897½	.....	10	65½	39½	.....	....	15 788	.....	43½	39	84½	2½	....	16 631	95½	141½	145½	2½	½	384½
O. R. M. Co.	11 066	.....	....	....	....	....	....	1 399	.....	½	2	½	....	....	1 396½	.....	1½	2½	1	.....	....	1 391	.....	1½	½	½	.....	....	1 389	.....	2½	5½	4½	....	....	1 382½	4½	6	5½	....	½	16½
M. C. Bessegeo	1 299	.....	....	....	....	....	....	1 299½	4 121	1	½	½	....	½	5 419	459	3	½	1	.....	....	5 905½	.....	1½	½	½	.....	....	5 903	7 780	11	74	25	15	....	....	13 601	16½	64	15½	....	½	396
N. O. R. M. Co.	1 291	.....	½	....	....	....	....	1 291	.....	15	68	18	1	½	14 836½	.....	15½	4½	6½	.....	....	14 840	.....	9½	24	3½	.....	....	14 773½	.....	74	25	225	....	....	14 447½	120½	131	254½	1½	½	498½
Joliet	8 467	6 491	6½	3½	2	....	....	14 952½	.....	4½	13	10	....	....	10 871	.....	2½	78	10	.....	....	10 780½	.....	3½	30	1½	.....	....	10 745	.....	15	2	37½	....	....	10 719½	55½	102	36½	....	....	246½
Troy Steel	10 964	.....	1½	10½	13	....	....	10 888	.....	24	4	14½	....	....	1 225½	.....	26	3	3½	.....	....	1 193½	.....	1½	1	½	.....	....	1 189	.....	14	1½	4½	½	....	1 135	60	24	57½	....	4	137
A. F. Cresset	1 372	.....	3½	....	½	....	....	1 368½	.....	....	2½	....	½	....	136½	.....	1	1½	1	.....	....	127	.....	....	....	....	.....	....	126	.....	4½	2½	2½	....	....	117½	4	7½	5½	1½	....	18½
O. Ill.	136	.....	....	1	....	....	....	135	.....	....	....	....	....	....	99	.....	7	....	....	.....	....	99	.....	1½	....	1	.....	....	97	.....	....	....	....	....	....	97½	22	3	3½	....	....	28½
B. & A Dikom	1 007	.....	....	....	....	....	....	1 006½	.....	8½	2	....	....	....	997½	.....	....	....	....	.....	....	27	.....	....	....	....	.....	....	27	.....	....	....	....	....	....	27	....	....	....	....	....	....
Bessemer Steel	27	.....	....	....	....	....	....	27	.....	....	....	....	....	....	27	.....	....	....	....	.....	....	27	.....	....	....	....	.....	....	27	.....	....	....	....	....	....	27	8	8½	1	....	....	17½
No brand	2 240	.....	....	....	....	....	....	2 240	.....	2½	....	....	....	....	2 237½	.....	....	....	....	.....	....	2 236½	.....	1½	....	1	.....	....	2 234½	.....	2½	3½	2	½	....	2 224½	....	....	....	....	....	....
S. S. Bantin & Co.	3	.....	....	....	....	....	....	3	.....	....	....	....	....	....	3	.....	....	....	....	.....	....	3	.....	....	....	....	.....	....	3	.....	....	....	....	....	....	3	....	....	....	....	....	....
Barrow	929	.....	½	....	....	....	....	929½	.....	14½	½	1½	....	....	913½	2 000	9	1	½	.....	....	2 904½	1 500	1½	1½	....	.....	....	4 400	.....	2½	5½	4½	....	1½	4 386½	27½	7½	6½	....	....	41½
Saunders Dowlas	1 016	.....	....	....	....	....	....	1 015	.....	½	....	....	....	....	1 014½	.....	....	....	....	.....	....	1 014½	.....	....	....	....	.....	....	1 014½	.....	....	2½	2½	....	....	1 009½	2½	2½	....	....	....	5½
A. F. Ferremore	9 826	.....	12	1½	3	½	....	9 808	.....	224½	76	107½	....	½	9 396	.....	107	33½	60½	.....	....	9 136½	.....	7½	42½	36	.....	1½	9 047½	.....	21	42	126	....	5	8 854½	431½	195½	336½	....	½	971½
B. V. & Co.	606	.....	½	....	....	....	....	605½	.....	1½	....	....	....	....	604	.....	3	....	....	.....	....	601	.....	....	....	....	.....	....	600½	.....	1	½	5½	....	....	594½	6½	....	9½	....	....	11½
West Cumberland	761	.....	½	....	....	....	....	760½	.....	17½	....	....	....	....	742½	.....	8½	....	½	.....	....	733½	.....	....	....	....	.....	....	733½	.....	....	....	....	....	....	732½	26½	½	2	....	....	28½
Scranton	15	.....	....	....	....	....	....	15	.....	....	....	....	....	....	15	.....	....	....	....	.....	....	15	.....	....	....	....	.....	....	15	.....	....	....	....	....	....	15	....	....	....	....	....	....
Totals	121 686	9 001	86	116½	53½	2½	....	130 427½	6 651	394½	476½	235½	8½	15½	135 948½	8 810	327½	268½	142½	1½	3	143 716½	12 096	110½	221½	168½	2	2½	155 358½	8 730	366½	353½	814	14½	14½	162 626	1 293½	1 435½	1 353½	28½	35½	4 147

find the weakest part of the rail, and the effect will there be first apparent.

The point I make is that in no way other than by corrosion, does material, good or bad, fail, except through strains beyond the limit of elasticity, and I expressly state in my paper that, while good material will render greater service by being correctly formed, poor material will also give better service for the same reason.

While I must admit that the early failure of many of our rails can be attributed in a great measure to faulty manufacture, I seek to call the attention of our members to a fault of design, which, when corrected, will, as I believe, add materially to the service of both good and faulty material. Good material correctly formed can only be secured through care in manufacture and inspection, and the rails should then be laid in their position as nearly correct as practicable. This can be done, and when done the objections raised against the flat headed rail will be in a great measure, if not entirely, overcome, provided we can have cylindrical wheels to run upon them.

M. L. Le Rond, C. E. (Ingénieur des Ponts et Chaussèes).—Mr. Whittemore's paper is highly interesting and expresses novel ideas on a problem which may be considered as vital for the near future of railroads. He points to the frightful overloading of the metal of rails and tires, and to the ever-growing danger concealed in such a practice. An arrangement which shall reduce the strains in the metal to a reasonable rate beneath the elastic limit, will avoid fearful disasters which must fatally happen otherwise before this century is ended. And none of the advantages claimed by the author of the paper for cylindrical wheels traveling on flat topped rails, seem to be overestimated.

The values given in the table, page 145, show that with Mr. Whittemore's arrangement, the weight on wheels can be safely increased to almost any extent, providing that the width of the flat top and cylinder shall be correspondingly increased. And is not this fact one of common evidence, viz., that the way to reduce rotary friction on a yielding surface is to increase the width of the tread of wheels?

On a solid road-bed narrow treaded wheels may do, but not on a soft or damp soil; country carts must have wheels provided with a broad tread, or they will be soon mired.

On railroads, with the weights now in use, both rail and wheel, or tire, must yield, increasing the resistance of trains, as well as crushing the metal.

So that the flat topped rail and cylindrical wheel, being the safest, are at the same time the best arrangement to secure the least frictional resistance on tangents.

It is easy to see that it generally reduces, also, the friction on curves.

In the case of flat topped rails and cylindrical wheels the frictional resistance will be $(R-R^1)\ \alpha f P$

Where R and R^1 are the radii of the outer and inner rails,
α the angle between two radial positions of the axle,
f the co-efficient of sliding friction,
P the weight on the axle.

On the other hand, in the case of usual rails and coned wheels, the inner traveling on its greatest, the outer on its least diameter, the friction will be for the same length of curve traveled on,

$$(r^1 - r)\, w f P + (R - R^1)\, \alpha f P$$

Where R, R^1, f, α, P have the same meaning and value as above,
r^1 is the greater wheel radius (inner),
r is the least wheel radius (outer),
w the revolution of the axle corresponding to the length traveled on.

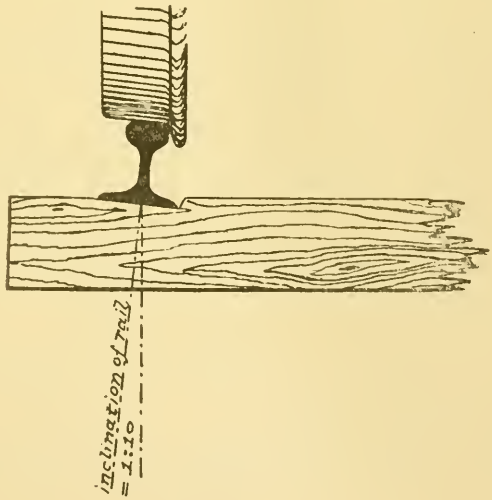
It appears therefrom that, as stated, the resistance is greater in the case of coned wheels, and the value of the excess is

$$(r^1 - r)\, w f$$

Which is reduced to zero, as it ought to be, when $r^1 = r$, or in the case of cylindrical wheels.

The adoption of Mr. Whittemore's ideas in American railroad practice will, therefore, be an evident progress.

On European, viz., on French railways, the advantages might be less.



It is a custom on our roads to notch down the rails on the ties so that their web is not vertical, but inclined, from bottom to top, toward the inside of the track; the inclination of the web is one in ten (see Figure).

This disposal has some advantages, to prevent, on tangents, the rails from spreading, and, on curves, from turning over.

The coned wheel has thus its tread surface sensibly parallel to the rail top, and the bearing area is not as small as in American practice.

The coned surface is then properly fitted to suit the inclined top of rails, and this is a reason which forbids, on French roads, the use of cylindrical wheels, till the vertically laid rail is adopted.

The coned wheel, unfit on American roads, seems not, therefore, to be as improper on ours ; although, if a proper fastening of rails be secured, the adoption of Mr. Whittemore's ideas, with vertically laid rails, would evidently simplify some of the most intricate problems now involved in railway construction and maintenance.

As for Brunel's double web rail, though it may seem theoretically commendable, it paid no good service, some years ago, on the "Chemin de fer du Midi," where it was tried as "rail Barlow." The iron rails of that pattern had to be all taken out after a few years.

Charles Blackwell, M. Am. Soc. C. E.—In regard to best form for rail heads, I believe that the excessive weight now imposed on engine and freight car wheels, renders it necessary to meet the evil by adding to the utmost extent possible to the areas in contact; and I fully endorse the proposition to make the rail heads entirely flat, with a $\frac{1}{4}$ or $\frac{5}{16}$ top radius. But in conjunction with this, the "rail path" or "tread" of all wheels must be made parallel.

C. Frank Allen, M. Am. Soc. C. E.—In considering rails and wheels and their preparation for the scrap heap, and in considering the proper form of section of rail, it seems to me that for the immediate present we should look closely to the form as affecting the quality of rails (or wheels), and I believe that emphasis should be laid upon the fact (recognized by Mr. Whittemore in his paper) that "all ills the rail is heir to cannot be attributed to the error of its curved top." The tendency at present seems to be in the direction of heavier sections as a remedy for the unsatisfactory wear of rails. Is it certain that this is a step in the right direction ? What can we gather from past experience upon this point ? Quoting from the paper of Robert W. Hunt, M. Am. Soc. C. E., read before the American Institute of Mining Engineers: "One of the most prominent chief engineers of the country said to me not long ago that he supposed his road would lay 80-pound rails next year, but he was at a loss what section to adopt, for if their 80's gave as much poorer service as compared with their 65's, as the latter had in relation to their old 60's, he guessed they would have to adopt a 90-pound rail for the following year. This states it broadly, but it is a generally admitted fact that the increased sections have as a rule been disappointing in their wear."

It would seem possible that the increased size was obtained at the cost of the "physical hardness" of the metal, the larger section rendering proper rolling difficult or impracticable.

In the case of locomotive tires the discussions of the Western Railroad Club, not long since, established beyond a reasonable doubt the fact that 3-inch tires were superior in wearing quality to 4-inch tires; and the principle involved would appear to be no different in the two cases of tires and rails. All the above evidence certainly is not favorable to large sections for tires or rails of steel.

History often repeats itself, and in the case of iron rails it appears that a similar unsatisfactory experience with heavy iron rails was had many years ago. Quoting from Colburn and Holley on "Permanent Way of Railroads," published 1858: "The 45-pound rails made in 1837 for the Philadelphia and Reading Railroad have stood a wonderful wear. The 64 and 68-pound rails since laid down in their place have gone to pieces with less than one-third the same wear."

Again: "In 1854 rails of 85 to 100 pounds were considered by English engineers to be the best. Since that time it is found in the Eastern counties line that the 95-pound rails made the worst road, were less durable, and in course of time became the most dangerous, as compared with 75-pound rails."

"The London and Northwestern officers report more failures with the 82-pound rails than with the former 56-pound rails."

"The New York and Erie Road has had rails of 58, 60, 63, 65, 68, 72 and 75 pounds laid down, in place of 56-pound rails, the heavier iron almost invariably proving inferior."

The experience of a number of other roads noted was in the same line, and opinion seemed to have become general in favor of 60 to 70 pounds, rather than from 85 pounds upward.

One experienced buyer stated "that the only good way to get good rails was to contract for 85 pounds per yard, and to then arrange with the manufacturer to supply a better quality, of 75 pounds per yard, for the same gross sum." Isn't it possible that the same rule will hold good for steel? It seems to me that better quality is what is needed, and that our energies can best be directed to that end at present.

It is not my purpose to discuss the correctness of the formulas and results used by Mr. Whittemore. The status of affairs seems to me to be this: whether or not we have thus far kept within the elastic limit, there can be little doubt that weights of rolling stock still tend to increase, and it seems probable to-day that this increase will be limited finally by the safe carrying power of the rail, and we shall be at last brought to decide what form of section will safely carry the heaviest load. Improving the quality of the steel will help us and may delay the issue for a time, but the question is going to be not altogether whether the elastic limit is now exceeded, but whether it will be exceeded sooner upon a rail with a curved or with a flat top. It seems to me, too, that when we are upon the point of exceeding the elastic limit on the tops of the rails, we shall probably have passed the elastic limit at the rail corners, for

the resultant of the vertical and horizontal forces acting on the rail of the curve would almost certainly be greater than the vertical force alone on the rail of the tangent; and the area of bearing surface ought not to be less. I am not of the opinion that the qustion of flat topped rails can be altogether settled upon paper. There are many points involved. What will be the effect upon traction—upon rolling friction? Can the rail be maintained flat? The question, it seems to me, is whether the expectation of benefit from the use of flat topped rails is sufficient to warrant a thorough trial being made, and it seems to me that this much at least Mr. Whittemore has shown to be the case in his paper.

C. L. Strobel, M. Am. Soc. C. E.—The subject of the relations of the sections of railway wheels and rails is of the greatest importance. Mr. Whittemore has taken exception to some of the Committee's conclusions as contained in its Preliminary Report, and has very ably supported his views.

Unless all mechanical practice and theory in the past are wrong, a point contact between bearing surfaces is more destructive than a line contact, and the latter is more so than a surface contact. Points and lines in this sense are, of course, surfaces also, only they are such of very small area. A line may be considered a succession of points placed side by side.

In theoretical investigations of this subject, it has usually been admitted that the form of the surfaces in contact, outside of the area of contact, has an influence upon the resistance of the parts in contact; but it has never before been claimed, so far as I know, that this influence can be so great as to make the wear less for a point contact than for a line contact. Thus cannon ball rollers, when used under the movable end of a bridge, have never been supposed to be the equivalent of cylindrical rollers, roller for roller; for example, a 3-inch ball has not been used in the place of a cylindrical roller 3 inches in diameter and say 2 feet in length. If this substitution had been made the result would undoubtedly have proved disastrous

The loads which are applied upon the wheels of cars and engines may not be excessive for a point contact, but Mr. Whittemore has produced strong evidence that they are.

A number of writers have endeavored to find a theoretical solution for the problem of wheel contact. They have been successful only by assuming certain approximations to the actual conditions. Professor Grashof's formulas are among the best known, and, in the absence of something better, have been extensively used to determine the permissible loads for bridge rollers. The values obtained cannot be assumed as final, but they are at least serviceable as "an educator of the judgment," as Mr. Whittemore puts it. Even if it were possible to solve the problem by theory absolutely, there would still remain the indeterminable elements of impact and unevenness of road-bed, which more or less invalidate all calculations.

In the light of all the information we have upon the subject, however, the conclusion seems warranted that our present practice overtaxes both wheel and rail.

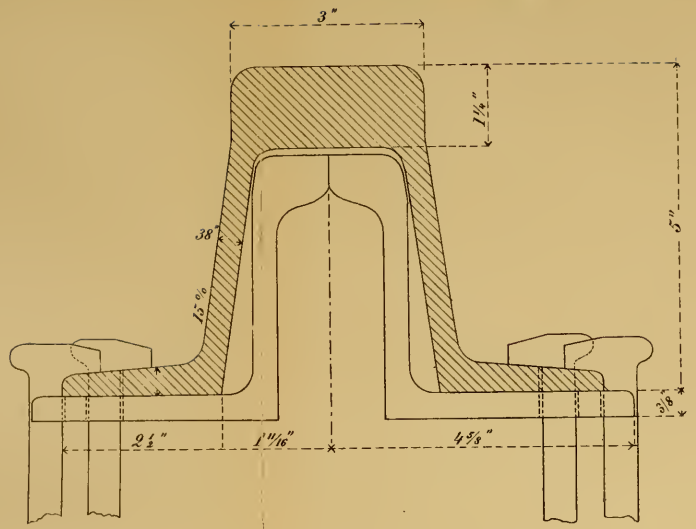
If the elastic limit is exceeded the metal flows, the molecules re-arranging themselves in conformity with the new conditions. This occurs more readily in a homogeneous metal like steel than in a fibrous metal like wrought-iron, and by continued action of this kind ultimate disintegration takes place. The motion of the wheel over the rail is particularly favorable for such destructive action in the rail.

The increase in the loading of wheels, which has been going on for some time, is not likely to have reached its limit yet. Metallic cars, of better construction, lighter weight and greater capacity than the present rolling stock, will be introduced, and the tendency will probably continue to put more weight upon the wheels. This may require a radical reconstruction of the railroads, and while this is somewhat startling to contemplate, it is a change such as takes place in manufacturing concerns in comparatively short intervals, namely, the substitution of a new plant for an old one, when it has been found that better results can be obtained by different machinery or methods. There is no reason why it can be expected that railroads will be exempt from such changes.

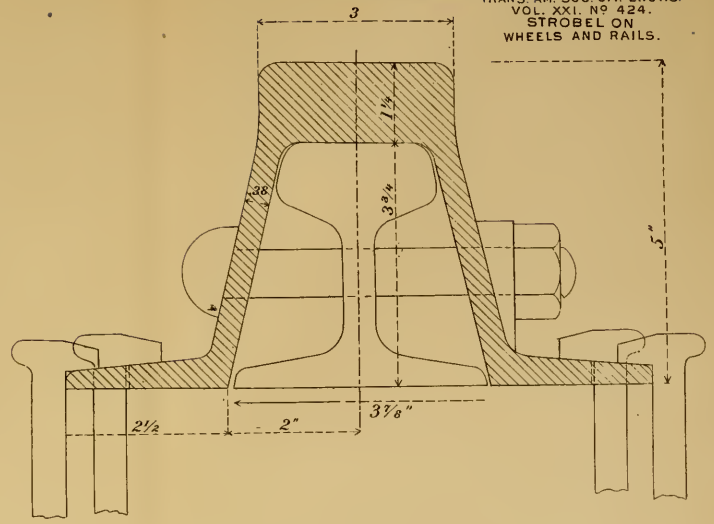
I think the contact between rail and wheel should be a line contact, as Mr. Whittemore proposes, but undoubtedly a slight tendency towards rounding the rail top will exist, due to abrasion from worn wheels, which will be more or less hollow in the tread. While, therefore, the theoretical condition of line contact cannot be absolutely maintained, there is no reason why we should not make as close an approximation to it as possible. Whether the rail-top should be an inclined surface or horizontal, and the wheel be conical or cylindrical, may be open to question. The rail could be inclined inward, as is done to some extent abroad, the gain from which would be that the flanges have a smaller duty to perform in preventing derailment for track on straight line. This would result in less flange wear and smoother running. The different diameters of the tread of conical wheels would, however, cause sliding on the rail, owing to the different velocities of the peripheries, and this will result in greater abrasion and friction. The Committee has found that flange wear with our present practice is not excessive, and in view of this, cylindrical wheels and upright rails, as proposed by Mr. Whittemore, may be the better plan. In this case there would be no sliding of surface for track on straight line, excepting for flanges. Experiments, however, would seem necessary, to decide the question satisfactorily as to which plan is preferable.

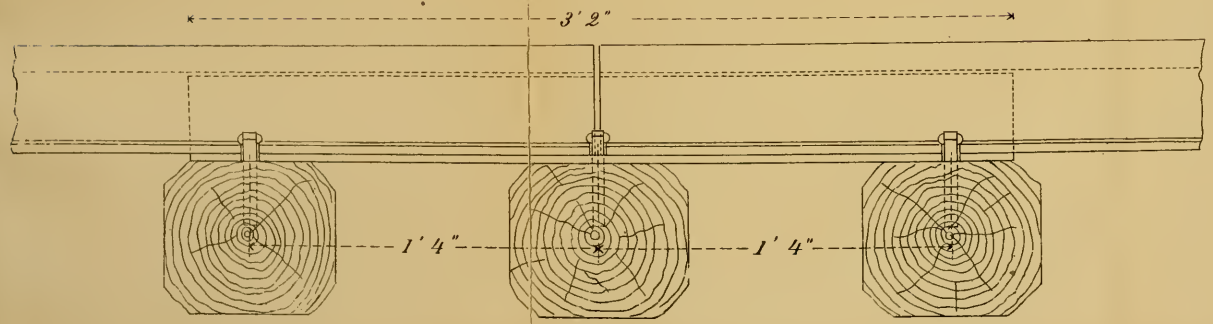
I am inclined to attach considerable importance to a change in rail section, only lightly touched upon by Mr. Whittemore in the conclusion of his paper. For present and prospective rail loads, the question arises whether we have not outgrown the old T-rail, and ought to

No. 1, WEIGHT 85 lbs.



No. 2, WEIGHT 85 lbs.





adopt the bridge rail of the type used on a number of roads in the early days of railroad construction. This rail was abandoned because of the extra weight required for the two webs, and it was found, also, that the rail heads could not be made of harder granular iron for this form of section, as was the practice for the other, and this resulted in a reduced life of the rail. As steel is the material for rails of the present day, the last objection does not apply, and the former objection loses its weight, as we are now called upon to provide a rail weighing from 80 to 100 pounds per yard, or about double 50, the weight of the bridge rails originally used. The bridge rail is much more stable than the T-rail, giving a broader base, hence taxing the spikes far less, and the bearing upon ties can be made as wide as desired. I have seen ties in a bridge floor spaced 1 foot between centers, so badly cut into by the rail, which was of a light pattern, that it was necessary to insert sole plates between the rail and the tie. We have doubled the weight upon the wheels as compared with former practice, but we have not increased the width of base of our T-rails an equal amount. Lastly, the bridge rail offers advantages in the splices not possessed by the T-rail, and would seem to solve the problem of an effective splice. This alone may be considered a sufficient gain to justify the change.

Sketch herewith (Plate LXVIII) shows an 85-pound bridge rail with two kinds of splices. For No. 1 no bolts are needed, or, if used, would have only a nominal duty to perform. No difficulty would be found in making the splice sufficiently strong for the purpose. The rail section shown has fully as much strength, to bridge the space between ties, as any T form of rail of same weight in use. The reason for this is that the extra metal in the two webs is compensated for by the head, which is shallow and wide, and the center of gravity of which is further from the neutral axis than in the case of a T-rail. As regards switches and frogs, there would be no difficulty in adapting the present forms of these devices to the new section. It must be said, however, that the rolling of this section would be somewhat more expensive than the rolling of the T-rail, but this additional cost would be small, and is more than made up by the advantages secured.

Onward Bates, M. Am. Soc. C. E.—The author of this paper has asked me to discuss it; a hard thing for me to do, because to my mind it should go without saying that wheels should be cylindrical and rails flat topped. If the paper advanced the converse of this proposition I think I could write a vigorous reply to it.

If there was no question of wear, I should favor conical wheels and round topped rails, for then the wheels would traverse a single element of the cylindrical rail top, and we might expect the circumferences of wheels to conform to the lengths of rails on curves. But wheels and rails do wear, and experience indicates that wheels in use soon lose their conical shape with the advantages claimed for it. With

conical wheels, the rails should be flat topped and inclined, so that on straight track the wheels should rest evenly across the whole portion of the rail top that is flat, and this inclination should be varied on curved track to fit the position which will be taken by the conical wheels. If it should be said that this latter requirement is more theoretical than practical, I would reply that the same criticism applies to the coning of wheels. With cylindrical wheels the rails should be flat topped and vertical.

Assuming, for sake of argument, that wheels and rails are incompressible, the area of contact between a conical or cylindrical wheel and a round topped rail is a point which geometry teaches us has no dimensions, and with the same wheel on a flat topped rail the area of contact is a line, with the one dimension of length. Consequently, according to Euclid, in the one case the wheel has nothing to stand on, while the other is a good line. With the compression that actually occurs there is a visible area of contact having dimensions of length and breadth, and which in Mr. Whittemore's experiments was of an oval shape. To obtain such contact displacement of metal must occur. This displacement is greater with the round topped rail, where the metal flows in every direction from a central point, than with the flat topped rail, where the flow is each way from a line. The total area of contact is small for the load sustained, and if the pressure varies over this area in proportion to the displacement, which I assume to be the case, the metal, where the displacement is greatest, must be strained far beyond its elastic limit, and the round topped rail, with its greater displacement, suffers more than the flat topped one. Immediate failure does not occur because the overstrained surface of the wheel or rail is supported by the mass of metal behind it, through which the pressure is diffused. Every loaded wheel traversing a rail strains the top surface of the rail beyond its elastic limit, causing surface wear. Another, and probably more serious result of this overstress, is deterioration of the metal in the direction of greatest pressure, as shown by the splitting of tires and of rail heads. Force acts in the direction of least resistance, and a tire or rail will inevitably suffer greater damage when the pressure is on a line or a narrow strip, than when distributed over a flat surface. One of my articles of engineering faith is that failure begins when the elastic limit of the metal is exceeded, and I cannot hold that faith and advocate a round topped rail. For those who believe differently I suggest a simple experiment. Take two strips of metal, one with a cross-section corresponding with the top ¼ of an inch of a round topped rail, and the other with the same portion of a flat topped rail; flatten them out by passing a roller over them in the direction of their length, and note the sults.

The question of the proper form for rails other than their top bearing surface is not here under discussion, and I have confined my remarks to

the relation of a wheel to this top surface. I believe that conical wheels and round topped rails are errors, which illustrate how long a fallacy may exist, and that it has existed so long confirms me in the belief that we shall yet see great and radical changes in our system of permanent way.

George S. Morison, M. Am. Soc. C. E.—The general substance of the argument contained in Mr. Whittemore's paper is, that the weights now put on the wheels of railroad rolling stock are so great that the elastic limit of the rails and of the wheels is exceeded at the point of contact, and that to reduce the wear the area of contact must be so increased that the pressure shall be brought safely within the elastic limit.

This area of contact can be increased in two ways; first, by increasing the diameter of the wheel; and, second, by increasing the length of the line of contact. As wheels are necessarily round, comparatively little increase of contact is obtained by increasing the diameter of the wheel. The surface contact, however, will increase directly as the length of line contact. Mr. Whittemore, therefore, proposes to use a cylindrical wheel, rolling on a flat topped rail, thereby securing a maximum length of line contact, obtaining exactly the conditions which are always sought for in rollers carrying extreme weights, or in machine finished work.

With perfect railroads, operated with the perfection of machine work, there would be no doubt of the correctness of Mr. Whittemore's proposition. Unfortunately no such railroads exist. The perfect railroad would be of uniform gauge, and without curves. On such a road Mr. Whittemore's cylindrical wheels would roll on his flat topped rails with a rate of wear which would probably hardly be appreciated. All friction and wear would be reduced to rolling friction and rolling wear, and the conditions would exist under which rolling wear would be reduced to a minimum. As soon, however, as we depart from this class of road new elements come in. On curves we could retain the flat topped rail, but it would become necessary to change the two wheels to the sections of one true cone, as is done in the wheels of turn-tables, while perfection could only be obtained by means of other impossible requirements.

The question before us is entirely a practical one. What we have to consider is how to obtain the maximum commercial value of the metal in rails and wheels. The case is not analogous to the strains in bridge work, where, by keeping the strains safely within the elastic limit, deterioration is practically avoided. It is admitted that rails and wheels must wear out. The question is, what is the principal source of wear, and in what shape the minimum amount of metal will endure the maximum of wear.

Leaving out the questions of the special strains produced by the pull of locomotive driving wheels, the wear of rails and wheels is due to two

causes; the rolling pressure which Mr. Whittemore considers so important, and the grinding action caused by the wheels slipping on the rail. If the rolling wear is the principal one, Mr. Whittemore's ideas should be accepted in the design of rails; if, however, the grinding action is the principal one, the form of rail should be sought in which this grinding would be a minimum.

As to the rolling wear, there are strong reasons for questioning whether elastic limit has as important an application in this respect as is commonly supposed. Every one who has tested the strength of metals knows how important the element of time is in all such tests. It takes time for metal, even under extreme strains, to develop the stretch or compression corresponding to those strains. In the case of a train running at high speed on a railroad track, this element of time is so much curtailed that the calculated compression in the rail head and in the wheel is undoubtedly very much reduced. The extreme strain does not exist long enough to bring about that distortion of material which has a permanent injurious effect, while the contact area is undoubtedly much less than calculations or experiments on wheels at rest would indicate. The action of thin ice under quickly moving weights has often been applied to structural conditions to which it bears little or no analogy; it does, however, bear a close analogy to the case before us. Thin ice will bear a heavy weight moving rapidly, simply for the reason that the ice can break only by the movement of the water under it, and there is no time for such movement. With an extreme rolling load on a rail or wheel, the metal cannot be extremely distorted, simply because there is no time for that movement to take place. Besides this element of time, the condition of shape has perhaps not been investigated as thoroughly as it should be, and it must also be remembered that the elastic limit can be varied very much by the process of manufacture, and that the direction on which it is called to resist weight in a rail is entirely unlike the direction on which tests are usually made.

On the other hand, all experience indicates that the wear of rails is due principally to the grinding action caused by a slip. A familiar illustration of this is the wear of rails on curves. If the track is laid with an elevation too great for the speed at which trains are commonly run, the outer rail is always found to wear out first, the wear being most rapid on the side where the slip of the flange produces the maximum grinding action; if, however, the curve has too little elevation the inside rail will always wear out first, the wear being approximately a flat wear on top of the rail, the rail usually assuming a shape which corresponds more nearly to Mr. Whittemore's flat topped rail than is to be found in any other portion of a track, and the wear being a maximum under these conditions. The explanation of this unequal wear is very simple. With too much elevation more weight is carried on the inside than on the outside wheel, and the latter slips. With too little elevation more weight is

carried on the outside than on the inside wheel, and the inside wheel slips; in either case, the wheel which has the greater weight on it rolls, and the one with the lesser weight slips.

The statistics collected by Mr. Wellington as to the wear of wheels, and contained in the Preliminary Report of the Committee, of which he and I are members, are principally valuable as showing how large a part the grinding action of the slipping wheel has to do with the wear of wheels and rails.

The conclusion which I individually draw from my own observations and from statistics, as far as I have examined them, is that the point to be studied is to produce a form of wheel and a form of rail head in which the slip shall be reduced to a minimum, believing that if we can once avoid the grinding effect of a slip we can stand the rolling wear of loads. In fact, I believe that the extreme loads now put on some of our car and locomotive wheels are objectionable, not from the wear they produce on the rail head, but from the effect on the lower portions of the track, the bearings on the ties, the spikes, the ties themselves, and the road-bed; on these their effect is so severe that I am strongly inclined to believe that a system of chair-laid track similar to that used in England may yet be adopted here.

To produce the minimum amount of slip, and, therefore, the minimum amount of grind, the line of contact between the rail and the wheel should be kept as nearly parallel as possible to the center line of the axle, and it should be no longer than is consistent with a reasonable approach to this condition. The former requirement is virtually the same as Mr. Whittemore's, but is reached from an entirely different line of reasoning. In practice, however, it must be somewhat modified, owing to the fact that wheels and tires will necessarily wear hollow so long as the width is greater than the width of the bearing, and safety makes this extra width absolutely necessary on every curved railroad. This brings us to precisely the conclusion given us in the Preliminary Report of the Committee, namely, that the head of the rail should be made with a large radius, but not absolutely flat—in other words, of a shape which will not produce exceptionally irregular results when the wheels become worn somewhat hollow; and, secondly, that the contact between the flange and wheel should be kept at as near the top of the rail as possible, which simply means that the upper corner of the rail should be given a minimum radius.

Of the three rails illustrated on the circular of April 20th, the best conditions seem to be made by Fig. 1, Michigan Central standard, the criticism which I am inclined to make of this rail being that the head is perhaps a little wider than is wise, and that a head 2¼ to 2⅜ inches wide would probably give at least equally good results. The objection to so narrow a head is, of course, the inadequate support it gives for the fish plate.

Mr. WHITTEMORE.—Mr. Morison's statement of stresses on rails in a curved track must be received as correct. With the outside rail elevated to suit the fast moving passenger trains, the preponderance of loads carried by the slower moving freight trains operates to crush the inside rail and cause slipping and consequent abrasion of the outside one. Both rolling resistance and friction of sliding and its abrasive effect are reduced by having the areas in contact such as to keep the pressure within the limit of elasticity of both wheels and rails, and it must be admitted that these resistances, when the pressures do not exceed the elastic limit, are nearly in proportion to pressures and not to areas of contact. It must also be admitted that to reduce abrasion, the wheel should have a tread of one radius, and not a varying radii, as the curved-topped rail hastens to make it.

Mr. Morison expresses the belief that our heavy loads on car and engine wheels are not objectionable from the wear they produce on the rail head, but are so in their action on the lower portion of the track. The author's effort has been to show that we do not destroy our rails so much through abrasion as by crushing them. Exceptional cases can be cited where great abrasion occurs, such as on steep gradients, where brakes are used to check velocity and sand is employed for additional driver adhesion, where switch engines are used in and near yards where sliding of wheels is common and sand abundantly used, and also along sharp curves elsewhere in the main line, as is abundantly shown by several of the diagrams furnished by our President.

Under our heavy loads the injury to the track by reason of the rail cutting into the tie is severe, and the remedy suggested by Mr. Morison should be earnestly considered.

Many of our railways have nearly 3 000 ties to the mile, while in England and on the continent only about 2 000 are used, but the latter are longer and wider than ours.

There is no doubt in my mind that through the use of sole plates of proper size and with longer and broader ties, a less number of ties will be required than we now generally use on our properly ballasted lines.

ROBERT W. HUNT, M. Am. Soc. C. E.—I have studied Past President Whittemore's able paper on "Cylindrical Wheels and Flat Topped Rails" with attention and much pleasure. In discussing it I approach the part in relation to wheels with less confidence than that pertaining to rails. We know that the augmented traffic has increased the wheel loads to an enormous extent. I called attention to this in my paper on "Steel Rails and Specifications for their Manufacture," presented before the American Institute of Mining Engineers, October, 1888, by giving the determinations of one fellow member, O. Chanute, M. Am. Soc. C. E., in 1881. To these Mr. Whittemore adds additional data.

It does not require any further argument to demonstrate that the rail of to-day must be of an increased section. It only remains to de-

termine the best form to give it. I am so thoroughly committed to a section having increased width rather than depth of head, and have so fully and repeatedly presented my arguments in its favor, that I will not here repeat them. I know you can obtain a better piece of steel in that form than when the head is deeper.

That increased wheel loads should require additional surface for contact, and hence support, seems to be self-evident. It has been proved that rails which originally had top curves of small radii have in service been worn to much larger ones. And it is also true that the earlier rails presented much greater surface in contact, in proportion to their loads, than later ones. And they also gave better results. It would, therefore, impress me as being true wisdom to go back toward successful practice, rather than continue in the direction we know has not, from some cause, given so satisfactory results.

Mr. Whittemore is radical in his propositions, and, no doubt, so tended. Of course, the rails of any railway system will be subjected to the wear from the rolling stock of the whole country. Hence until all roads adopt an universal standard for their wheels, we will not have constant conditions. But that is not a logical reason why the best forms should not be accepted, and used by somebody. There must be a beginning to all things.

In the sections which I have designed, and which I had the honor to present to the American Institute of Mining Engineers at their New York meeting in February, 1889, I adhered to a top radius of 12 inches, and did so more in a spirit of conservatism than because I believed it to be absolutely right. But less than that I cannot approve.

It is a little more convenient, on account of roll dressing, for the rail makers to have some curve to the top of the rails; but this is not a necessity. Flat tops can be rolled as well as flat bases. In fact, during my experience in rail inspection I have found one mill from which it has been a constant difficulty to obtain rails which were not rolled flat when the section had a head at all wide. In short, so far as the manufacture is concerned, and if flat topped rails are right, the makers can produce them without serious inconvenience.

I have been asked if it is absolutely necessary to have a middle "parting" to the rolls. In other words, whether this could not be on one corner of the rail head. If I remember correctly, A. J. Gustin—inventor of the rail cambering machine bearing his name—when in charge of the St. Albans Rolling Mill, St. Albans, Vt., so turned all of his rail rolls, and claimed much merit for the system. I have never known of any other mill having this practice, but it can be done. Still, as I have already stated, this would not be necessary to roll flat topped rails. We had them in service years before steel rails were invented, and of course produced on the slow running, old-fashioned, "two-high" mills.

We owe Mr. Whittemore much for his investigations and this con-

tribution to the literature on "Steel Rails." It is from such sources we must seek true information. Let the civil engineer design his sections, based on the safe foundation of experience; at the same time let him be willing to give due consideration to the criticisms and advice of the metallurgical engineer. The two acting in harmony will the sooner secure that which we all desire—the best.

Olin H. Landreth, M. Am. Soc. C. E.—The importance of the evils which Mr. Whittemore proposes to remedy, viz., undue wear of rails and wheels and the failure of rail heads before they have given more than a fraction of what may be considered a reasonable tonnage service, has never been more clearly set forth than in the paper of Mr. Whittemore and permits of no contradiction.

As to whether Mr. Whittemore's proposed form of rail top and wheel tread constitutes the most effective and the most feasible remedy attainable for these two distinct evils, is a question for discussion.

Individually I am inclined to the opinion that flat top rails (flat when laid) would constitute an improvement in reducing rail wear, but I do not recommend an entirely flat top rail, because I believe that would be overdoing the proper remedy, and would not insure so low a rate of wear as a rail top having a radius of curvature greater than 14 inches, but not entirely flat.

My reasons are based on the following considerations, which are confined to top rail wear alone, and exclude side rail wear as in no way effecting the special question of top curvature.

When a rail has worn to a permanent form of curvature it has reached a condition of nearest possible approach to a uniform distribution of wheel load, and hence the lowest maximum wheel pressure across the rail top, and therefore has attained its minimum rate of wear. Could a rail have been given at first a curvature which should remain permanent in radius, it would therefore insure a minimum rate of wear throughout the life of the rail. Such a curve should be sought, and when determined, should be adopted in the manufacture of the rail. A top curve, which should remain permanent throughout the life of the rail, would doubtless differ from the permanent curve assumed by a rail that had worn to its permanent form from a curvature originally sharper or flatter, since the form of the original curve would affect the form of the tread wear of the wheels rolling on it, and this form of tread wear would in turn effect and determine the form of the ultimate curve assumed by the rail top.

Car wheels rolling over rails ranging in original curvature from 6 to 10 inches top radius ultimately wear all rails on which they run to an average top radius of about 14 inches on tangents and about 10 inches on curves. Were all rails of a new system laid originally flat, and all car wheels running over them originally cylindrical, it appears impossi-

ble to escape the conclusion that the rails would ultimately wear to a curved form, and that the permanent curve assumed would be flatter in curvature than if the rails had been laid with the usual 6 to 10-inch radius. The grounds for these conclusions are :

First.—It would be impossible to lay rails, or mount wheels, or maintain either, so truly as to insure a uniform bearing of the cylindrical wheel on the flat top rail. Owing to this, there would result an augmented wheel pressure and an augmented wear toward either one edge of the rail or the other, or both edges, successively, according to the condition and rigidity of the rail-fastenings, and a curved form of the rail top would result.

Second.—Even were this impossible uniformity of wheel pressure across the rail attainable, the different portions of the rail top are not equally strong against wear under heavy compression; the portions near the edges having less metal outside would be deformed under lighter pressures than the central portions, and the same effect, viz., a curved rail top, would be the result.

Third.—The curve thus produced would gradually become sharper, until a permanent form should be reached, which, however, would be flatter than if the rail had been laid with the usual curvature of 6 to 10 inches radius, since the wheels starting originally with straight cylindrical treads, and being worn more by the outer portions of the rail top than by the inner, would remain straight across the tread, or if worn curved, would assume a flatter curve than if the rails had been laid with the usual sharp top curve; hence the permanent curve assumed by the rail—which is dependent on the curve of the wheel tread—would also be flatter than the permanent curve which the rail would have assumed had it been laid with the usual sharp curve, *i. e.*, the permanent curve would be flatter than 14 inches on tangents or 10 inches on curves. The exact value of this radius it is impossible to determine in the absence of experience with flat top rails, but it is hardly probable that it would be less than 20 inches under ordinary conditions of track or 30 inches where unusual precision in track laying or great rigidity of rail fastenings should exist.

If now a rail of 6 to 10 inches original radius wears flatter and assumes a permanent curve of about 14 inches radius, and a flat top rail wears sharper and assumes a permanent curve of from 20 to 30 inches, say 25 inches radius, it follows that the curve sought, viz., one that would wear neither flatter nor sharper, should have an original curve of radius between 14 and 25 inches; the precise value of this "radius of no variation," while falling between the above limits, could be precisely determined only by experience. I am strongly inclined to the opinion that rails laid with such a top curve, *i. e.*, one of about 20 inches radius, would result in a lower rate of top rail wear than either our present forms or Mr. Whittemore's proposed flat top rails, and would give equally low rates of side rail wear.

But I do not believe that rail wear proper is the only or the most important evil that reduces the life of our rails. Mr. Sandberg objects to the flat top rail from the "producers' point of view," for the following two reasons:

1. The heads of flat top rails would be more porous than curved top, because the flat top necessitates less pressure or work in the last roll groove, and hence less close grain than the curved top.

2. The heads of flat top rails would not deliver so well from the rolls and more "wasters" would be the result.

The first objection appears to me to touch the true root of the evil of undue rail wear, viz., a lack of density and hardness in the heads, but should not be directed against flat top rails alone and made a ground for their rejection, for the difference of density of rail head due alone to the form of curve of rail top must be quite insignificant, since it is measured by the versed sine of one-half of the top curve, which in the flat top rail would be zero, and in Mr. Sandberg's rail of 6 inches radius $\frac{1}{12}$ of an inch. In either form the density is mainly given—as in the perfectly flat base of the rail—by the pressure or work of the rolls acting upon the semi-fluid metal, and forcing it against the vertical surfaces of the rail grooves, which act simply as molds to shape the rail base and head.

As the rail metal at its passage through the last grooves is far from being perfectly fluid, the final pressure on the rail head and rail base is far from being equal to that produced by the rolls on the web and sides of the head. This remarkable fact, that the portion of the rail on which the heaviest and most severe service falls is the very portion on which the least pressure and work in the rolls is expended, points to the desirability of considering whether modifications or additions to our present mode of rolling rails may not be possible. As Mr. Whittemore remarks, "A way is always found to do what must be done," and when engineers of maintenance of way shall become satisfied—as I believe they must—that their rails, in order to render a reasonable tonnage service, must not only be so improved in top curvature as to increase the durability of the rail surface, but must be made proof against crushing and disintegration under our heavy wheel loads by acquiring physical hardness and density without sacrificing toughness, then the mechanical engineer and rolling mill manager may be depended on to meet the demand by improvements in manufacture; possibly not without increased cost of production; but the margin of possible improvement between the tonnage service that our rails actually average, and that which good rails under similar conditions are known to give, is so wide, that a considerably increased cost of production would be warranted in securing the higher service.

The second objection, concerning the difficulty of delivering flat top rails from the rolls, is doubtless a valid one, but one which would be

obviated by giving the rail any appreciable curvature which should release the horizontal grip of the rolls on any one portion of the rail head as soon as that portion should have passed the point of contact with the rolls, or as Mr. Sandberg says, "as long as the rail is not flat;" hence the 20-inch radius herein proposed would not be subject to this objection.

J. B. JOHNSON, M. Am. Soc. C. E.—Mr. Whittemore bases his tables of allowable wheel loads for given radii and unit stresses on formulas of Professor Grashof, which, as he gives them, are erroneous. These formulas are given as general, and suitable to any unit of measure, as the inch, the foot, the centimeter, etc. If this be so, then we should obtain the same result, whatever unit be used. Thus, if we use the foot unit, for E K and R, then the value of P found should be the same as when the inch unit is used, since this load is independent of any dimension. Taking the foot as the unit of measure, we obtain twelve times as large values from the first formula (cylinder on cylinder), and $\sqrt{12}$ times as large values from the second formula (cylinder on a plane). This proves the formulas to be defective somewhere.

Since the derivation of these formulas is simple, we may as well derive them at once and find the error.

RELATION BETWEEN TOTAL LOAD AND MAXIMUM STRESS, WHEN ONE CYLINDER RESTS ON ANOTHER AT RIGHT ANGLES TO IT.

It will be assumed that the maximum stress is within the elastic limit of the material, and hence that the stress varies as the distortion.

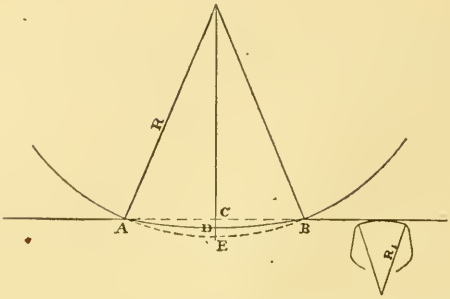
When one cylinder rests across another, the area of contact is an ellipse. The maximum stress is at the center and is proportional to the distortion at this point. The total distortion of one of the cylinders, if referred to a plane base, would be the segment of an ellipsoid, but so small a segment that it well might be considered the segment of a paraboloid. Similarly, since stresses are proportional to distortions, if the stresses over this area of contact be plotted from a plane of reference, the surface formed by their upper extremities may be taken as the segment of a paraboloid. Now, the average length of ordinate to a paraboloid is $\frac{1}{2}$ the length of the maximum ordinate, or the volume of a segment of a paraboloid is $\frac{1}{2}$ the inclosing cylinder. This volume would represent P, the total load, since any ordinate represents the intensity of the stress at that point.

We have, therefore,

$$P = \tfrac{1}{2} K A \qquad (1)$$

Where K = maximum intensity of stress, and A = area of surface of contact.

It remains to find the area of contact in terms of the two radii, the moduli of elasticity and the maximum stress.



In Figure, let $A\ C\ B$ represent the original surface of the rail, and $A\ E\ B$ the original surface of the wheel. These surfaces are now both coincident in $A\ D\ B$.

The length of $A\ C$ is, by geometry, equal to

$$\sqrt{(2\ R - \overline{CE})\ \overline{CE}} = \sqrt{2\ R\ \overline{CE} - \overline{CE}^2}.$$

Since $\overline{CE}$ is very small as compared to $2\ R$, we may neglect its square, and if we put $A\ C = d$, and $\overline{C\ E} = e$, we would have

$$A\ C = d = \sqrt{2\ R\ e} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (2)$$

Now $e = D\ E + C\ D$, and these distances are the distortions of wheel and rail respectively under the common intensity of stress K.

Now the formula for the modulus of elasticity is

$$E = \frac{Kh}{a}, \text{ or } a = \frac{Kh}{E},$$

where E is the modulus of elasticity;

K " intensity of stress per unit of area;

h " length of fiber under stress K;

a " distortion of fiber whose length is h.

But in our problem $D\ E = a$ for wheel, and $C\ D = a_1$ for rail, hence we may write:

$$D\ E = \frac{Kh}{E}, \text{ and } C\ D = \frac{Kh_1}{E_1},$$

$$\text{and } D\ E + C\ D = C\ E = e = K\left(\frac{h}{E} + \frac{h_1}{E_1}\right) \ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

Substituting this value of e in formula (2) we have:

$$d = \sqrt{2\ R\ \ K\ \left(\frac{h}{E} + \frac{h_1}{E_1}\right)} \ldots\ldots\ldots\ldots\ldots\ldots \quad (4)$$

This is the semi-axis parallel to rail of the elliptical surface of contact.

The semi-axis of the surface of contact across the rail would be

$$d_1 = \sqrt{2\ R_1\ K\left(\frac{h}{E} + \frac{h_1}{E_1}\right)}$$

where the subscripts refer to the constants of the rail instead of the wheel.

Now the area of the ellipse of contact is

$$A = \pi\ dd_1 = 2\ \pi\ K\left(\frac{h}{E} + \frac{h_1}{E_1}\right)\sqrt{RR_1} \ldots\ldots\ldots\ldots\ldots \quad (5)$$

and from equation (1) we now have $P = \pi\ K^2\left(\frac{h}{E} + \frac{h_1}{E_1}\right)\sqrt{RR_1}$ (6)

This is the first equation given in the paper under discussion, with the exception that where h and h_1 appear in this equation, he has unity. But h and h_1 represent the lengths of the compressed elements of rail and wheel, and when they are called unity, it means that these elements are one unit in length. If the unit chosen is the inch, then these filaments are assumed to be 1 inch long, but if the foot is the unit chosen, then they are assumed to be 1 foot long. With our formula (6) the result is the same, whatever unit is chosen.

The proper evaluation of h and h_1 in this formula is most uncertain from purely *a priori* grounds. They should be evaluated by experiment. There is no doubt but that the stress is rapidly spread over an area much larger than the area of contact, through the shearing stresses that are brought into action, and that consequently the maximum stress K, rapidly diminishes in value as we proceed from the surface of contact. The equation is derived, however, on the assumption that the vertical elements of the wheel and rail in contact are separate and distinct from each other, that they all have a definite length, h for the wheel, and h_1 for the rail, and that these lengths rest at their other extremities on absolutely rigid surfaces. All these assumptions are so far from the truth that it is really impossible to apply the formula to any particular case, except by the aid of experiment.

Relation Between Total Load and Maximum Stress, when a Cylinder Rests on a Plane Surface.

Referring again to the figure above, and understanding that the lower form is a plane, and taking a cylinder of one unit's length along its axis, and reasoning as before, we may say that if the stresses be plotted from a plane surface, the volume thus formed will be the segment of a cylinder, one unit long, the maximum ordinate being K, the maximum stress. Since this segment is very small, it may be taken as the segment of a parabolic cylinder, in which case its volume, which represents the total

stress, P would be $\frac{2}{3}$ the inclosing parallelopipedon, or $P = \frac{2}{3} K A$, where K is maximum stress, and A is the area of the surface of contact.

Referring again to the figure, we have $A =$ width $A\ B = 2\ \overline{C B} = 2\ d$, since the length of cylinder is unity.

As before, we may say, $d = \sqrt{2\ R e}$, where $e = \frac{h}{E} + \frac{h_1}{E_1}$

$$\therefore\ P = \frac{4}{3} K \sqrt{2\ R \left(\frac{h}{E} + \frac{h_1}{E_1}\right)} = \sqrt{\frac{32}{9} K^2\ R \left(\frac{h}{E} + \frac{h_1}{E_1}\right)} \ldots (7)$$

This is the same as Mr. Whittemore's second equation, except that we have h and h_1, instead of unity. This equation was derived by the writer as an improvement on one quoted by Mr. Charles Macdonald, M. Am. Soc. C. E., from the German, in a discussion in *Transactions* Am. Soc. C. E., 1874, p. 140; and it was published in the *Journal of the Association of Engineering Societies*, Vol. IV, p. 110, having been embodied in a paper before the St. Louis Engineers' Club, November 19th, 1884. In that discussion, however, the object sought was the bearing power of bridge rollers, and the lengths of the elementary columns (h and $_1$) in compression were both assumed to be equal to the radius of the roller.

Since friction bridge rollers are from 2 to 3 inches in diameter, the error in this assumption was small, the formula then being,

$$P = \sqrt{\frac{32}{9} K^2\ R^2 \left(\frac{1}{E} + \frac{1}{E}\right)} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (8)$$

The Above Formulas Practically Worthless.

It is now proposed to show that all the above formulas are absolutely worthless for solving practical problems. Since Mr. Whittemore's paper appeared the writer set about making some laboratory experiments for the purpose of amassing data on which to rest a valid argument. A section of a 33-inch chilled wheel, one of a 44-inch steel-rimmed driver, and several short sections of rail were prepared for experimental tests of areas of contact in a 100 000 pound testing machine. Contacts were made with both wheels with loads varying from 5 000 to 60 000 pounds.

It was found that the area of contact varies directly with the load, or, in other words, the average intensity of stress $\left(\frac{\text{load}}{\text{area}}\right)$ is a constant for all loads.

The experiments are not yet complete (October, 1889), but it is believed this is very nearly the law. This is directly contrary to the deductions made from Fig. 1.

The law seems to be for 22-inch radius steel tire, on 14-inch radius steel rail,

$$A = 0.000012\ P \qquad (9)$$

where A is area of contact in square inches, and P is the load on wheel in pounds. What the maximum stress is cannot now be stated, but if the mean stress is about a constant for all loads, it is probable that the maximum stress is nearly a constant also.

Furthermore this maximum stress must be nearly twice the mean stress, since the maximum distortion must, of necessity (a geometrical necessity), be twice the mean distortion.

We may therefore say that the maximum stress is nearly

$$K = \frac{2\,P}{A} \tag{10}$$

and from (9) we obtain for a 44-inch steel driver on a 14-inch radius steel rail,

$$K = \frac{2\,P}{A} = \frac{2\,P}{0.000012\,P} = 167\,000$$

pounds per square inch.

This astonishing result seems at first incredible, but it cannot, in the writer's opinion, be successfully disputed. All the data of these experiments will, at an early day, be laid before the Society. A few of the more important conclusions only are given here. In the meantime it would be well for other investigators to make similar experiments. I have found that a little wet chalk or whiting rubbed on the surface to be placed in contact, takes the impression as well as anything, and is quite satisfactory. The impression can then be copied off on tracing paper.

This result leads at once to the study of the elastic limit in compression. Here is where we have all been blundering. There is no such thing as an unconditioned elastic limit in compression as there is in tension.

The elastic limit is the point where the material begins to permanently distort or flow, or where the particles begin to rearrange themselves in new positions.

Now, for compressive stresses, we have various conditions limiting the flow of the particles. For instance, a cylindrical column in compression is in a condition of free flow. There is nothing to hinder the lateral spreading action of the column as it shortens up. If we place a square-ended die on a flat surface and load it until it begins to indent that surface the material below the end of the die must flow laterally around the corners of the die to get out of the way. But this lateral flow is restricted by the surrounding metal. Here we have a restricted flow, and a much higher pressure is required before it will occur.

But when a sphere or cylinder is placed on a plane, or one cylinder on another cylinder, and loaded, the maximum stress and the greatest tendency to flow is at the center of the area of contact, and here the flow must begin. But this metal cannot find as near escape as in the case of

the square-ended die, for the pressure diminishes gradually and the flow must be through a longer distance before it can really escape. Here, therefore, is a case of still greater restriction of flow and still higher elastic limit.

I estimate that a grade of steel having an elastic limit in compression when tested as a column, with free flow of some 40 000 to 45 000 pounds per square inch, has a limit of about 100 000 pounds under a square-ended die, and of some 160 000 to 200 000 pounds under curved surfaces of as much as 20 inches radius.

Observations on this subject are also in progress at the Washington University Laboratory, St. Louis, and it is hoped they will also be made elsewhere.

The conclusion which I am coming to is, therefore, that the maximum stress in and also the elastic limit of steel rails under wheel loads is dependent on the radii of the wheel and the rail, and that the maximum stress is practically independent of the total load on the wheel. Also that the elastic limit is in the vicinity of 200 000 pounds per square inch for the ordinary locomotive drivers on the ordinary steel rails.

These conclusions are so contrary to the commonly received opinions on this subject that the writer hopes that by the time his forthcoming paper on this subject appears others will have made similar experiments, and can thus either fortify his conclusions or prove them erroneous.

Equivalent Surface of Contact.

When a wheel tread becomes worn or grooved, and the rail head becomes flattened somewhat, so that the surface of contact is extended laterally across the rail from 1 to 2 inches, the effect on the intensity of the stress is the same as though we had a flat wheel on a flat rail.

It is sometimes said that if a flat topped rail is the surface of least wear the rail would constantly approximate to this form. This is not true. If the rail is originally curved, and the wheel tread flat or conical, the final section of both will be a mean between the two original surfaces. And this is exactly what happens. The tread becomes grooved, and the rail becomes flattened, both approximating to a mean between the original curvature of the rail head and the flat tread. When they have both come to the same curvature, the effect is exactly the same as a flat tread on a flat rail.

If, now, both wheel tread and rail top should be made flat in the start, I see no reason why they should not remain so, except from the rounding off of the corners somewhat as a result of the grooved drivers shifting slightly in a lateral direction. If, when the rail is first laid, it cants slightly, it will soon adjust itself by cutting into the tie more on one side than on the other, from the increased pressure on the high side.

If a flat head cannot be rolled, then let the head be as flat as can be rolled, for certainly a wider surface of contact is of the first importance.

Mr. WHITTEMORE.—While I cannot know in what order the several criticisms will appear in print, I desire to say that through my absence from the country and also in the knowledge that Professor Johnson was investigating the subject experimentally, and possessing the desire that some of his results might appear in this discussion, the publication of the paper and discussion has been delayed, until this late day, and not through any fault of the Secretary.

If we admit that the able analysis of Professor Johnson is correct, it will then be seen that our excessive loads do strain the metal much beyond the limit of elasticity as usually determined, even when considered as static loads, and undoubtedly their destructive effects are largely augmented when under motion and subject to violent impact.

Dr. Grashof's formula, I believe, has been considered about the safest to use, as he does not take into consideration that any support is derived from metal outside of the space of contact, but he does advise that the modulus of elasticity to be used in his formulas be ascertained from experiments under similar conditions of stress. It has been usual, however, to use it as ordinarily determined.*

It must be conceded that any generalization derived from the application of formulas to observed areas of contact as usually made, must be received with considerable allowance for error. The insertion of paper or pigment between bearing surfaces for the purpose of securing impression of areas of contact, must necessarily show greater areas than the absolute contact, as these materials are themselves compressible.

Perhaps the most accurate method by which the law governing contacts of spherical or cylindrical surfaces, with like or plain surfaces under varied pressure, can be best determined is in the way Newton's colored rings are observed, in which case we are compelled to use glass or other transparent substance for the experiment. That celebrated philosopher showed that by pressure the dark spot at and surrounding the center of contact of a lens on a plane increased as the pressure was increased, and it seems to me that by observing this increase under varied pressures up to near the limit of elasticity, the laws governing increased area with increased pressure may be pretty accurately determined, and that this, with experiments on metals strained to beyond their elastic limit, may afford sufficient testimony to establish the laws governing contacts of metals.

It is hoped that Professor Johnson will be able to pursue his experimental inquiry further and give the results of his labors to our society.

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* Dr. Grashof cannot be considered the first, however, in this field of inquiry, and those who wish to know something of what has been done before his work was issued, are referred to papers by Kopcke in Deutsche Bauzeitung, 1869; Kubler in Zeitschrift des Vereins Deutcher Ingenieure, 1874; Stevart in Revue Universelle, 1874; and by E. Winkler in Gittertrager, 1875.

Granting his premises, then it is seen what enormous stress is concentrated on a point, in the case he cites. We are taught that the Almighty never created but one perfect man, hence, how impossible it is for man to produce metal perfectly homogeneous; there will be faults in a steel rail, such as specks, cinders, voids by piping, and from other causes, even when the greatest care is taken in manufacture, and these may be near to this great concentrated stress, and it is here that the metal's apparent limit of elasticity would naturally approximate to what it would show when tested without adjacent support, and it is also here that failure may be first expected.

We have ample evidence to show that the best of metal in rails does flow during repeated applications of stress; hence it is that I offer my plan to avoid, as far as practicable, these concentrations.

Since an abstract of my paper was published, it has received criticisms in our engineering journals. One of these critics has intimated that by test he has never been able to detect a permanent depression on a rail from an over-loaded wheel, or words to that effect.

To such as may be influenced by such reasoning I respectfully suggest this line of thought:

We will suppose that a line is laid with what all will concede a very poor quality of rails, such as show a wear or depression on tangent rails of say one-half inch to a tonnage of ten millions only. This will give five million tons to each rail. Now suppose the wheels passing over the rail to be loaded to five tons, this would indicate that each loaded wheel as it passed would depress the metal one two-millionth of an inch. Have we any means by reflectors or otherwise for measuring such small increments?

While in the act of dictating the above the following communications are handed me, and with permission of the authors I insert them here as evidence pertinent to the subject, and in support of my position.

CHICAGO, ILL., October 11th, 1889.

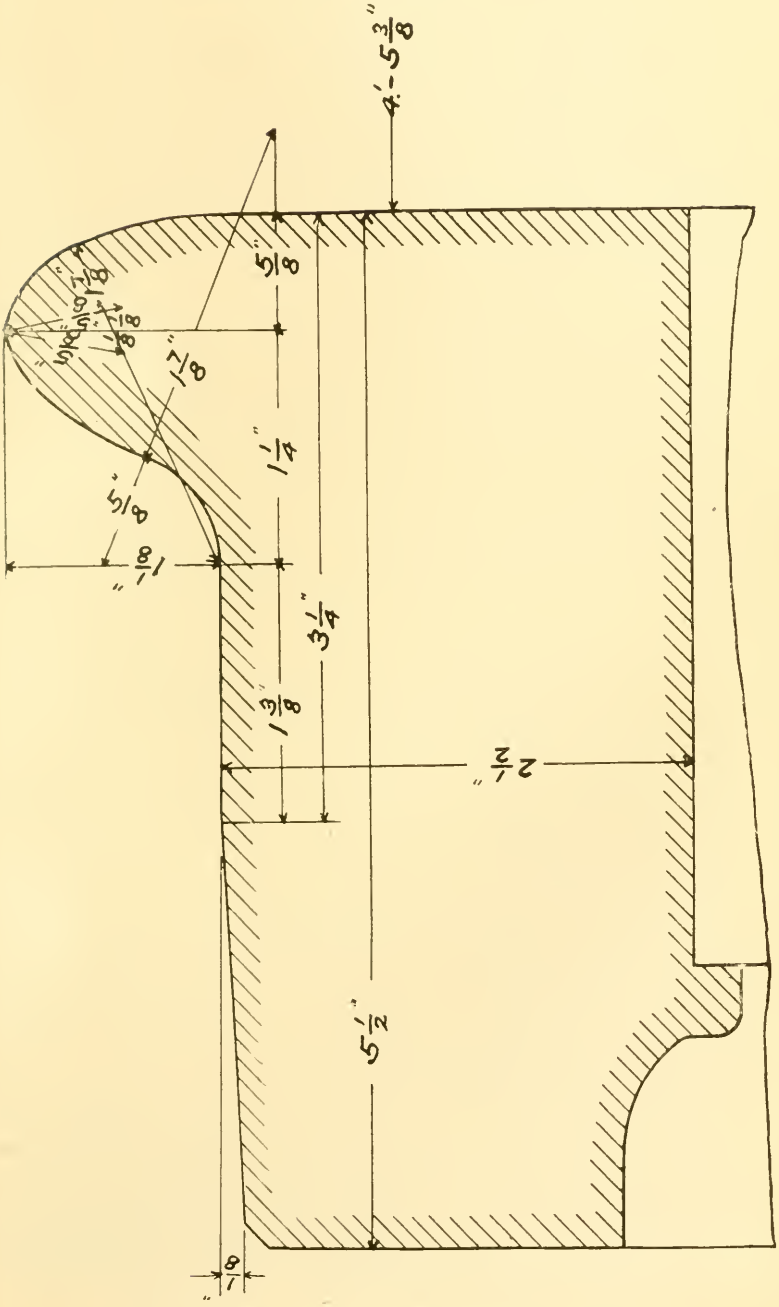
D. J. WHITTEMORE, Esq.,
Milwaukee, Wis.:

Dear Sir,—I am considerably interested, not as an expert, however, in the question of conical or coned wheels and the proper shape of rails on which they run. Sometime ago I went from Chicago to Albany with Mr. Edward W. Jackson, General Manager of the Mexican Central, in his private car. The wheels were cylindrical wheels, and the car rode very nicely, and I had quite a conversation with him in regard to wheels and rails. He is decidedly of your opinion, and I have asked him to send me some data with a blue print showing his idea of what wheels and rails should be. I have the letter and blue prints. Please find enclosed the print, which after examination and use kindly return to me.

Plate LXIX only shows wheels; he is in favor of rails to correspond.

Yours truly,

(Signed), E. L. CORTHELL.



STANDARD TIRE FOR CAR WHEELS, MEXICAN CENTRAL RAILROAD.
Mexico, June 25th, 1883.—Scale: Full Size.

Approved by EDWARD W. JACKSON,
General Manager.

Plate LXIX.

MEXICO, September 27th, 1889.

Dear Mr. CORTHELL:

I have the pleasure to acknowledge receipt of your letter of the 5th inst.

I had not forgotten the question, but have been so extremely engaged since my return that I have omitted to attend to it.

I now send you blue print showing the tires as we turn them.

You will observe we have $1\frac{3}{8}$ inches of flat cylindrical wheel, bevelled off to the outside edge of same, so that we have $2\frac{1}{8}$ inches of cone and $1\frac{3}{8}$ cylindrical tread.

You had an opportunity of seeing how my car (which is a light one, only 51 feet long) and four-wheeled trucks rode behind fast trains on which you and I traveled.

I also mentioned to you that several years ago I had occasion to note this on an old car on the Mexican road.

I sent to the United States for paper wheels from the Allen Wheel Company, which were put under my car, and she whipped so terribly that I was obliged to set her out at the first station from Mexico.

I spoke with the Master Car Builder about it, and he explained the reason.

I then had the wheels turned down, and they have run admirably ever since.

When wheels are too much coned the least deviation from perfect track starts them rolling, and they keep it up frequently until another inequality produces a similar result.

(Signed) EDWARD W. JACKSON,
General Manager.

F. COLLINGWOOD, M. Am. Soc. C. E.—As others have had more recent experience than I on the general subject of rails, I shall confine my comments on the valuable paper by Mr. Whittemore to the single question of the state of the rail when under pressure.

It has been stated that ordinary compression tests do not apply in this case; and as a proof it is said that a beam of light reflected from the rail shows no change in the rail surface when the latter is pressed by a heavily loaded wheel.

One cannot examine the several sections of used-up rails illustrated in Mr. Whittemore's paper without being struck by the very strong indications of a splitting action having resulted from the applied load. Now we have a precisely parallel case in the use of hooks to chains and sheaves for hoisting heavy weights. The area of contact of the inside of the hook with the inside of the ring or wires is doubtless still more limited than in the case of the wheel and rail.

In the case of the hook we know there is a gradual deterioration of the metal from continued use, so that the hook becomes unsafe. It would seem as though each time it hoists a heavy load a certain line of the molecules are left in a state of strain, as if a wedge had been partially driven; and this action continued finally results in rupture. As an illustration I would mention a hook I saw break under about 7 tons oad, which had repeatedly hoisted 11 tons. This is so well recognized

now with cranes in constant use that it is customary to anneal all such parts at stated intervals, to restore them to their normal condition.

Now the surface of contact spoken of by Mr. Whittemore with an area of about 1 square inch is not, from the nature of the case, uniformly pressed; and we are not justified in assuming that some portion of it is not left in a state of strain. If it is, then it is only a question of time when disintegration in some form will make itself manifest. If such action does occur we ought to have exactly such sections resulting as those illustrated in the paper, and I see no reason for trying to dodge the conclusion.

Having gone thus far, we come to the question, is it practicable to devise a form which shall remove or lessen this danger? We here trench on the province of the rail-maker. The form suggested in the paper would certainly be practically free from the splitting action suggested; and there remain two questions to be answered concerning it: 1st. Can it be rolled? 2d. Will the metal in the head be in as good a state to withstand wear—that is, will it get its due share of work in the process of rolling?

It is quite possible that some radical change in practice at the mills might be required to give an affirmative answer to both these questions, but I would like to see it tried. We shall never get nearer to a perfect rail by doing nothing but theorize about it. Is it impossible to roll the wearing surface of a rail?

J. M. Goodwin, M. Am. Soc. C. E.—I submit my opinion that, let the shape of the "corner" of the rail be what it may, persistent jamming and grinding of a wheel flange against the rail will have the effect of cutting away, and "sharpening," the flange so subjected to grinding; and that a rail corner having a distinctly "large" radius will, in time, put a "knife edge" on the flange, while the corner of less radius will wear more into the fillet of the flange. A rail having a "square" edge would, probably, wear the inside of the flange into a vertical plane. To the Master Car Builders, sitting in Convention at Chicago, in 1883, an English railroad man (invited to address them while the subject of "sharp flanges" was under discussion) put this question: "Did any one of you ever see two 'sharp' flanged wheels on one axle?" After "pausing for a reply," which he did not elicit, the gentleman proceeded to state that in several years of observation, in his capacity of superintendent of rolling stock, he had never seen two sharp flanges in association; and that when you see a sharp flange, you may be quite sure that the wheel, on which it is, has, or had, a mate of circumference greater than its own.

The fact is obvious, that if of two car wheels, fixed on one axle, one is "larger" than the other, the larger wheel will "drag" the smaller, and will operate to jam the leading flange of the smaller wheel against its rail; and we may reasonably conclude that a flange so jammed, and ground, against a rail, will, in due course, wear "sharp."

In case of mismatched wheels, the forcing of the smaller wheel to the rail is, presumably, persistent; reversing the direction of movement of the car merely reverses the direction of cut of the wearing surfaces, of flange and rail respectively.

In any case of "sharp flange," the circumstance that only one flange in a truck is affected is diagnostic of "big wheel." Where diagonally opposite wheels show undue flange-wear, one may expect to find the truck, in which they are, "out of square," or rigidly fixed at an angle with the draft line. Cases of absolute center rigidity of truck are not entirely unknown to the experienced car-repairer; rigidity due to excessive side bearing, caused by uneven loading, or by over-loading of car, announcing itself through a harsh sound of grinding of flanges and skidding and twisting of wheel-treads on the rails, is to be detected by the practiced ear in almost any train of loaded cars. It is the cause of much flange grinding, and, of course, of much waste of locomotive power.

Considerations of convenience induce establishment of straight tracks at places at which cars are to receive cargo. This is a happy circumstance, inasmuch as it operates to produce squareness of the trucks with the car body, and track, pending the operation of loading; whence, if rigidity is produced by overloading, or side-loading, the truck is fixed straight with the car.

Load an eight-wheeled car, on a smart curve, to the right, for instance, and, by any means, fix the trucks, or either of them, so that prompt response thereof to flange pressure, tending to traverse the truck, is impracticable. Then at the first curve to the left encountered by the car the right hand leading wheel of the cramped truck will climb over the rail, except, indeed, the corner of the rail has a distinctly small radius; in which case the flange may not be able to get a "climbing hold." If the wheel cannot escape by climbing, fillet grinding results.

In view of the fact that while of eight wheels under a car seven run without developing sharp flange nor any tendency thereto, while the flange of one wheel of the eight wears "sharp" or is ground out in the fillet, we may reasonably conclude that the one flange has been, recurrently or constantly, subject to extraordinary wear, referable to some condition affecting it particularly. This conclusion is, of course, irreconcilable with any theory in which the shape of the corner of the rail appears as an active, positive, value.

I have only time, now, to say, further, that I advocate cylindrical wheels and flat top rails now, as always heretofore.

Mr. D. J. Whittemore.—The author decidedly dissents from the conclusions of some who have participated in this discussion, in the plea that having track slovenly laid and rails poorly inspected, we must have a curved top to the rail to secure a bearing at or near

the center of the rail head; also that the head, if made flat and subject to pressure of cylindrical wheels, will soon acquire a curved form between corner curves to any appreciable extent, through the lateral movement of the wheels of about one-quarter of an inch either way from their central or normal bearing on tangent rails. When the rails are laid as nearly true as is reasonably practicable, any slight defect in the plane of rail surfaces would soon be corrected, as has been stated by Professor Johnson.

No doubt it is of paramount interest to secure the manufacture of rails without external or internal defects; this is admitted. There can be no doubt that a great error was committed some years ago in advocating the use of very mild steel.

When we can secure a steel that has a limit of elasticity of not less than from 50 000 to 55 000 pounds to the square inch, and which will undoubtedly entail a corresponding reduction of the modulus of elasticity, much benefit will result, as can at once be seen if any of the formulæ cited in this paper and discussion be received as approximately correct. With a bearing over the entire flat top of the rail head, it becomes of the utmost importance that all parts of the head be properly supported. Hence, is here found one of my objections to the Sandberg pattern, and it is the belief of the author that we should return to an approximation to the old pear shape in forming that portion joining the head with the web, and in the event that our loads increase within the next decade in anything like the proportion they have in the last, we may find it economy to adopt a form of rail having two webs, like unto the Strickland or Brunel rail.

Intimately connected with the subject of rail forms is that of the splice, and it is indeed impossible to consider the rail problem properly without reference to it; but as that matter is beyond the immediate scope of the paper under discussion, no further reference will be made to it other than to say that the author is of the opinion that the flange splice as generally designed presents about the most ugly appearance, mechanically considered, of all our railway appliances.

Objection has been raised to the form of rail head suggested, on the ground that it cannot be rolled and secure that compactness in the head that can be secured in the ordinary forms.

The opinion of our member, Mr. Hunt, who, by his experience of over thirty years in the manufacture of rails, can, I believe, be considered the superior of Mr. Sandberg as a manufacturer, and he can also be considered his peer as an inspector, is deserving of consideration in the matter of the practicability of rolling the form suggested.

The author is informed by Kennett Bayley, M. Inst. C. E., and Chief Engineer of the Great Southern and Western Railway of Ireland, that some seven years ago he changed the radius of his rails from 9 inches to 18 inches, and the result has been most gratifying in giving increased

service, and to his knowledge no English rail manufacturer has objected to the form.

The author, in his endeavor to compound the evil, has lately designed two patterns of rail, each having a top radius of 18 inches, and no manufacturer has objected to the guarantee of service that is applied to the usual design. After diligent inquiry, the author has failed to learn that any objection whatever has been made by manufacturers of the flat headed rails of the Northern Railway of France; hence the author concludes that the idea of want of compactness or sponginess in the heads of such rails is a myth, existing not, however, in the head of a flat topped rail.

During the time the author's paper has been under discussion, the final report of the Committee on the proper relation to each other of the sections of railway wheels and rails, has been made, and he may be pardoned in quoting from the preliminary and final report the following passages; the author calls especial attention to the words italicized by him:

From the preliminary report, page 42.

Your Committee are not clear that even if we consider the top bearing only, when no question of consequent rubbing friction comes in, it can be stated without many allowances, *that increase of bearing surface is in itself an advantage. * * * They are rather disposed to believe that it would be entirely disadvantageous.*

From the final report, page 4.

The prevailing sentiment, *which your Committee shares*, appears to be that the end sought in the suggestion of an entirely flat top between the rounded corners, viz.:—a wider bearing for the wheel tread, is a *desirable one*, but that it is and will be better secured in practice by using a slight curve for the top of the section instead of a straight line.

As between the preliminary and final report, an important advance in the right direction is made in the latter, and now only two more modifications are required to fully comply with the views of the author, viz:—that the broad surface of the rail-head be flat and that wheels be cylindrical.

In conclusion, the author desires to say that, knowing the persistency of evil, particularly when fortified by precedent, he fully expected to find himself the "under one" in this controversy. That so many agree with him in whole or in part is certainly gratifying. At the same time, his hearty thanks are due and given to those whose opinions are adverse to his premises, for the labor, ability and interest they have shown in this discussion.

If the paper and criticisms it has received will tend to arouse an interest in the matter treated, and cause the younger members of our profession, those who will soon be called to direct and design, to think of it—and by this expression I do not mean that they merely "think they think," but put absolute thought into the investigation—the author's desire will be in a great measure accomplished, and in the end, as he believes, his position vindicated.

ADDENDA.*

Modification No. 10, February, 1888.

3d Division.—Construction and Inspection.—Compagnie du Chemin de fer du Nord.—Specifications for Furnishing Steel Rails.

Vignole Patterns...	43 K	37 K	35 K	30 K
	94.8 lbs.	81.6 lbs.	77.2 lbs.	66.1 lbs.

Article I.

These specifications are intended to govern the delivery of steel rails.

Article II.

The rails will be of the American, called Vignole, pattern. They will conform exactly to a template which will be furnished the contractor. There must be no variation in the entire length of the rail from the above template, and particular care must be taken in cutting to length so as not to alter the end sections.

To allow for wear of rolls, and errors in setting of rolls, a deviation of 0.0005 meter (equals $\frac{1}{64}$ inch) will be allowed for the transverse dimensions of the rail, but in all cases the section must be symmetrical.

All rails not conforming exactly to template, within the above prescribed limits, will be rejected.

The standard weight per running meter will be determined by taking the average weight of one hundred of the rails first delivered, whose sections conform most nearly to the templates. A variation of 2 per cent. over or under the standard weight thus established will be allowed. Rails falling more than 2 per cent. below the standard weight will be rejected. Those being more than 2 per cent. above the standard weight will be accepted, but the contractor will not be allowed for any weight in excess of 2 per cent.

If the total weight furnished on a contract exceeds the estimated total standard weight by more than 1 per cent., the contractor will not be paid for this excess.

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* See Discussion by D. J. Whittemore, p. 169.

Article III.

The rails will have the lengths given in the following table, or such lengths as necessity may require.

Lengths of Rails for different Weights of Section.				Remarks.
30 Kil.	35 K.	37 K.	43 K.	
Standard Lengths.				
8 M.	6 M.	6 M.	12 M.	To be delivered in quantities not exceeding one-twentieth of the contract, as may be determined by the Chief Engineer of the company.
7.96	5.96	5.96	11.91	
Short Lengths.				
7 M.	5 M.	5 M.	10 M.	Lengths which will be allowed to facilitate the manufacture. The quantity of short length rails may vary at the option of the Railroad Company from one-twentieth to one-twenty-fifth part of the whole contract, a proportionate quantity of each length to be furnished.
6 "	4 "	4 "	8 "	
5 "	—	—	6 "	

The Railroad Company reserves the right to order rails of a greater length than specified above.

There may be a variation of 0.002 meter ($\frac{5}{64}$ inch) in the length of a rail.

Article IV.

The rails will be marked, in plain raised letters and numbers, with the name of the mill, year and month of rolling. These marks will be cut into the finishing rolls.

Furthermore, near one end of each rail, the number of the charge and the quality of the steel in the ingot from which the rail was rolled will be marked with a cold chisel.

Article V.

The rails will be made from non-phosphorous metal by the Bessemer process; any other process of manufacture must first receive the sanction of the Chief Engineer of the Railroad Company. The operations of manufacture will be so conducted as to produce steel of the first quality, fine grain, compact, hard and tough, capable of being tempered, and similar in every respect to the sample which will be furnished to the Railroad Company by the contractor. The manufacture in quantity will not be commenced until the sample has been accepted by the Chief Engineer of the Railroad Company.

The steel will be run into ingots of rectangular section, not being less than 0.25 meter by 0.25 meter (9¾ by 9¾ inches), with rounded corners, or of circular section of at least 0.28 meter (11 inches) diameter. The size of these ingots will be so determined as to produce, when rolled, a bloom of 1 meter in length, for rails rolled in single lengths, and of 1.20 meter in length for rails rolled in several lengths.

The ingots will be examined with care, and those having blow-holes, impurities or other defects which rolling cannot eliminate, will be rejected. Small cavities and seams will be dressed with the greatest care, so as to avoid any possibility of overlapping of metal in rolling.

The Company's agent will have an ingot broken as often as he may deem necessary during the progress of manufacture. The fracture should be exempt from blow-holes. However, the number of ingots thus broken should not exceed one-five-hundredth of the total number.

The Company's agent will have the right to inspect the tests made on the runners taken at the time of pouring the ingots, from which the rails, subject to his control, are to be rolled. The rolling will be conducted in a manner to obtain rails of exact form, with smooth and uniform surfaces, and especially to avoid bending when the rails leave the finishing rolls. Those rails showing cold shuts, kinks, or flaws of any kind, will be rejected. Only small flaws which do not alter the shape of the section, or affect the strength of the rail, will be accepted, on condition that these flaws, after having been passed upon by the agent of the Company, are carefully dressed with a cold chisel and fine file, in the presence of the said agent.

All other patching up in a cold or warm state is forbidden. Rails not fulfilling the above requirements will be rejected.

Article VI.

The rails will be straightened on four sides with the greatest of care. The straightening will be done as much as possible when the rail leaves the finishing rolls in a hot state. If it becomes necessary to further continue the straightening operation, when the rails have cooled, it must be done by a gradual pressure, and without shock, so as to avoid creating any fissures in the rail. Dies of proper shape will be used to protect the flanges.

The rails, immediately upon leaving the finishing rolls, will have the crop ends cut off with a circular saw. The cutting to exact length may be done with a milling machine, or in any other way which will insure perfect work. The ends thus cut shall have perfectly plane surfaces, square to the axis of the rail. The heads at each end of the rail will be chamfered 0.0015 meter by 0.0015 meter ($\frac{1}{16}$ by $\frac{1}{16}$ inch) for a depth of 0.006 meter (¼ inch) from top of rail.

The burrs created in cutting will be carefully removed with a cold chisel and file. It is absolutely forbidden to hammer them off.

Reheating the ends of the rails and then cutting them with saw or shears is absolutely forbidden.

Article VII.

The rails of main line will be drilled and notched according to standard, or only drilled, as may be required. For rails of extraordinary length there will be special requirements.

The holes must be drilled in web, perfectly cylindrical, and of such diameters as directed. All burrs around the holes will be removed with the cold chisel and file. The positions of holes must not vary more than ½ millimeter ($\frac{1}{64}$ inch) from those prescribed.

Article VIII.

The rails will be classed as they come from the mill, from day to day, into series. The authorized agents of the Company will select from each series a number, not exceeding 2 per cent., for the following tests.

Each of the rails will be placed over two points of support, 1 meter (3.28 feet) apart, and must carry, for five minutes, suspended at the middle between the two supports:

First.—Without permanent set after removing the load.

26 000	K. for the	43	Kil. type		*Note.*—1 kilogram=2 205 pounds.
20 000	" "	37	"		
18 500	" "	35	"		
17 000	" "	30	"		

Second.—Permanent set must not exceed 25 millimeters (1 inch) at:

40 000	K. for the	43	Kil. type.
35 000	" "	37	"
33 000	" "	35	"
30 000	" "	30	"

The rails may then be loaded to destruction.

Each of the two halves of a broken rail, placed on its side, between two supports spaced 1.1 meters (3.609 feet) apart, and fastened to a cast-iron base weighing 10 000 kilograms (11 tons), the latter resting on a block of masonry 1 meter (3.28 feet) high, and 3.3 meters (10.82 feet) square at the base, must endure, without rupturing, the impact of a hammer weighing 300 kilograms (661.5 pounds), striking the rail at the center between supports from the following heights:

3.00	meters height for the	43	Kil. type		1 meter=3.28 feet.
2.50	" "	37	"		
2.40	" "	35	"		
2.25	" "	30	"		

After taking the impact successively from the following Heights	1 M.	1.5 M.	2 M.	2.25 M.	2.4 M.	2.5 M.	3 M.	
	Mm.	Mm.	Mm.	Mm.	Mm.	Mm.	Mm.	
The deflections should not vary more than 30 per cent. from the figures here given	.....	2	4	.....	.....	9	15	For 43 K. type.
	1	3	6	.....	.....	11	.....	" 37 K. "
	1	3½	7½	.....	12	.....	.....	" 35 K. "
	2	5	11	16	.....	.....	.....	" 30 K. "

The elasticity of the rail will be judged by the rebounding of the hammer.

If one of the rails thus tested breaks before a drop height of 2½ meters for the 37 K., 2.40 meters for the 35 K. and 2.25 meters for the 30 K. is reached, the tests will be repeated with other rails, and if more than one-tenth of the number of rails thus tested does not fulfill the requirements, the whole heat from which the rails were taken will be rejected.

The Company reserves the right to prescribe, and have made at the mill, any other tests which it may deem necessary, such as tension tests and the working of the steel into tools, in order to judge of the quality of the rails.

The test pieces for tension will be prepared cold, according to the instructions given by the Company's agent, and will be taken from the blooms or from the rails which were tested for impact. Cold chisels manufactured from the head and tempered under ordinary conditions must cut the surface of spiegel iron without dulling, becoming dented or doubling up. These various tests will be made independently, and the results will be written down and testified to by the agent of the Company and the agent of the contractor.

Article IX.

The agents of the Company will select, as fast as rolled, such rails as satisfy the requirements, and have them weighed and drilled, according to the conditions stipulated.

The contractor will furnish all necessary apparatus for testing, and all necessary help to facilitate the inspection of the steel. The accepted rails will be drilled at both ends, and the rails rejected will be marked at both ends, between the letters of the manufacturers' brand, with a special mark, plainly visible and indelible, so as to avoid the possibility of having rejected rails offered a second time.

Article X.

The manufacturer will warrant the rails during six years, commencing from the date of manufacture. Every rail which breaks during this time, or is damaged more than it ought to be by regular usage, will be replaced free of charge by the contractor. The exchange of rails damaged for new rails destined to replace them will take place at the point of delivery indicated in the contract.

Article XI.

The engineers of the Railroad Company, as well as their assistants, will have free access to the mills day and night, and be permitted to inspect all operations pertaining to the manufacture of the rails, and to make all necessary tests to satisfy themselves that the conditions of these specifications are being fulfilled.

Article XII.

The inspection by the engineer of the Railroad Company or by his agents at the mill, the tests and the preliminary acceptance of the rails manufactured, will not, in any case, diminish the responsibility of the contractor, which remains in full force until the expiration of his time of guarantee, as provided for in Article X.

Article XIII.

This contractor will be governed, except where these specifications conflict therewith, by the general stipulations relating to contractors of the Railroad Company, compiled by the Chief Engineer of Ponts et Chaussees, in charge of construction and inspection, on the 8th day of November, 1863, and approved the 10th day of the same month by a committee of the Directors of said Railroad Company.

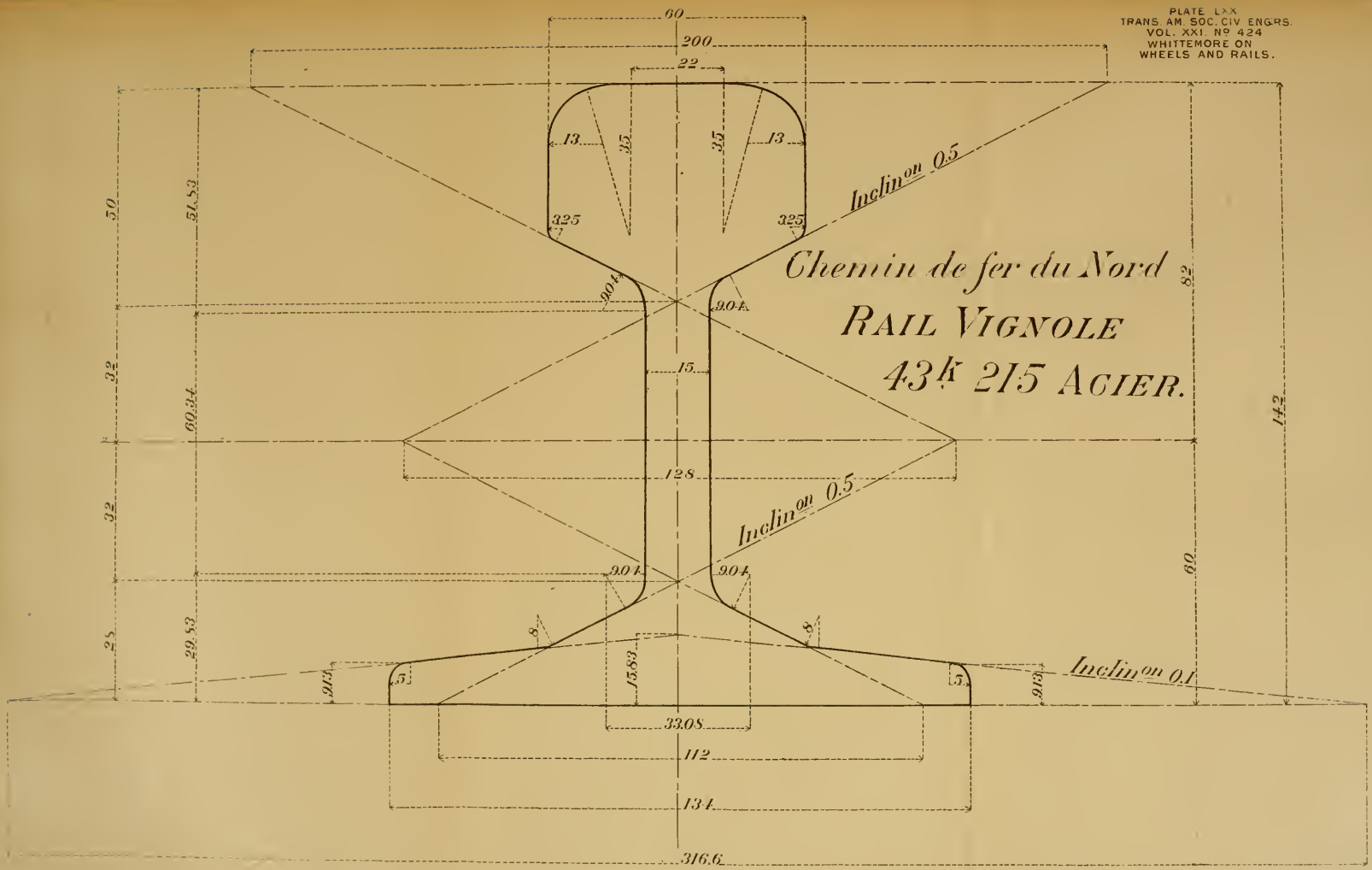
No deviation from these specifications or from the general stipulations will be allowed, except the same be authorized in writing by the Chief Engineer of the Railroad Company, which order the contractor must present at every requisition.

Compiled by the Chief Engineer in charge of Construction and Inspection, Paris, 1888.

Approved by the Operating Department.

Accepted by the undersigned contractor.

The section of rail is shown in Plate LXX.


Chemin de fer du Nord
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5
9.13
9.13
33.08
112
134
316.6
50
31.53
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60.34
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29.53
82
60
142

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

425.

(Vol. XXI.—October, 1889.)

FINAL REPORT OF THE COMMITTEE ON THE PROPER RELATION TO EACH OTHER OF THE SECTIONS OF RAILWAY WHEELS AND RAILS.

To the President and Members of the American Society of Civil Engineers:

Your Committee, to whom were referred the questions covered by the subjoined resolution,* has satisfaction in reporting that since the appearance of its Preliminary Report it has discovered by circulars of inquiry that a fair approach to unanimity of sentiment exists among engineers in regard to the questions involved. It now unanimously submits the following final report:†

* The Committee is acting under the following resolution offered at the Deer Park Convention, June 25, 1885:

"*Whereas*, The relation which the form of the head of a rail should bear to the section of a car-wheel tread and flange has recently been in dispute, it being asserted, on the one hand, that they should have as long a line of contact as possible, and, on the other hand, that such long contact would be dangerous and injurious; and,

"*Whereas*, The question raised by this diversity of view is of direct importance to the many members of this Society, as well as to the public interest, and hence is one which this Society may appropriately consider through Committee; therefore be it

"*Resolved*, That a Committee of five members of this Society be appointed by the President to consider and report to the Society on the proper relation to each other of the sections of rails and wheels; to what extent and at what points it is expedient that their sections should be such as to bring them normally in contact, and to what extent and at what points it is not expedient that they should be so in contact; and that the Committee be instructed to seek information from all those who are known to have given the subject attention."

This resolution was submitted to letter ballot and adopted by a general vote of the Society. Subsequently, by vote at the annual meeting, the President was authorized to add two additional members to such Committee. The Committee then sent out circulars of inquiry to all those deemed likely to be able to aid them by submitting definite records bearing on the question, and submitted a preliminary report giving in full all the valuable information thus gathered, with their preliminary conclusions based on a study thereof. (See Trans. Am. Soc. C. E., Vol. XIX, page 1.)

† At the Annual Convention, June 24th, 1889, this report was received, and the Committee discharged with the thanks of the Society for the very valuable work which they have done.

The Preliminary Report of the Committee was published in the Transactions of the Society for July, 1888. A copy of this report was sent to all members of the Society, and to about 250 others, railway officers and engineers not members of the Society, including all those known to the Committee as having shown interest in the subject in any way. Circulars of inquiry asking certain specific questions (reprinted in Appendix A) were sent out in the same manner to all persons in and out of the Society, some 500 in all, known or thought to be interested in the question. The responses have been numerous and valuable. Sixty-four responses, many of them very elaborate and complete, have been received from engineers, mechanical officers, rail manufacturers and others as specified in the subjoined list,* including a large majority of those whose views the Committee was most anxious to obtain. All the Eastern trunk lines and numerous other

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* The Committee have received discussions in response to their circular, most of them answering in full and giving reasons for their opinions, from the following persons, in addition to which a number of discussions were received not in answer to specific questions and hence not readily abstracted.

DECATUR AXTELL.....Receiver, Richmond and Alleghany R. R.
W. S. G. BAKER.....Prest., Baltimore Car Wheel Co.
OLIVER W. BARNES.....Ex-Chief Engineer, So. Penna. R. R., etc.
M. J. BECKER.....Chief Engineer, Pittsburgh, Cinc. and St. Louis Ry.
CHARLES BLACKWELL.....Engr. of Machinery, Georgia Central R. R.
J. BLICKENSDERFER .....Consulting Engineer, Union Pacific Ry.
VIRGIL G. BOGUE.....Chief Engineer, Union Pacific Ry.
J. M. BOON.....Supt. Motive Power, West Shore R. R.
M. F. BONZANO.....Supt., P. and N. Y. Div. Phila. and Reading R. R.
G. BOUSCAREN.....Consulting Engineer, Ex-Chief Engineer, Cincinnati So. Ry.
THOMAS J. BRERETON.....Supervisor, Penna. R. R.
WM. H. BROWN.....Chief Engineer, Penna. R. R.
R. E. BRIGGS.....Chief Engineer, Denver and Rio Grande Ry.
WM. BUCHANAN.....Supt. Motive Power, N. Y. C. and H. R. R. R.
M. BURPEE.....Chief Engineer, New Brunswick Ry.
EDWARD C. CARTER.....Prin. Asst. Engr., Chicago and N. W. Ry.
WILSON CROSBY.....Civil Engineer, Bangor, Me.
J. FOSTER CROWELL.....Civil Engineer, Ex-Engineer of Construction, Penna. R. R.
W. G. CURTIS.....Supt. Track, Southern Pacific Ry. (with other officers).
F. G. DARLINGTON.....Supt. Cinc. and Muskingum Valley R. R.
F. A. DELANO.....Chief of Bureau of Rail and Rail Joint Inspections, Tests and Records, Chicago, Burlington and Q. R. R.
M. M. DEFREES.....Engineer of Bridges and Buildings, Ohio, Ind'a and W. R. R.
P. H. DUDLEY.....Engineer of Tests, etc., N. Y. C. and H. R. R. R.
ROBERT FLETCHER.....Professor of Civil Engineering, Dartmouth College.
ROBERT FORSYTH .....Manager Union Works, Illinois Steel Co., Chicago.
WILLIAM FORSYTH.....Mechanical Engineer, Chicago, B. and Q. R. R.
GEO. W. GIBBS.....Mechanical Engineer, Chicago, M. and St. P. Ry.
G. B. HAZLEHURST.....Engineer of Bridges and Tests, Baltimore and Ohio R. R.
J. D. HAWKS..... .....Chief Engineer, Michigan Central R. R.
H. V. HINCKLEY.....Office Engineer, Atchison, Topeka and Santa Fé R. R.

great systems are represented in these responses or on this Committee. The discussions will make a large pamphlet, and they are accompanied by many drawings. It was impossible to do more than tabulate the specific responses without unduly delaying this report; but the discussions constitute such a valuable collection of practical information as to rails and wheels that your Committee recommends their publication in full.

Your Committee was surprised as well as gratified to find that, instead of developing the diversity of opinion which is the frequent result of such inquiries, and which the condition of present practice as to rail sections made especially probable, the responses show throughout the degree of agreement as to essentials above noted, amounting almost to unanimity in respect to the specific questions submitted; those, namely, concerning the form and general proportions of the head of

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R. HITCHCOCK.....Master Car Builder, Conn. River R. R.
WM. E. HOYT.....Chief Engineer, Rochester and Pittsburgh R. R.
M. G. HOWE.....Receiver, Houston, E. and W. Texas and Pacific Ry.
J. B. JOHNSON.....Professor of Civil Engineering, Washington University.
THOS. H. JOHNSON.....Prin. Asst. Engr., Pittsburgh, Cinc. and St. Louis Ry.
WM. R. JONES.....Gen. Supt., etc., Edgar Thompson Steel Works, Pittsburgh, Pa.
WALTER KATTÉ .....Chief Engineer New York Central and H. R. R. R.
LEWIS KINGMAN.....Asst. Engr. Atchison, Topeka and Santa Fé R. R.
O. H. LANDRETH.....Professor of Civil Engineering, Vanderbilt University.
L. F. LOREE.....Engr. of Maint. of Way, Chicago, St. L. and Pittsburgh Ry.
A. P. MAN, JR.....Gen. Manager, Silver Springs, Ocala and Gulf R. R.
HUNTER MCDONALD.....Engineer in Charge, Nashv., Chatta. and St. L. Ry.
MANSFIELD MERRIMAN.....Professor of Civil Engineering, Lehigh University.
JAS. A. MILLHOLLAND.....Gen. Manager, George's Creek and Cumberland R. R.
S. F. MORRIS.....Div. Engr., New Croton Aqueduct, New York.
JOHN NEWELL.....President and Gen. Man., Lake Shore and M. S. Ry.
H. W. D. OTIS.....Gen. Roadmaster, N. Y. C. and H. R. R. R.
E. PONTZEN.....Civil and Consulting Engineer, Paris, France.
EDWARD W. PENFIELD.....Eng. in charge Rail Records, C., B. and Q. R. R.
THOS. PROSSER & SONS.....Steel Tire Manufacturers, representing Krupp Works.
J. T. RICHARDS.....Asst Chief Engineer, Pennsylvania R. R.
A. A. ROBINSON.....Vice-Prest. and Chief Engr., Atch., T. and S. F. Ry.
S. W. ROBINSON.....Professor Mechanical Engineering. Ohio State University.
C. P. SANDBERG.....Rail Expert and Inspector, London, England.
WM. F. SEARLES.....Consulting Engineer, Cleveland, O.
R. I. SLOAN ....Chief Engineer, Manhattan (Elevated) Ry.
J. P. SNOW.....Civil Engineer, Woonsocket, R. I.
W. W. SNOW.....Prest and Gen. Man., Ramapo Wheel Works.
WATERMAN STONE.....Engr. and Supt., Providence, Warren and Bristol R. R., Providence, R. I.
A. J. SWIFT.....Chief Engr. R. R. Dept., Delaware and Hudson Canal Co.
G. H. THOMSON.....Bridge Engineer, N. Y. C. and H. R. R. R.
THEODORE VOORHEES.....As t. Gen. Supt., N. Y. C. and H. R. R. R.
H. D. WHITCOMB .....Chief Engr., Richmond and Alleghany Valley R. R.
W. HOWARD WHITE.....Ex-Asst. Chief Engr., C. B. and Q. R. R.
DON J. WHITTEMORE.....Chief Engineer, Chicago, Milwaukee and St. Paul Ry.

rails. This result has greatly encouraged your Committee in unanimously recommending, as it now does, the following details as standards for practice.

First.—YOUR COMMITTEE RECOMMENDS A TWELVE-INCH TOP RADIUS AS A STANDARD FOR RAIL-SECTIONS OF ALL WEIGHTS.

The discussions upon this question show so far as they answer it specifically:

In favor of 12-inch, "10 to 14 inches," etc.	36
[Axtell, Barnes, Becker, Bogue, Bonzano, Bouscaren, Brereton, Brown, Briggs, Crowell, Delano, Dudley, Forsyth (William), Gibbs, Hawks, Hinckley, Hitchcock, Hoyt, Johnson (T. H.), Jones, Katté, Kingman, Landreth, Loree, Man, McDonald, Merriman, Millholland, Newell, Penfield, Robinson (A. A.), Snow (J. P.), Stone, Swift, Thomson, Whitcomb.]	
In favor of 12-inch or less	3
[Forsyth (Robert), Richards, Sandberg.]	
In favor of 12-inch or somewhat more	5
[Burpee, Carter, Fletcher, Pontzen, White.]	
In favor of 12-inch by implication [Curtis]	1
Total favoring about 12-inch top radius	45
In favor of flat top	7
[Blackwell, Darlington, Defrees, Hazlehurst, Morris, Searles, Whittemore.]	
Prefers flat top [Howe]	1
Prefers 20 to 30 inches (Robinson, S. W.)	1
Total favoring flat top	9

The prevailing sentiment, which your Committee shares, appears to be that the end sought in the suggestion of an entirely flat top between the rounded corners, viz., a wider bearing for the wheel-tread, is a desirable one; but that it is and will be better secured in practice by using a slight curve for the top of the section, instead of a straight line. The evidence is that an entirely flat top is difficult, if not impossible, to roll properly; trials have resulted badly. Inevitable irregularities of track and the usual slight bends in the web, due to the rails lying on their sides when hot, would throw the bearing of the wheel-tread on one or the other corner instead of evenly across the top, causing early

flowage and rounding of section. The standard wheel-tread (shown in Appendix A) is now slightly coned, a perfectly cylindrical tread having been first rejected. While few hold that the coning has a sensible influence on curve resistance, the belief is general that coning does have a sensible steadying effect to diminish oscillation, and it is questionable if it would be wise to wholly dispense with it. Were the tread originally perfectly cylindrical, with an even bearing for the whole width of rail head, the necessary gauge play alone would tend to wear it hollow. Under existing conditions wheel treads wear slightly hollow, corresponding to about a 12-inch radius, very early in their lives. As a consequence, nearly all rails now wear down to a curved top on the outside as well as the inside of the head. (See Fig. 7 of the Committee's Preliminary Report, reproduced in Appendix A.) Were flat rails and cylindrical wheels universal on any one line, and were they thus made free from all tendency to wear to a curve, the constant interchange of cars would bring hollow treads on flat rails and straight treads on round-top rails. With the top curve the load is sustained centrally and transmitted directly downward through the web. Without it, one or the other corner will sustain the load in practice, and neither long in succession, causing a tendency to split off the side of the head if the metal be in any way defective. Most wheels being worn slightly hollow, the bearing is in practice now tolerably wide.

For these and other reasons, your Committee is entirely clear that the attempt to introduce a perfectly flat top is inexpedient.

Second.—YOUR COMMITTEE RECOMMENDS A BROAD HEAD RELATIVELY TO DEPTH FOR SECTIONS OF ALL WEIGHTS, taking care not to go too far in either dimension, especially in very large or very small sections, or to endanger the flange cutting into the joint.

The discussions upon this question show, so far as they categorically answer it (the Committee's question having been framed with reference to the proportions of the Michigan Central section, Fig. 1, Appendix A):

Unconditionally in favor of about that proportion..... 36

[Axtell, Barnes, Becker, Blackwell, Bonzano, Bouscaren, Crowell, Dudley, Fletcher, Forsyth (Robert), Gibbs, Hazlehurst, Hinckley, Hitchcock, Hoyt, Howe, (M. G.), Johnson, (T. H.), Jones, Katté, Kingman, Landreth, Loree, Man, McDonald, Merriman, Morris, Newell, Pontzen, Richards, Robinson (A. A.), Sandberg, Searles, Snow (J. P.), Swift, Whitcomb, White.]

Want head still wider in proportion	5
[Delano, Forsyth (William), Hawks, Penfield, Thomson.]	
Wants head slightly narrower [Carter]	1
Approves for generally light alignment [Millholland]	1
Approves by implication [Curtis]	1
Total approving above recommendation	44
Disapproving [Brereton, Brown, Crosby, Robinson (S. W.)]	4
Non-committal or doubtful answers	2
[Bogue, Stone.]	

Your Committee is much impressed with the weight of evidence and opinion tending to show that, in the effort to secure three ends in themselves desirable and proper, too little attention has been paid to metallurgical laws and processes of manufacture in designing rail sections. The tendency has been strong:

A.—To secure a broad bearing on the tie, by widening the base. At the same time:

B.—To save metal, by making the base thinner, and also the web; while:

C.—To increase tonnage life, additional metal has been placed on the top of the rail head. These ends, as stated, are in themselves good, but the result of seeking to attain them in this way is a great disproportion of mass in the different parts of the section; and this in turn requires that, in order to have the thinner portions of the section sufficiently plastic for the finishing passes through the rolls, the head must be so hot when it leaves the rolls as to be almost semi-fluid; and so the rolls do little of that effective compressive work which unquestionably is essential for properly consolidating and toughening either wrought-iron or steel. This unequal temperature after leaving the rolls also makes it much more difficut to insure that the rails shall be straight when cool, and hence results in more "gagging." As there exists also a constant and injurious tendency to roll even the thinner parts tolerably hot, in order to quicken the process, save making seconds, and reduce the number of passes, it will be seen how probable it is that the heads of many rails have been leaving the rolls in but little better molecular condition than when they entered them.

In European practice no such extreme conditions prevail. The double-headed or "bull-head" sections are nearly symmetrical about

their horizontal axis. Even in respect to the increasing proportion of flat-base American sections used in European practice, the tendency is to use narrower and thicker bases and thicker webs than have of late prevailed here. It seems probable that European practice has been in this respect more nearly correct than ours, and that our manufacturers have been at times suspected unjustly of using inferior metal and processes, when the real fault lay in the harder problem given them to solve, because of the form of the section. Your Committee knows of rails which chemical analysis and subsequent working under the hammer showed to be rolled from excellent metal, yet which showed a coarse, uneven fracture when broken, and proved almost worthless in service. An important contributing cause to the badness of many such rails may be insufficient allowance for the fact that hot metal, like cold, requires a certain time to flow, if its texture is to remain homogeneous; in other words, that the process of rolling must not be too fast; and your Committee does not regard a relatively wide head as in itself a cure-all, nor does it forget that many rails of quite unsymmetrical form have rendered good service; but it thinks it probable that better wearing qualities would be obtained by nearer approach to equality in mass and form of the base and head, while a relatively wide head also increases the top bearing surface.

Third.—YOUR COMMITTEE RECOMMENDS A ONE-FOURTH ($\frac{1}{4}$) INCH CORNER RADIUS, AS A STANDARD FOR SECTIONS OF ALL WEIGHTS.

The discussions on this question show, so far as they categorically answer it:

Explicitly in favor of $\frac{1}{4}$-inch radius	37
[Axtell, Barnes, Blackwell, Bonzano, Bouscaren, Brereton, Brown, Briggs, Burpee, Carter, Crosby, Crowell, Darlington, Defrees, Dudley, Fletcher, Forsyth (Robert), Forsyth (William), Gibbs, Hazlehurst, Hawks, Hitchcock, Hoyt, Johnson (T. H.), Katté, Landreth, Loree, Man, McDonald, Merriman, Morris, Penfield, Pontzen, Richards, Searles, Whitcomb, White.]	
In favor of $\frac{1}{4}$ to $\frac{5}{16}$-inch	3
[Becker, Thomson, Whittemore.]	
Prefers $\frac{5}{16}$-inch. [Delano, Robinson (S. W.), Snow (J. P.)]	3
Prefers by implication (sends section). [Newell]	1
In favor of $\frac{3}{8}$-inch	5
[Bogue, Hinckley,* Kingman,* Robinson (A. A.),* Howe.]	
Total favoring short corner radius	49
Explicitly in favor of $\frac{3}{8}$-inch radius or disapproving $\frac{1}{4}$-inch	5
[Baker, Millholland, Sandberg, Stone, Swift.]	

* Conditional upon fillet radius being the same.

Your Committee did not expect such unanimity in favor of a short corner radius. It can only say that it has especially endeavored, in every way, to secure discussions from those supposed to favor a long corner radius corresponding to the now standard $\frac{5}{8}$-inch wheel fillet. Only one such discussion has been offered; that of Mr. Millholland. The arguments on this question were given at length in the Committee's Preliminary Report, and the preliminary conclusions announced therein have been endorsed, as shown in Appendix A.

Fourth.—YOUR COMMITTEE RECOMMENDS A ONE-SIXTEENTH ($\frac{1}{16}$) INCH LOWER CORNER RADIUS FOR THE HEAD, AS A STANDARD FOR SECTIONS OF ALL WEIGHTS.

The discussions on this question show, so far as they categorically answer it:

Favor a very small radius (question was asked as to a $\frac{3}{32}$-inch radius; see Fig. 1, Appendix A) 31

[Barnes, Becker, Bouscaren, Brown, Briggs, Burpee, Carter, Crosby, Curtis, Darlington, Dudley, Hazlehurst, Hawks, Hitchcock, Hoyt, Howe, Johnson (T. H.), Jones, Katté, Loree, Man, McDonald, Milholland, Morris, Pontzen, Sandberg, Searles, Snow (J. P.), Swift, Whitcomb, White.]

Favor still smaller radius ($\frac{1}{16}$-inch) 5

[Hinckley, Kingman, Newell, Robinson(A. A.), Thomson.]

Total favoring $\frac{3}{32}$-inch or less 36

Opposed to so short a radius 7

[Blackwell, Bonzano, Brereton, Forsyth (Robert), Landreth, Robinson, S. W., Stone.]

"Possibly too small" [Bogue, Delano] 2

"Unimportant" [Axtell, Crowell,] 2

Non-committal 4

[Blickensderfer, Forsyth (William), Penfield, Richards.]

The purpose of the very short radius is, of course, to secure as much bearing as possible for the fish-plate, but your Committee is unable to see that it can do any harm even if no fish-plate be used. The evidence of the discussions from the rail-makers is, that there is no difficulty in rolling the section, even if the corner be a point, as some of the sections submitted have it. The specific section on which the question was based showed a $\frac{3}{32}$-inch radius, and it is impossible to say if as many would have approved it had it been $\frac{1}{16}$ inch instead; but from the general tenor of the discussions, your Committee feels satisfied that

they will do so now, and the principle of the short corner being approved, the Committee felt that it should recommend as short a one as was clearly practicable.

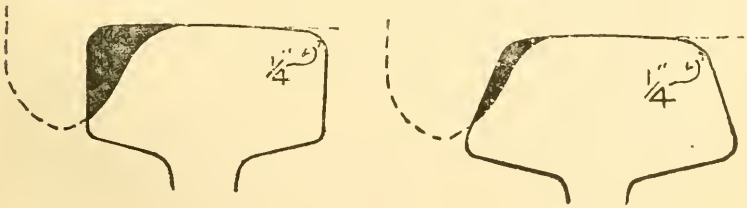
Fifth.—YOUR COMMITTEE RECOMMENDS THAT, STARTING FROM A SUFFICIENT BASE WIDTH OF HEAD TO GIVE AMPLE BEARING FOR THE JOINT, OR TO CONFORM TO RECOMMENDATION No. 1, THE SIDES BE CARRIED UP VERTICAL, AS A STANDARD FOR SECTIONS OF ALL WEIGHTS.

The discussions on this question, so far as they categorically answer it, show:

Explicity in favor of vertical sides	40
[Axtell, Barnes, Becker, Blackwell, Bogue, Bonzano, Bouscaren, Briggs, Burpee, Carter, Crosby, Crowell, Curtis, Darlington, Delano, Defrees, Fletcher, Forsyth (Robert), Forsyth (William), Gibbs, Hazlehurst, Hawks, Hinckley, Hitchcock, Hoyt, Johnson (T. H.), Kingman, Landreth, Loree, Man, McDonald, Merriman, Morris, Penfield, Pontzen, A. A. Robinson, Sandberg, Searles, Stone, Whitcomb.]	
In favor of vertical sides or nearly so [Richards]	1
" " " or an inward flare [Snow (J. P.)]	1
" " " or an outward flare [Howe, Katté]	2
" " " on tangents, slight incline for curves [Jones]	1
Total favoring in substance vertical sides	45
Opposed to vertical sides	9
[Brown, Dudley, Millholland, Swift, White; and wanting slight flare, 3 to 4 degrees: Brereton, Newell, Robinson (S. W.), and Thomson.]	

Starting from a proper base width, which appears the proper point to start from, and carrying the side up vertical instead of inclining inward, in the first place increases the top bearing surface, and in the second place increases the life of the rail by a trifling addition to the weight of the section, near the upper corner, where the metal is most useful and long-lived.

The accompanying cut shows in black the relative areas to be worn



away before the flange is bedded in the rail to the depth of one inch and hence approximately the relative tonnage life on the outside of curves; because, after the flange is once bedded in the rail to this depth, wear goes on at such an accelerated rate that the subsequent life of the rail, of whatever original section, amounts to little. By the vertical side, likewise, the difficulty about the gauging point is done away with, and the head is brought into a nearer approach to symmetry with the base.

The contrary practice starts with a given top width, and argues that by flaring out the sides there is more metal to be worn away before the rail becomes useless, while the bearing for the angle-bar is increased—which is true. But the added metal is put on chiefly at the bottom of the head, where it is least effective, and is cut away by the flange when it does reach it almost as fast as if it were lead. After a head has been designed with such an outward flare, by filling out the corner so as to restore the vertical side the life can be still further prolonged, as respects both flange wear and top wear, by metal very effectively disposed.

As respects any of the preceding recommendations, your Committee sees no reason why they will not give an equally favorable section for either curves or tangents, and either for angle-bar joints or for any form of base-support joint. An argument of some strength is advanced in favor of an inward flare for such joints, but your Committee sees no gain therefrom corresponding to the loss.

Sixth.—YOUR COMMITTEE ANNOUNCES AS FINAL CONCLUSIONS THE PRELIMINARY CONCLUSIONS AS TO THE GENERAL LAWS OF RAIL AND WHEEL WEAR INCLUDED IN ITS PRELIMINARY REPORT.

These conclusions, with a summary of what may be called the vote thereon, are appended hereunto in Appendix A. One or two verbal ambiguities, which seem to have caused misapprehension, are corrected; but beyond a few such verbal corrections the discussion has developed nothing which seems to indicate that any part of the Committee's Preliminary Report should be reconstructed.

Your Committee does not deem it proper to go beyond the above recommendations and attempt to elaborate details for standard sections complete. All that is included within the scope of its instructions it has now done. Anything beyond this must be done by a committee expressly appointed to recommend standard forms of rail sections, and

not merely to "consider the proper relations to each other of the sections of railway wheels and rails." But the fact that such unexpected unanimity of opinion has developed in respect to the proper form of the head of a rail section encourages the Committee to believe that a moderate amount of intelligent effort would secure the adherence of three-quarters, at least, of the engineers and railway mileage of the country to a set of standard sections elaborated after due inquiry and comparison of views.

Your Committee, having discharged the duty intrusted to it, now asks to be discharged.

Respectfully submitted,

H. STANLEY GOODWIN,
A. M. WELLINGTON,
SAMUEL REA,
GEORGE S. MORISON, } *Committee.*
THOMAS RODD,
S. M. FELTON, JR.,
JAMES ARCHBALD,

APPENDIX A.

TO REPORT OF COMMITTEE ON FORM OF RAILS AND WHEELS, ETC.

The Committee sent out on April 20th, 1889, the following Circular of Inquiry, supplementary to another circular of date March 18th, which was was substantially identical except as explained in the body of this circular:

The Committee desire to supplement their last circular, which showed only one section (Fig. 1 herewith), by submitting with it two other sections, the three together being typical of all the varieties of practice which exist. You will greatly aid the Committee in the work of preparing a final report (especially if you have not yet answered the first circular), by answering briefly the following questions in regard to each of these sections. The preliminary report of the Committee, giving all the facts which they could collect, and the preliminary conclusions which the facts seemed to warrant, was published in the Transactions of this Society for July, 1888.

Please state if you approve or not the following features of the rail section shown in Fig. 1 (a lately adopted standard of the Michigan Central Railway)?

A. The ¼-inch corner radius?
B. The 12-inch top radius?
C The vertical sides?
D. The very small lower corner radius?
E. The broad head relatively to depth?

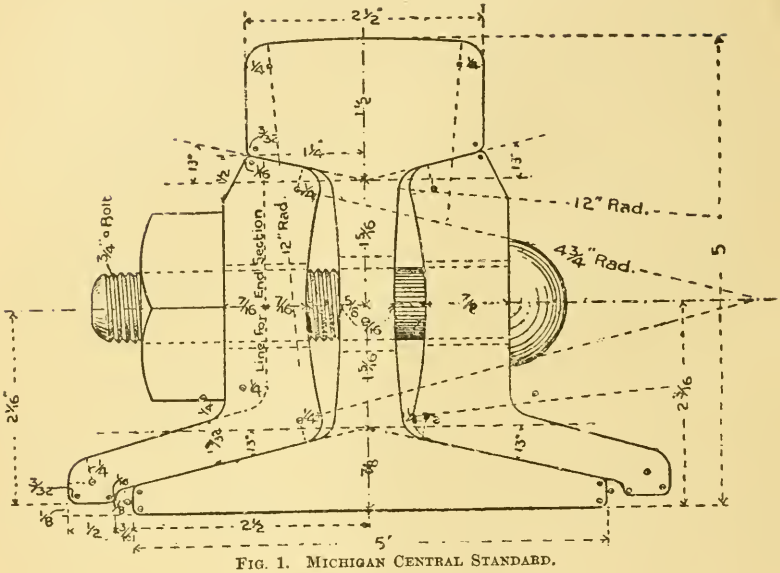


FIG. 1. MICHIGAN CENTRAL STANDARD.

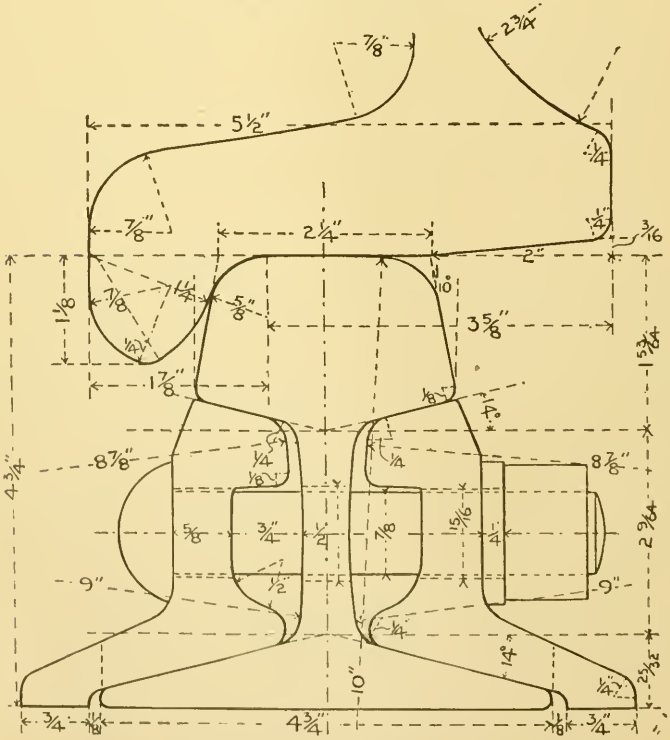


FIG. 2. LEHIGH VALLEY STANDARD.

Do you approve or not the following features of the rail section shown in Fig. 2 (the standard of the Lehigh Valley Raiload)?

A. The ⅝-inch corner radius, identical with a similar fillet radius for the inner corner of the flange?

B. The 10-inch top radius?

C. The decided flare of the sides?

D. The high and large head relatively to base?

Do you approve or not the following features of the rail section shown in Fig. 3 (the standard of the George's Creek and Cumberland Railroad)?

A. The corner radius identical with fillet radius of wheel?

B. The extension of the identical form of rail and wheel to afford bearing contact over the entire side of the rail and inside of flange, instead of over a part only?

C. The general form of the head?

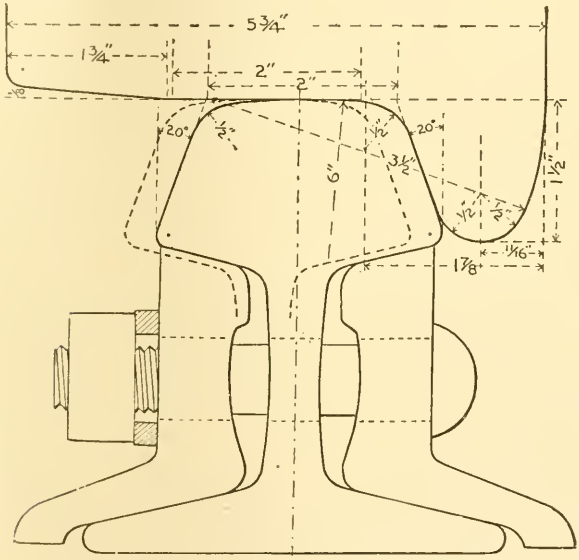


FIG. 3., GEORGE'S CREEK AND CUMBERLAND STANDARD.

Those who have not answered the previous circular of March 18th, are again asked to refer to the preliminary report of the Committee and answer the following questions in regard to it. The Committee cannot properly prepare a final report without a full expression of the views of those who are interested in this important question.

[The parts of the Preliminary Report referred to in the questions below are reprinted in this Appendix in connection with the vote.]

(1) Do you consider that the evidence submitted in the Committee's report, published in the Transactions of the Society for July, 1888, together with your own experience and observations, warrants accepting as final conclusions the preliminary conclusions as to causes of sharp flanges outlined at the foot of page 28? If not, please state your objections.

(2) Do you consider, on the same basis of fact and personal experience, that the preliminary conclusion as to rail-wear stated near the bottom of page 35, "that the rate of rail-wear grows very materially greater as the rail corner is rounded off to fit the fillet of the flange," may properly be accepted as a final conclusion? If not, please state your reasons.

(3) Do you consider that the engravings on page 38-9, with your own observations and experience, warrant the conclusion that worn rails in service take a top radius of about 12 inches, seldom much less or more, and a corner radius of about ¼ inch, or somewhat more? If not, please state your own conclusions.

(4) Do you regard as warranted by the evidence the preliminary conclusions stated in the two paragraphs at the bottom of page 44? If not, why not?

(5) Do you regard as warranted the preliminary conclusions as to fillet radius, on page 45, at the end of the report?

(6) Mr. Don J. Whittemore, Past President Am. Soc. C. E., Chief Engineer of the Chicago, Milwaukee and St. Paul Railway, in a paper,* of which advance copies have been published by the Society, objects to any top curve at all, and proposes an entirely flat top between proper corner curves. Do you deem this likely to be effective in reducing rail-wear, and advise recommending an entirely flat top?

The responses to the questions asked as to the Michigan Central section, Fig. 1, are summarized in the body of this report. To the question as to the Lehigh Valley section, Fig. 2, the responses capable of summary were:

Disapprove unconditionally	22
[Axtell, Barnes, Blackwell, Bonzano, Bouscaren, Brereton, Burpee, Carter, Darlington, Delano, Fletcher, Gibbs, Hazlehurst, Hitchcock, Hoyt, Johnson (T. H.), Landreth, Loree, Merriman, Searles, Snow (J. P.), Stone.]	
Disapprove by implication (answered first circular, in which this question was not specifically asked, in favor of the main features of the Michigan Central section)	20
[Becker, Brown, Briggs, Crosby, Curtis, Defrees, Dudley, Forsyth (William), Hawks, Howe (M. G.), Jones, Katté, Man, McDonald, Morris, Newell, Penfield, Pontzen, Richards, Whitcomb.]	
Disapproves, but like high head [Crowell]	1
" " 10-inch top radius [Forsyth (Robert), Sandberg, Richards]	3
Disapproves, but likes the flare [White]	1
Total disapproving	47
Total approving [Baker, Millholland]	2
Non-commital [Hinckley, Kingman, Robinson (A. A.), Swift]	4

[Messrs. Hinckley, Kingman and Robinson (A. A.) are, by implication, disposed to favor Fig. 1 rather than Fig. 2, but do not so respond categorically.]

* Transactions Am. Soc. C. E. Vol. XXI, p. 133.

To the question as to the George's Creek and Cumberland section, there is but one favorable response, that of Mr. Millholland, and that approves it only for roads of excessive curvature. Five others [Baker, Hinckley, Kingman, A. A. Robinson, Swift] approve the corner radius. All other responses disapprove the section, many of them with emphasis.

Question (1) above referred to the following summary of conclusions (the first one being slightly modified in form to make its intended meaning more clear) as to the causes of flange wear, from page 28 of the Committee's Preliminary Report:

"*First.*—That a tendency to eventually wear to a sharp flange is a natural phenomenon, which would exist and must exist in all perfect and perfectly running wheels, if there were no specific causes for it, and which cannot be eliminated by any form of rail or wheel.

"*Second.*—That imperfections of diameter of wheels or of trucks, or both, give a tendency to run to one rail or the other to nearly all axles, relieving half the wheels of flange-wear tendency, and aggravating that of the other half.

"*Third.*—That the quality of the wheel then has an enormous effect on the number of sharp flanges, causing many more when the fillet-chill is softer than the tread.

"*Fourth.*—That when the wheel is sound and the quality even, about half the wheels get sharp flanges, while this proportion is reduced as the number of special defects increases, which cut short the life of the wheel before it is worn out.

"*Fifth.*—That wheels which fail from flange wear are among the most durable of all wheels, if not the most durable, approximating to, if not exceeding, the life of those which fail from long-continued tread wear only, without developing other defect; so that no appreciable money loss results from wheels running to flange instead of failing from tread wear only."

To the question as to approval of this summary of laws of flange-wear the responses were:

Approving unconditionally	28
[Axtell, Barnes, Boon, Bouscaren, Brereton, Brown, Briggs, Carter, Crosby, Crowell, Delano, Fletcher, Forsyth (Robert), Gibbs, Hawks, Hitchcock, Hoyt, Howe (M. G.), Johnson (T. H.), Landreth, Man, McDonald, Penfield, Searles, Snow (J. P.), Stone, Swift, Whitcomb.]	
Approving, with comments and limitations, chiefly referring to the ambiguous original wording of the first statement, which is corrected above	9
[Baker, Burpee, Dudley, Katté, Merriman, Millholland, Pontzen, Thomson, White.]	
Approving, with limitations as to the fifth [Curtis]	1
Approving all but the first [Forsyth (William)]	1
Approving, with doubts as to third, fourth and fifth [Becker]	1
Approving by implication, but not answering categorically	11
[Blackwell, Buchanan, Darlington, Defrees, Hazlehurst, Jones, Loree, Newell, Otis, Richards, Voorhees.]	——
Total approving unconditionally or in substance	51

Disapproving unconditionally..... 0

"Generally, no" [Bonzano]..... 1

Disapproving for the most part, although approving in part. 3

[Hinckley, Kingman and (A. A.) Robinson.]

Total disapproving..... 4

Question (2) of circular above was as to approval or disapproval of the Committee's preliminary conclusion expressed in the *italicized* words of the following quotation from page 35 of its Preliminary Report:

"If we remember in connection with the records just given: (1) that they included both inside and outside rails, only the latter showing a tendency to flange wear; (2) that the rails on the sharper curves were much younger than on the easier ones, so that the full contrast had not developed, and (3) that the rate of tangent rail-wear of the same rails seemed rather to decrease with age, these figures seem to your Committee to be strong evidence that *the rate of rail-wear grows very materially greater as the rail corner is rounded off to fit the fillet of the flange.*"

The responses showed:

Unqualifiedly accepting the conclusion..... 41

[Axtell, Baker, Barnes, Becker, Blackwell, Bonzano, Bouscaren, Brereton, Brown, Briggs, Buchanan, Burpee, Carter, Crosby, Crowell, Delano, Dudley, Fletcher, Forsyth (Robert), Gibbs, Hawks, Hitchcock, Hoyt, Howe, Johnson (T. H.), Katté, Loree, Man, McDonald, Merriman, Milholland, Otis, Penfield, Pontzen, Richards, Snow (J. P.), Snow (W. W.), Thomson, Voorhees, Whitcomb, Whittemore.]

Accepting conclusion with certain comments or limitations not appearing to affect the substance..... 2

[Searles, White.]

Accepting conclusion by implication from other answers as to form of rail sections, but not answering categorically..... 8

[Darlington, Defrees, Forsyth (William), Hazlehurst, Jones, Katté, Newell, Sloan.]

Total accepting conclusion explicitly or in substance.. 51

No by implication [Hinckley, Kingman, Robinson (A. A.)]... 3

Total not accepting the conclusion..... 3

Doubtful or experimenting [Curtis, Katté, Landreth]..... 3

Question (3) above, as to whether the engravings on pages 38 and 39 of the Preliminary Report (reproduced herewith), with personal observation and experience, warrant the conclusion that "worn rails in service

take a top radius of about 12 inches, seldom much more or less, and a corner radius of about ¼ inch, or somewhat more," is answered as follows:

12-inch top radius	36
[Axtell, Baker, Barnes, Becker, Bonzano, Bouscaren, Brereton, Briggs, Curtis, Delano, Fletcher, Forsyth (Robert), Forsyth (William), Hawks, Hinckley, Hitchcock, Hoyt, Howe, Johnson (T. H.), Jones, Katté, Landreth, Loree, Man, McDonald, Merriman, Millholland, Penfield, Richards, Searles, Snow (J. P.), Stone, Swift, Thomson, Whitcomb, White.]	
12 to 14 inches [Carter, Gibbs, Katté]	3
12 to 16 inches [Dudley]	1
12 inches or less [Brown]	1
12 inches with conditions not affecting substance [Burpee, Merriman, Pontzen]	3
12 inches by implication [Newell]	1
Total agreeing that natural wear is to about 12 inches radius	45
Dissenting, "wear nearly flat" [Darlington]	1
Dissenting—Worn rails in service do not take a top curve unless they have been turned [Crowell]	1
Dissenting without comment [Defrees, Hazlehurst, Whittemore]	3
Total dissenting	5
Not prepared to answer [Blickensderfer, Crosby]	2

As to the second part of the above question, whether wear tends to maintain about a ¼-inch corner radius, the responses stand:

Yes, ¼-inch radius	22
[Axtell, Baker, Barnes, Bonzano, Bouscaren, Brereton, Briggs, Carter, Delano, Fletcher, Forsyth (Robert), Gibbs, Hawks, Hitchcock, Hoyt, Johnson (T. H.), Landreth, Loree, McDonald, Millholland, Richards, Stone.]	
Not over ¼-inch [Katté, Voorhees]	2
Somewhat less than ¼-inch [Brown]	1
Somewhat more than ¼-inch [Howe, Snow (J. P.)]	2
¼-inch + $\frac{1}{32}$ [Forsyth (William), Penfield]	2
¼-inch to $\frac{5}{16}$ [Becker, Man, Thomson]	3
¼-inch to ⅜ [Merriman, Searles]	2
¼-inch with modifying comments [Dudley*]	1
⅜-inch [Hinckley, Kingman, Robinson (A. A.)]	3
⅜-inch to ½ [Curtis]	1
Total answering about ¼-inch in substance	39

* For light rails, $\frac{3}{32}$; for 60 to 75 pound rails, ¼; above that, $\frac{5}{16}$.

"$\frac{5}{8}$-inch" [Swift] 1
"Not proven" [White] 1
Total dissenting 2

About twenty of the responses either omit to answer this question or explicitly state that they have not examined into the matter enough to feel justified in expressing an opinion.

Question (4) above is as to whether the views expressed in the following quotations from page 44 of the Committee's Preliminary Report are warranted by the evidence:

"Is it desirable that the corner radii of rail and wheel in Figs. 9 and 10 should be nearly alike, or differ considerably? (See page 246.)

"So far as rubbing friction only is concerned there will be less wear if they differ considerably; so far as crushing action is concerned, there will be less as the radii approach each other. As a whole, it seems probable to your Committee that the very minimum rate may be when the fillet and rail radii are nearly alike, but not quite; but that they should be therefore made so in the beginning appears more doubtful for the following reasons:

"The evidence seems to be that under all working conditions the rate of rail-wear is very much less before the conditions of Fig. 11 are reached than afterwards. It is therefore desirable to postpone the attainment of such conditions as much as possible. But if the radii be made nearly alike in the beginning, we have but little metal to wear away before the conditions of Fig. 11 are attained. In proportion as the corner is originally sharper we have more. That is to say, if we take the total metal available for wear in the head at 10 pounds per yard or 1 square inch, by using for the corner radius:

$\frac{1}{4}$-inch	in place of	$\frac{5}{8}$-inch	we add	14.08	per cent.	to wearing	section.
"	"	"	"	8.05	"	"	"
"	"	"	"	3.35	"	"	"

"If the preceding indications are correct as to this being very nearly the metal of slowest wear in the rail head, this is a serious loss, much larger than the above percentages alone indicate, and the question of which radius will give the lowest rate of wear in the new rail is less important than which gives the most metal to be worn away before the conditions of Fig. 11 obtain."

The responses stand as follows:

Approving unqualifiedly 31
[Axtell, Barnes, Becker, Bonzano, Bouscaren, Brereton, Brown, Briggs, Carter, Crosby, Crowell, Delano, Dudley, Fletcher, Forsyth (Robert), Forsyth (William), Gibbs, Hawks, Hitchcock, Hoyt, Howe, Johnson (T. H.), Katté, Man, McDonald, Merriman, Penfield, Pontzen, Searles, Stone, Thomson.]

Approving with limitations of minor moment 3
[Curtis, Landreth, Snow (J. P.)]

Approving by implication from other answers 9
[Buchanan, Darlington, Defrees, Hazlehurst, Jones, Newell, Richards, Voorhees, Whitcomb.]

Total approving 43

Disapproving explicitly..... 3

[Baker, Millholland, Swift.]

Disapproving by implication..... 3

[Hinckley, Kingman, Robinson (A. A.)]

Total disapproving..... 6

Question (5) above related to the views suggested in the following quotation from page 46 of the Committee's Preliminary Report, which quotation follows immediately after the one last given:

"The same reasoning, if sound, applies conversely to the fillet radius of flange. The larger this radius, the longer will it take to wear away enough metal to bring about the conditions of Fig. 11. (See page 246.) The most usual fillet radius some years ago was ¾ inch, but ⅝ inch has lately been made standard by the Master Car Builders' Association, for the supposed purpose of approximating more nearly to the form of the rail-head, with some admitted disadvantage in manufacturing, because the smaller the radius, the more difficult it is to get a perfect chill in the fillet. If the preceding deductions are correct, a ¾-inch radius is rather to be preferred for all reasons; but there is no particular necessity from a mechanical point of view for having any single standard radius, either for rail or wheel (although of course it still remains highly desirable, for commercial and manufacturing reasons, to unify practice)."

[To avoid possible misconception, which appears in some of the discussions, the Committee has deemed it wise to add the words in parentheses to this quotation.]

The responses in regard to this question are:

Regard the conclusions as warranted..... 36

[Axtell, Baker, Barnes, Becker, Bonzano, Bouscaren, Brereton, Brown, Briggs, Burpee, Carter, Crosby, Crowell, Curtis, Delano, Dudley, Fletcher, Forsyth (Robert), Forsyth (William), Gibbs, Hawks, Hitchcock, Hoyt, Howe, Johnson (T. H.), Katté, Landreth, Loree, Man, McDonald, Penfield, Pontzen, Searles, Snow (W. W.), Stone, Thomson.]

Approve by implication..... 12

[Buchanan, Darlington, Defrees, Hazlehurst, Newell, Otis, Richards, Thomson, Voorhees, Whitcomb, White, Whittemore.]

Total accepting conclusions, explicitly or impliedly. 48

Dissenting explicitly..... 5

[Hinckley, Kingman, Milholland, Robinson (A. A.), Swift.]

Dissenting by implication [Boon, Snow (J. P.)]..... 2

Total dissenting..... 7

The Committee submits these views, which it has gathered as to fillet radius, for consideration on their merits; but inasmuch as there now exists a standard wheel section which has come into extensive use, it felt that it was inexpedient in this report to recommend any change in the existing standard.

Question (6) above relates to the suggestion of an entirely flat top between the rounded rail corners. One vote on this question (approval or disapproval of a 12-inch top radius) is recorded in the body of this report, but as the vote on the direct question differs somewhat, it is here given, as follows:

In favor of a flat top	7
[Blackwell, Darlington, Defrees, Hazlehurst, Howe, Morris, Whittemore.]	
In favor, with qualifications [Crowell]	1
Inclined to favor a flat top [Axtell, Baker, White]	3
Total favoring a flat top	11
Disapproving a flat top	36
[Barnes, Becker, Bonzano, Bouscaren, Brereton, Brown, Briggs, Buchanan, Carter, Curtis, Delano, Dudley, Forsyth (Robert), Forsyth (William), Gibbs, Hawks, Hinckley, Hitchcock, Hoyt, Johnson (T. H.), Jones, Katté, Kingman, Man, McDonald, Merriman, Millholland, Pontzen, Richards, Robinson (A. A.), Sloan, Snow (J. P.), Stone, Swift, Thomson, Voorhees.]	
Disapproving by implication [Loree, Newell]	2
Disapproving for coned wheels; otherwise doubtful [Whitcomb.]	1
Total opposed to a flat top	39
Doubtful; "as nearly flat as possible, say 18 inches" [Burpee, Fletcher, Johnson (J. B.), Landreth.]	4

The matter contained in this Appendix might with propriety have been included in the Committee's report, and is to be considered as a part thereof, but as it did not relate to their final recommendations, but only to premises on which to base such recommendations, the Committee concluded to confine its report to specific recommendations only. As of probable interest in this connection, the Committee append herewith a drawing of the present Master Car Builders' standard wheel tread. (Fig. 4.)

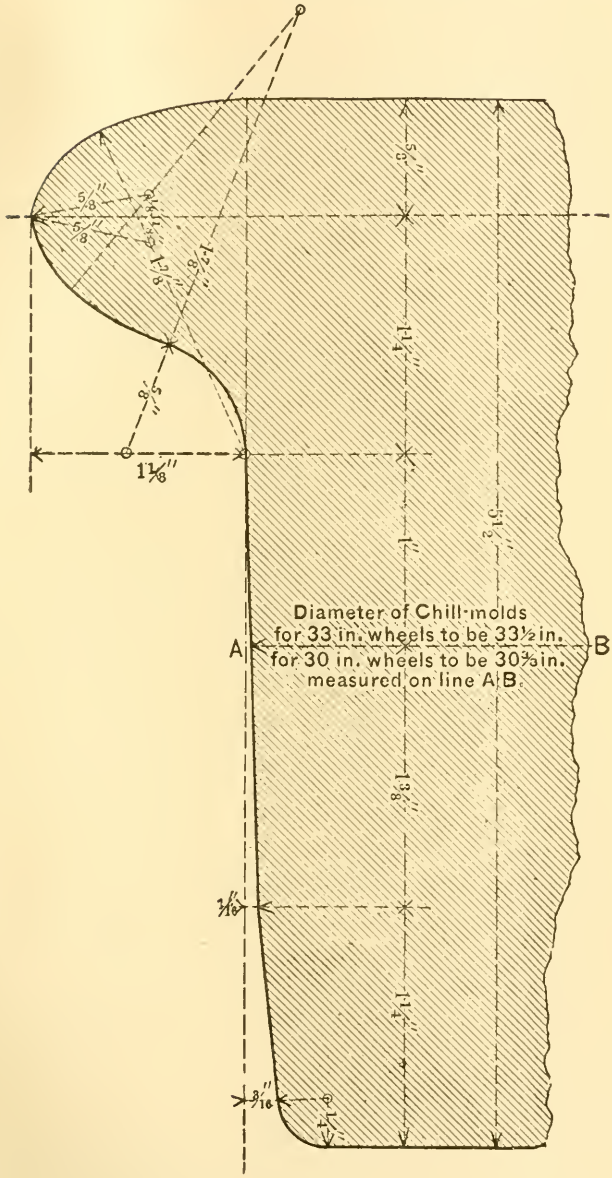


FIG. 4.—MASTER CAR BUILDERS' STANDARD WHEEL TREAD SECTION.

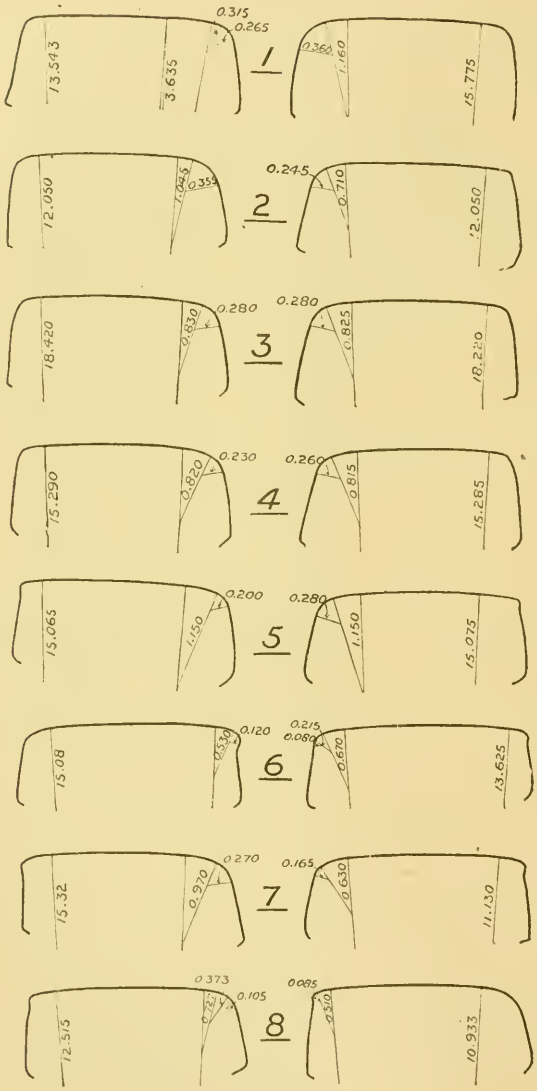


FIG. 7.

RADII OF WORN SURFACE OF RAILS IN SERVICE, PENNSYLVANIA COMPANY. TANGENT RAILS.

[Reproduced from the Committee's Preliminary Report.]

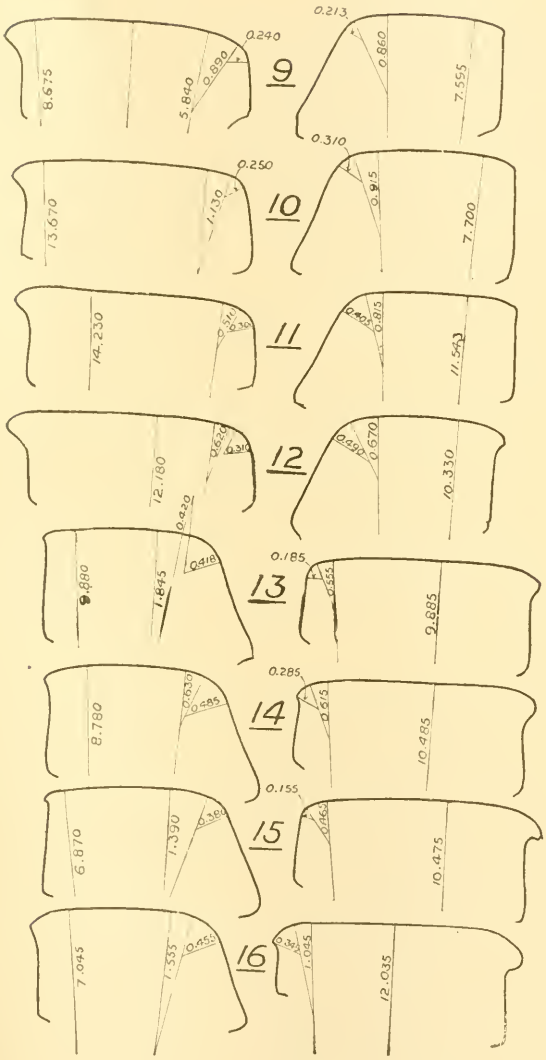


FIG. 7—*Continued.*

RADII OF THE WORN SURFACES OF THE RAILS IN SERVICE, PENNSYLVANIA COMPANY.
INSIDE AND OUTSIDE RAILS OF CURVES.

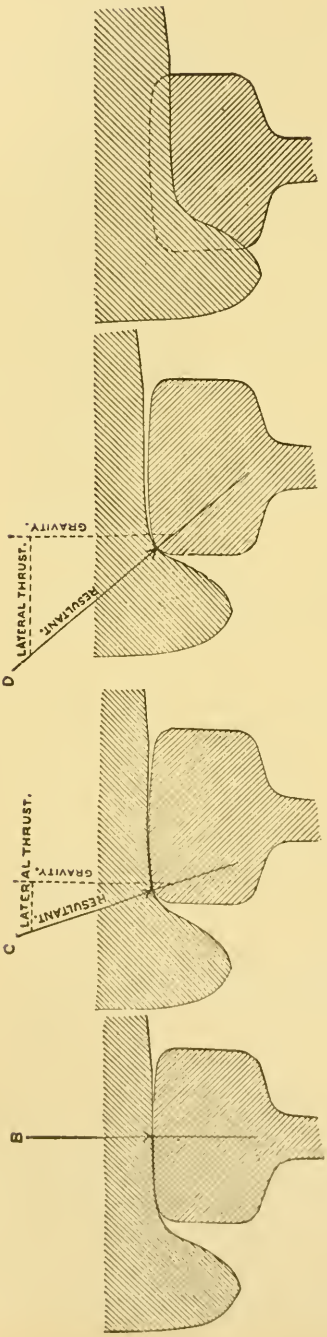


FIG. 8.

Flange acted on by little or no lateral force, but as close to rail as its form permits without such force.

FIG. 9.

Flange acted on by a MODERATE lateral force, as by impacts or oscillation on tangents. (The line C gives the resultant line of pressure, perpendicular to the bearing surfaces in contact.)

FIG. 10.

Flange impelled against the rail by a STRONG lateral thrust, as on outside rail of curves. (The line D gives the line of pressure, still perpendicular to bearing surfaces in contact. The wheel rises as shown until they become so.)

FIG. 11.

Worn outside rail on curves, having the bearing surfaces worn to conform to each other, as in Fig. 2 of Preliminary Report.

[Reproduced from the Committee's Preliminary Report.]

APPENDIX B.

The following extracts are made from letters received in addition to categorical replies to the questions of the circulars issued by the Committee:

M. J. Becker, President Am. Soc. C. E.—If it is admitted that the flanges of wheels (no matter how small their number may be in proportion to the entire lot) are becoming sharpened by use, then it cannot be denied that the primary cause for their being so sharpened is on account of their contact with the rail heads while rolling over the track. Imperfect diameters, oblique trucks, and improper gauging may aggravate the result, but they are not the original causes. I agree with the first conclusion of the Committee, as stated on page 28,* that this result cannot be eliminated by any form of rail or wheel; and I also admit the second one of the Committee's conclusions. But while I am not prepared to controvert, from personal experience or trustworthy information in my possession, the correctness of the third, fourth and fifth conclusions on the same page, I am not ready, from the data appearing in the report, to accept them as final. Generally speaking, I have always been of the opinion that the rail section is affected by the wheel flanges to a much greater extent than the wheel flanges are affected by the rail section; the rail metal being much softer than the chilled wheel metal, the rail would seem naturally to suffer most from the contact.

My experience corroborates the conclusions of the Committee on page 35, that the rate of rail wear increases with the increased contact area, caused by the rounding of the corners of the rail to fit the fillet of the wheel flange. This is certainly true as far as the outer rail on curves is concerned. I have noticed often that upon a portion of our line, composed largely of 9 and 10 degree curves, the inner face of the head of the outer rail was being rapidly planed off as in a lathe, after an impression had once been made, and that the track was thickly strewn with the shavings and small clippings planed off the rail head by the rubbing action of the wheel flanges. I have never noticed a similar effect upon tangents and lighter curves, but upon general principle the conclusions of the Committee would seem rational.

I have never seen a rail worn entirely flat on top. Mr. Rodd's illustrations compare very closely with some samples collected by myself; one of these I inclose (see Fig. 5). The rail was made at the Cleveland Rolling Mill and sold as second quality on account of its supposed hardness; it was laid in the Columbus Depot Yard in 1873, and was literally worn down to nothing when removed about two years ago. The rail was without a flaw throughout, and wore remarkably uniform under a very heavy but irregular traffic in and around the passenger station.

* Preliminary Report, Transactions, Vol. XIX, July, 1888.

A top curve, with a radius of 12 inches, and corners with radii of from $\frac{1}{4}$ to $\frac{5}{16}$ inches would seem to me about the proper shape for a rail head.

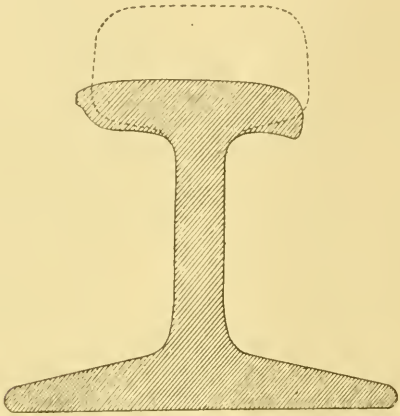


FIG. 5.

Second-class steel rail made by Cleveland Rolling Mill Company and laid in Union Depot tracks at Columbus, O., 1873. Taken up 1887. Dotted line shows original section. This rail carried all of the L. M. R. R. freight traffic and a large amount of switching for fourteen years.

The conclusions of the Committee on page 44 seem to me warranted. The wheel tread should fit the rail top for as great an area as possible; the wheel fillet should be of slightly larger radius than the rail corner, and the sides of the rail head made nearly or quite vertical so as to prevent, for the longest possible time, the close rubbing contact between wheel flange and rail head.

I consider the standard rail section of the Michigan Central Railroad a very good one in every particular, unless it should be found that the shoulder under the rail head is insufficient for the angle bar, in which event I would increase the top width, but leave the sides of head vertical, and, if necessary to maintain the weight, would shave the web a trifle.

The object of Mr. Whittemore's flat head rail sections is to obtain larger bearing surface and thereby relieve the rail from the excessive wheel pressure. This is certainly desirable, and theoretically the flat head would accomplish the object, but I would apprehend that, even with a perfectly flat rail head in general use, the wheel treads would still continue to wear somewhat hollow, and, if so worn, would ride on one or both edges of the flat-headed rail instead of the entire top, and thereby fail to accomplish Mr. Whittemore's purpose; eventually they would, I think, wear the rail top round to conform to their own grooved shape.

W. S. G. Baker, President Baltimore Car Wheel Company.—I regret that I am unable to give an opinion from practical knowledge or experience, except to say that I fully concur with observation First, upon page 28 of Transactions of Society for July, 1888, but believe with properly made wheels, round in form, uniformly chilled, well paired, and properly placed in trucks with load centrally placed so loads will not overhang center line of draft, flange wear will be comparatively small, regardless of form of rail heads.

The only reason why a flange-worn wheel would show a greater per cent. of service is, that they have better wearing qualities upon tread—others being either lightly chilled on tread or defective from special causes—in other words, improperly made.

I believe that chilled wheels can be made that will give even wear and be free (practically) from special defects.

I regret, as a wheel manufacturer, to admit that the chilled wheel in general use is not properly made; and hold the railroads responsible for such state, as price and not quality govern their purchases in large degree.

Oliver W. Barnes, M. Am. Soc. C. E.—I take the opportunity to inclose a section (see Fig. 6) of standard pattern of 60-pound rail which I have just designed and adopted for the Baltimore and Drum Point Railroad, 80 miles long. The peculiar feature of this rail pattern

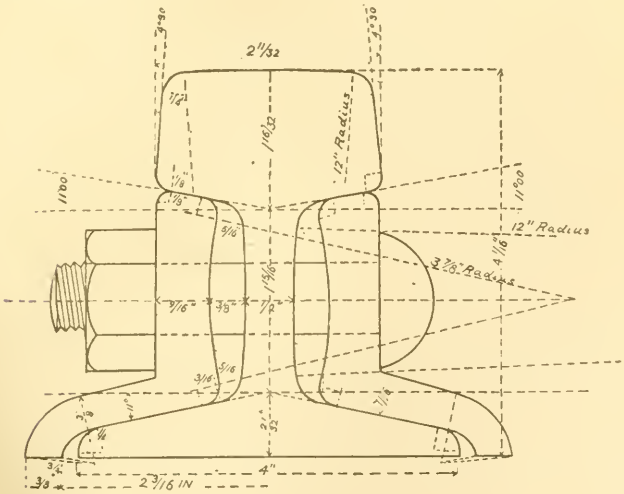


Fig. 6.

is in the angle under the head of 11 degrees, which is the smallest of any rail pattern that has ever been used that I am aware of. If this proves satisfactory, I intend in the future to use a still less angle.

The angle bars used with this rail are somewhat different from those in use on the Pennsylvania Railroad, inasmuch as the Pennsylvania Railroad Company use six holes in each joint, whereas I consider four as amply sufficient.

CHARLES BLACKWELL, M. Am. Soc. C. E.—Sharp flanges are caused by disparity in circumferences of the individual wheels forming a pair. I advise an entirely flat top with maximum of width, and in conjunction, entirely cylindrical wheels, so far as the rail bearing is concerned.

VIRGIL G. BOGUE, M. Am. Soc. C. E.—The accompanying sketch, Fig. 7, of our (Union Pacific R. R.) 75-pound rail section, practically illustrates my view of what the rail section should be.

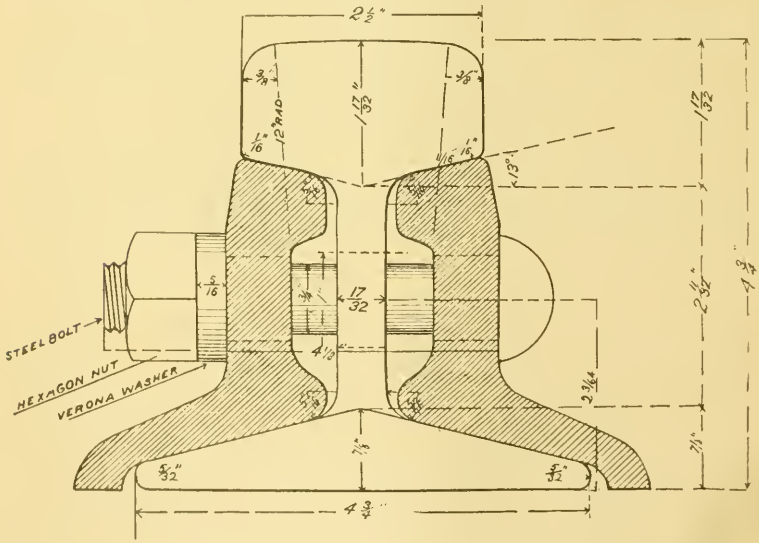


FIG. 7.

	Square inches.	Per cent.
Area of head	3.36	46
" web	1.32	18
" flange	2.67	36
Total area of rail	7.35	100

I prefer the 12-inch top radius because I believe the rail will wear to about that shape, even with cylindrical wheels. I am inclined to think the ¼-inch top corner radius used in the Michigan Central standard is a little too small. I believe it will rapidly wear away the wheel fillets, and therefore prefer ⅜-inches. I prefer vertical sides for head of rail. I prefer small lower corner radius, but at this time I think ⅛-inch about right for it. I believe rails will roll better

when this radius is not quite so small as in the Michigan Central standard, or as shown in Fig. 7. I prefer vertical sides for web of rail, because I believe the rails will roll better and last longer when so made. I have never heard any good reason for making these sides curved, as is usually done. Metal is needed at center of web to sustain heavy blows.

In my opinion, the reduction of metal in the head to 41 per cent. of the whole, as shown by Michigan Central standard, is too much of a good thing. We have used 46 per cent. in the belief that we shall get equally good results in the manufacture of the rails and better results as to wear.

I am inclined to think the general conclusions of the Committee as to wheel and rail wear are about correct, but have not given the matter the attention required for positive opinion.

If we were to revive our 75-pound rail section and joint we should make the lower corner radius of head $\frac{1}{8}$-inch; would make the bolts 1 inch in diameter, and the angle bars at least 42 inches long. They

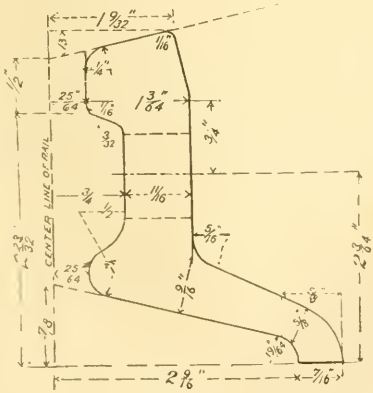


FIG. 8.

NOTE.—One pair of angle bars weighs 70 pounds.

are now 40 inches long. In my judgment there is no gain in leaving a space between the tie and lower flange of angle bar. In practice, the rail soon cuts the tie enough to bring the angle bar to a bearing on it. I prefer to have this bearing exist from the start.

G. BOUSCAREN, M. Am. Soc. C. E., makes the following annotation in addition to his answers to the questions of the circular, as elsewhere recorded:

Decrease width of flange, making thickness on edges not less than $\frac{3}{8}$-inch; allow $\frac{1}{2}$-inch for wear off top of head and make remaining area *B* equal to area of flange *C* (see Fig. 9).

The engravings of worn wheels and my own experience warrant the conclusion that worn rails in service take a top radius of about 12 inches, seldom much less or more, and a corner radius of about ¼-inch, or somewhat more. Old wheels with grooved treads tend to give a

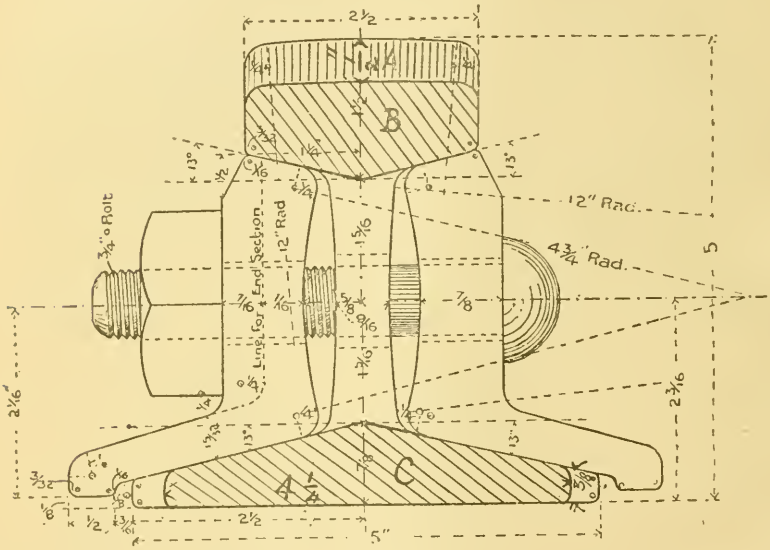


FIG. 9.

surface of smaller radius; the more perfect the wheel the longer the radius. With perfect cylindrical wheels I would be in favor of flat top heads; the point of greatest importance is to secure the largest possible area of contact between tread and head. So long as it is not possible to maintain the majority of wheels in a perfect cylindrical condition, I would not recommend an entirely flat top.

THOMAS J. BRERETON, Jun. Am. Soc. C. E.—According to my experience your committee has reached proper conclusions with regard to relative forms of flange and rail head. I suggest the following objections to the form of rail illustrated (Michigan Central Standard). The vertical sides allow less shoulder for the splice bars than sides which slope outward for the same width of head on top. The splice bars therefore project too far from the rail, and after the already slight depth has been decreased by wear a very little, the flanges of wheels will strike upon the splice bars, and in cold weather this may break pieces from the flange and produce disastrous results.

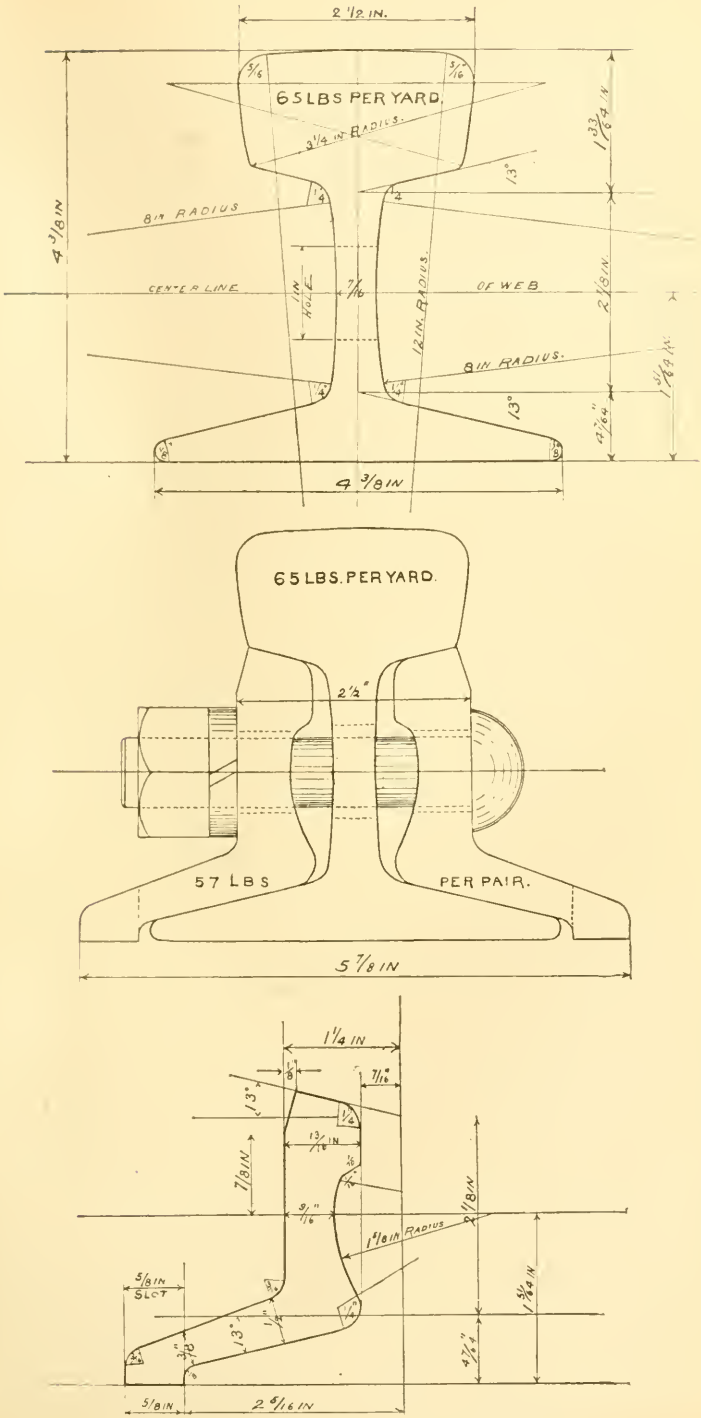


FIG. 10. - 65 pound rail, Denver and Rio Grande Railroad.

I could point out to your committee a great many curves upon our road (Pennsylvania Railroad) where this process has been going on upon the outside rail, and in some instances where a groove has been planed out of the projecting top of the splice bar from ¼ to ⅜-inch deep by this action. This always occurs where we have placed the recent heavy splice bars (our No. 4 pattern) upon rails of the old 60 and 70-pound Pennsylvania Railroad standard section, which possessed greater depth of head and less projection of splice bar than the illustrated pattern of your circular.

I approve of the 12-inch top radius and ¼-inch corner radius, as illustrated.

I should consider the Michigan Central standard better if the head were ¼-inch higher and the sides flared out about 3 or 4 degrees.

The Pennsylvania Railroad has tried and discarded very nearly the same section as the Lehigh Valley. The high and large head is especially objectionable.

The George's Creek and Cumberland section is, I think, very bad.

R. E. Briggs, M. Am. Soc. C. E.—I enclose sections of our 65-pound rail, adopted by the Denver and Rio Grande Railroad, June 26th, 1888. (Fig. 9.)

To secure the broad head and keep the weight of the rail within such limits as would be approved, the sides of the head were inclined toward the base of the rail and the lower corner made sharp to increase the bearing surface on the angle bars. This was done with the idea of putting the metal in the head where the greatest wear comes. We have laid nearly 20,000 tons of this pattern, and thus far the results have been very satisfactory. I do not deem an entirely flat top likely to be effective in reducing rail wear. So far as my experience and observation goes, 12-inch top radius is about right.

W. H. Brown, Chief Engineer, Pennsylvania Railroad Company.—We do not advise recommending an entirely flat top, because the corners of a flat top rail would wear away rapidly, while a curved top would fit the tread better.

Moses Burpee, M. Am. Soc. C. E.—I would not expect sharp flanges to be caused by rails shown in Figs. 2 or 3 of the Committee's preliminary report, but would by Fig. 1, at least until the whole inside surface of flange was worn to the very edge; the other conclusions, on page 28, I think reasonable.

I should think that the top radius of rail caused by wear was determined by the average shape, etc., of the wheels which passed over it, and a different radius might result if a largely predominating proportion of the rails were at first rolled to a different radius, or that the effect of wear of wheel from certain shapes of rails is to wear other rails to a like shape.

I recommend an entirely flat top, if practicable, or as nearly so as possible. With a wheel and rail that are made to present the largest practicable bearing on each other, it seems fair to expect both to last longer than when the bearing is very much less. I would rather have the largest possible crown radius that could be made without loss of a proper degree of solidity of metal in the head of rail; believing also that if a flat top rail could be properly made it would, with flat wheels, give better results than the present practice, but that, should it ever be proven that this is so, it must be done by gradual changes in both rails and wheels, which changes should be in the same direction.

I think that the wear of wheels indicated, as to a radius of 12 inches, a good reason for belief that with a flat top rail in general use, the wear would be flat also on the wheels.

Edward C. Carter, M. Am. Soc. C. E.—The Committee's conclusions are, for the most part, correct. I think that 14 inches would be nearer the proper curve than 12 inches; ¼-inch corner radius is all right.

I do not at all approve the idea of an entirely flat top. A flat rail would very soon be worn to a rounding top surface, with a very considerable loss of metal. This would be the case even if you started with new wheels and rails, respectively cylindrical and flat headed, for the wheels will wear away into a groove and then wear down the edges of the rails to correspond.

Wilson Crosby, M. Am. Soc. C. E.—In addition to his recorded responses to questions of circular, states that he prefers a top flatter than 12 inches for cylindrical wheels, but with coned wheels a 12-inch radius of rail top may do; also that he deems an entirely flat top between the rail corners likely to be effective in reducing rail wear with cylindrical wheels, but with coned wheels, doubtful.

He sees no gain from the curve or objection to making the rail top flat instead of slightly curved, if the flat top of the rail can be as readily rolled, and he advises recommending an entirely flat top, or nearly so, between wheels not coned, but with coned wheels should prefer a 12-inch radius for the top.

J. Foster Crowell, M. Am. Soc. C. E.—The preliminary report of the Committee has been carefully studied from the point of view which it suggests.

As it seems to me, your Committee shows an inclination to adopt a compromise which shall most fairly meet all the conditions in wheels and rails as they now exist; and to make only such recommendations as may be followed without materially affecting the manufacture of them. It is not my intention to question the wisdom of such a tentative course, and it may well be that the hour is not here, nor the man, for more radical changes which, sooner or later, probably must take place. If, in the final

rendering of the Committee's report, the preliminary conclusions are confirmed, the question, in so far as it affects the rail head, will have been decided according to the extreme conditions pertaining to outer rails on curves, as rails are at present made and laid, leaving untouched the grave suggestiveness of that wear as to whether the rails for curves should not be differentiated in form or position, or both; it is of course to be admitted that the customs now prevailing in rail manufacture and supply do not give much encouragement in such a direction, but it would seem that the additional stress on outer rails or about 12 per cent. of all rails, should be provided for before disposing of the matter.

With such reservation as to scope, I send reply to the questions which you put.

I add that I do not consider that worn rails in service take a top curve unless they have been turned; the curve then results from the composite effect of the coning of the majority of the wheels, tending to wear the inner half of the rail head and of flattening along the center of rail by occasional straight treads or treads grooved by wear. I should say that the engravings on pages 38 and 39 (Trans., July, 1888) do not illustrate the primary effect so well as Figs. 32 and 33 on page 6; Fig. 32 showing the primary effect, while Fig. 33 appears to be a rail that has been turned.

The evidence appears to warrant the preliminary conclusion as to fillet radius on page 45. This is a question so largely of manufacture that the wheel makers may be safely left to themselves to evolve the best wearing fillet, provided its form does not react upon the rail corner; the evidence is, however, clear that the larger radius is best for the rail.

In answer to the question in regard to Mr. Whittemore's proposed top, entirely flat between rail corners, I would say that I am greatly in favor of dispensing with the curve, provided the coning of the wheel is abandoned also, as he recommends. Under those circumstances I should recommend an entirely flat top, and see no objection whatever to making it so.

W. G. Curtis, M. Am. Soc. C. E.—The various divisions of the Pacific system lines of the Southern Pacific Company, in the States of Oregon, California and Nevada, and the Territories of Utah, Arizona and New Mexico, aggregating 4 250 miles in length, present a wide diversity of physical characteristics.

Many of the divisions have steep grades and crooked alignment, and some of the others lie on easy grades and comparatively straight lines. The Central Pacific through line crosses the Sierra Nevada Mountains at an elevation of 7 017 feet above the sea; 60 miles of the Southern Pacific on the Colorado Desert, in Southern California, lie below ocean level, the lowest point being — 266 feet. The minimum winter tem-

perature in Nevada is about 60 degrees below zero, and the maximum summer temperature in some parts of Arizona is over 120 degrees above zero, Fahr. The mean annual precipitation (rain and melted snow) in some portions of Central and Northern California is considerably over 50 inches, and in Western Arizona it is less than 3 inches.

The motive power consists of locomotives of many classes, ranging from those with a single pair of drivers and weighing 15 tons, to those of the "El Gobernador" type, with five pairs of drivers and weighing 77 tons.

These, as well as other great differences in conditions, cause many of the operating problems to present themselves, chameleon-like, in various colors according to the surroundings of the observer, and as a necessary consequence from this state of facts, our general and division officers are not so well agreed as we could wish upon some of the facts required by the gentlemen of the Committee. But I may say, as the result of our observation and experience, that we consider, as to sharp flanges, that the conclusions of the Committee are correct, and in this connection I submit the following, which we regard as conclusive evidence on this point:

Statement of steel tired and cast-iron car wheels in six-wheeled trucks, removed from under passenger stock at Oakland, Cal., account condemned for sharp flanges under Master Car Builders' rules, from January 1st, 1884, to November 28th, 1887.

	Number of pairs with		
	both wheels sharp flanged.	one wheel sharp flanged.	Total pairs.
Number of pairs of wheels on outside axles....	14	99	113
" " inside axles.....	15	72	87
" " middle axles....	9	126	135
	38	297	335

The Committee considers "That wheels which fail from flange wear are among the most durable of all wheels, if not the most durable, approximating to, if not exceeding, the life of those which fail from long-continued tread wear only without developing other defect; so that no appreciable money loss results from wheels running to flange instead of failing from tread wear only."

This conclusion appears to be correct for chilled cast-iron wheels, according to our experience, evidenced by the accompanying statements of wear of numbered wheels. In connection with these statements I should explain that, April 30th, 1881, we commenced putting in service numbered, Southern Pacific standard, cast-iron wheels. From that date up to April 30th, 1885, we had put in from time to time, all told, 29 114 new numbered wheels not ground, 4 114 new wheels ground and 5 136 old wheels ground. The old wheels in question had no numbers before

being ground, but were assigned numbers after being ground and fitted for further service. 27 114 of the new ground and unground wheels were manufactured in the Southern Pacific Company's wheel foundry at Sacramento, Cal., and 2 000 of them were made by the Ensign Manufacturing Company, Huntington, W. Va. All of these wheels were Southern Pacific Company standard, having a ¾-inch fillet radius. The make of the 5 136 old wheels (which were withdrawn on account of defects in tread, and afterward fitted for further service by being ground or "turned true" in a wheel grinding machine) is not known.

Unquestionably, however, the tendency to run to sharp flange shortens the life of cast-iron wheels on crooked mountain divisions, because, other things being equal, this tendency for leading wheel to run to outer rail of a curve, acting in the same direction with other forces, causes the flange on such wheel to wear more rapidly than would be the case with a more perfectly running wheel.

Again, this conclusion, for our average conditions, is not true of steel-tired wheels. Our records are not very extensive or complete in this connection, but we believe the following computation, based on the actual service of ten 42-inch, steel-tired paper wheels, fairly represents the average service of such wheels, and I believe we are very near the truth in saying that wheels of this description would make double their present actual mileage on our line were it not necessary to take them out on account of sharp flanges and turn them in the shop after about each 100 000 miles run.

MILEAGE LOST ON ACCOUNT OF SHARP FLANGES ON 42-INCH PAPER WHEELS.

Mileage made by ten 42-inch paper wheels, as per statement of April 2d, 1889	4 525 994
Average mileage per wheel	452 599
Average number of times each wheel turned	4.4
Number of miles made to each turning	102 863
Number of times each wheel can be turned (assuming ⅛ inch only as actual wear on tread at time of turning)	8
Number of miles each tire should make if turned for wear on tread only	822 904
Actual mileage per wheel	452 589
Mileage lost by turning wheel, account sharp flange	370 305

Regarding the proposition "that the rate of rail wear grows very materially greater as the rail corner is rounded off to fit the fillet of the flange:"

Our observers here are not agreed on this point, but the weight of the evidence indicating the experience of this company seems to lead to the opposite conclusion. I consider further experiments are needed

to settle the matter beyond all question or doubt, and to that end we have put into service quite a number of rails shaped down to forms indicated by the accompanying sections, Figs. 1, 2, 3, 4, 5, Plate LXXI, the gauge side of rail varying from a vertical line to one inclined about 77 degrees from the horizontal and with the top corner of curve on the gauge side ranging from $\frac{1}{8}$ to $\frac{5}{8}$ inch radius. These rails are from the same lot and supposed to be quite uniform in quality. To eliminate nearly all doubt as to difference in quality of the metal, so far as it cuts any figure in the experiment, some 30-foot rails have been cut into two and three pieces and the different pieces planed out to different forms; these rails have been laid on outside of curves. The tonnage over them will be recorded and the rate of wear, with all other essential facts, carefully observed. I regret the resulting data will not be available for immediate use by your committee.

As to radius of top curve of worn steel rails, our experience confirms the committee's conclusion, that this worn top curve has a radius of "12 inches, seldom much less or more." The wear of "corner radius," however, according to our observation, averages somewhere between $\frac{3}{8}$ and $\frac{1}{2}$ of an inch, as indicated by what we regard as typical worn sections, sent you herewith (Plate LXXI, Figs. 6, 7, 8).

Referring to questions on page 44 of the report of Committee:

"*First.*—Will there be more or less grinding friction or abrasion under the conditions of Fig. 10 or under those of Fig. 11?"

"*Second.*—Is it desirable that the corner radii in Figs. 9 and 10 should be nearly alike, or differ considerably?" (See page 246.)

It is quite plainly evident that, while the conditions continue to be practically those of mere rolling friction, as in Fig. 10, the abrasion will be at a less rate than for the sliding contact indicated by Fig. 11; but, especially for outer rail of curves on a crooked road, it is somewhat doubtful whether the conditions of rolling contact in question can be maintained for any considerable length of time, and when the element of sliding friction begins to appear the sliding takes place under much greater pressure per unit of surfaces in contact than it afterward becomes under the conditions indicated by Fig. 11. It seems, therefore, as above stated, that further experiments are very much needed in addition to the information written down in the committee's preliminary report, before the conclusions outlined in that report can either be confirmed or disproved.

Regarding the wheel fillet radius, the standard of our road is $\frac{3}{4}$ of an inch, and regardless of all other considerations, we shall doubtless continue to prefer this curve as permitting the production of wheels with a better and more uniform chill than obtainable when using curves of a shorter radius.

Mr. Whittemore's paper referred to, proposing cylindrical wheels and flat top rails, seems to agree with theoretically correct conclusions;

yet it is evident that with wheels and rails of this description, if the rail is inclined inward or outward to the slightest extent, contact between wheel and rail top is reduced enormously, and the moment or leverage of a heavy wheel on one side of a wide rail top has, especially on tangents, a tendency to break off one side of the rail head. In other words, to realize the advantages Mr. Whittemore claims for his design, the flat tops of both rails must lie in the same plane which must be coincident with a "horizontal" plane tangent to the wheel treads in contact with the rail. It is very doubtful whether it is practicable, with ordinary care in maintenance, to provide for this perfect condition and adjustment of wheels and rails.

The conditions of contact, other things being equal, can be accomplished with flat top rails and ordinary coned wheels, coning about 1 in 20 inches, almost as well as by the proposed cylindrical wheels, by inclining the rail inward toward the center line of track (as illustrated by Fig. 9, Plate LXXI), which indicates the practice of this road on mountain divisions and to some extent on valley divisions, new ties being "spotted" by machinery to carry the rail in the position shown.

Fig. 10, Plate LXXI, shows about the average form of all locomotive driving wheel tires in service on our lines. Fig. 11, Plate LXXI, indicates the same thing for a badly worn driving wheel tread. Referring to Fig. 10, it is evident that the rail may be inclined either inward or outward to some considerable extent, and yet with a round topped rail (either section shown in Fig. 9 or Michigan Central section on page 234 *ante*) and driving wheel tires in average condition, there is an opportunity for the wheel to take such a position as will bring a considerable portion or its tread and the rail top in contact, thus giving reason to believe that under average conditions, with comparatively light rails used on the greater portion of the mileage of the country, there is as likely to be as much of the wheel tires and rail tops in contact under the coned wheel round rail top practice as with the cylindrical wheel and flat rail top design proposed by Mr. Whittemore.

I am in confident expectation that my friends and fellow-members of our Society east of the Rocky Mountains, who have so much better opportunities so to do than we of the extreme West, will conduct the exhaustive practical experiments, which certainly seem to be necessary to clear away all doubt from this most interesting branch of the subject.

Plate LXXII shows sections of iron rails in track of the Sacramento Division of the Central Pacific Railroad.

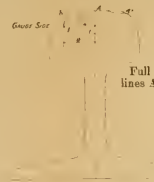
SOUTHERN PACIFIC COMPANY.—SERVICE OF ALL NEW NUMBERED 33-INCH CAST-IRON CAR-WHEELS, NOT GROUND FOR FOUR YEARS, ENDING APRIL 30TH, 1885.

		Total Mileage.	Average Mileage.	
Total number in service	29 114			
Passenger wheels condemned	2 421	144 477 963		
Freight wheels condemned	2 713	56 374 268		
Total wheels condemned	5,134	200 852 231	39 122	
Cost per wheel				$8.50
Cost per mile				.00022
Of wheels condemned, 788 or 15 per cent. sharp		50 535 394	64 131	
Of wheels condemned, 348 or 7 per cent. worn out		23,530 717	67 617	
Of wheels condemned, 3 998 or 78 per cent. other causes		126 786 120	31,712	
Wheels in Passenger Service.				
Of wheels condemned, 696 or 28 per cent. sharp		46 317 460	66 548	
Of wheels condemned, 245 or 10 per cent. worn out,		18 606 815	75 946	
Of wheels condemned, 1 480 or 62 per cent. other causes		79 553 688	53 752	
Total wheels condemned,	2 421	144 477 963	59 677	
Cost per wheel				$8.50
Cost per mile				.00014
Wheels in Freight Service.				
Of wheels condemned, 92 or 3 per cent sharp		4 217 934	45 847	
Of wheels condemned, 103 or 4 per cent. worn out		4 923 902	47 405	
Of wheels condemned, 2 518 or 93 per cent. other causes		47 232 432	18 757	
Total wheels condemned,	2 713	56 374 268	20 779	
Cost per wheel				$8.50
Cost per mile				.00041

SOUTHERN PACIFIC COMPANY.

Experimental Rail Section, formed by Planing down 61¼ Lb. Scranton Steel. Pattern 1882.

No. 1.



No. 2.

No. 3.

NOTE.
Full exterior lines and dotted lines A A' A'' indicate section of Standard Rail.

No. 4.

No. 5.

Outside Rail.
Fig 6

Outside Rail.
Fig 7

Outside Rail.
Fig 8

Cambria Steel Rail, 61 lb per yard, removed from track Sacramento Division, Central Pacific R. R. August, 1883. Laid on Curve 8° 20' L. No. 59, near Tuolumne Station. November, 1874

Fig 9

Rear Driver Central Pacific Engine No. 229 after running 18107 miles. Weight on Wheel, $6\frac{63}{100}$ tons.

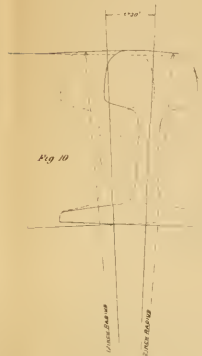


Fig 10

Worn "Blind" (Flangeless) Driving Wheel Tire 10 Wheel Engine. S. P. Co.

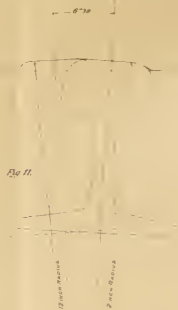
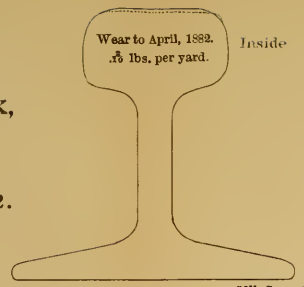
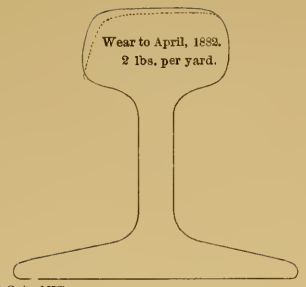


Fig 11

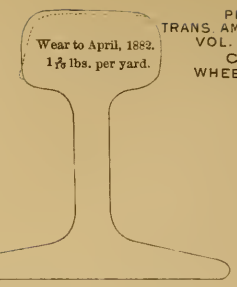
Southern Pacific Company, 56 lb. steel rail inclined at different angles under partly worn engine driver

Half Size Sections of Worn Rails in Track, near Colfax, Measured April, 1882. Sacramento Div. Central Pacific R. R.

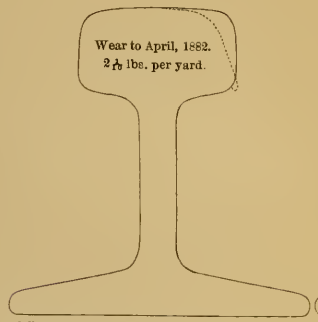




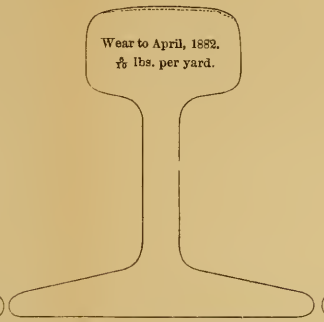
50lb Scranton, laid Oct., 1877.
Inside 6° Curve, Sta. 2892. Outside 6° Curve, Sta, 2892.



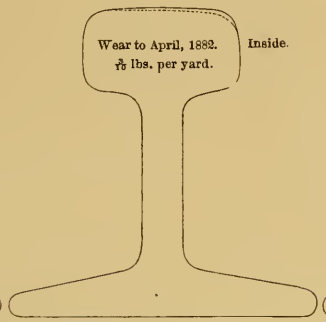
50lb Scranton, laid Oct., 1877.
Inside 3° Curve, Sta. 2881. Outside 3° Curve, Sta. 2881.



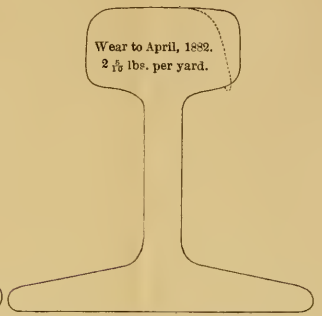
60lb Troy, laid in March, 1876.
Outside 6° Curve, Sta. 2798, Curve, up to Sept. 1881, bad ¼" Elev. pr Deg.



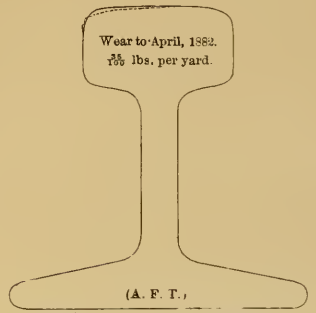
60lb Troy, laid in 1876, inside 6° Curve, Sta. 2798.



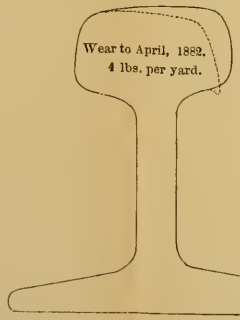
60lb French (A. F. T.) laid in 1872.
On Tangent, Sta. 2967.



60lb French, laid in 1872. Outside 10° Curve Sta. 2930.



60lb French, made in 1871, laid in 1872
Inside 10° Curve, Sta. 2930.



60lb Cambria. laid in 1874.
Outside 7° Curve, Sta. 2994. Inside 7° Curve, Sta. 2974.

50lb Scranton, laid Oct., 1877.
On Tangent, Sta. 2885.

60lb French (A. F. T.) laid in 1872.
Inside 8° Curve, Sta. 2975.

SOUTHERN PACIFIC COMPANY.—SERVICE OF ALL NEW NUMBERED 33-INCH GROUND CAST-IRON CAR WHEELS FOR FOUR YEARS, ENDING APRIL 30TH, 1885.

		Total Mileage.	Average Mileage.	
Total number in service	4 114			
Passenger wheels condemned	1 012	56 503 248		
Freight wheels condemned	41	1 762 760		
Total wheels condemned	1 053	58 266 008	55 333	
Cost per wheel				$8.72
Cost per mile				.00016
Of wheels condemned, 356 or 34 per cent. sharp		20 744 693	58 272	
Of wheels condemned, 144 or 13 per cent. worn out		9 366 190	65 043	
Of wheels condemned, 553 or 53 per cent. other causes		28 155 125	50 913	
WHEELS IN PASSENGER SERVICE.				
Of wheels condemned,	343 or 34 per cent. sharp	19 994 521	58 293	
Of wheels condemned,	142 or 14 per cent worn out	9 335 564	65 743	
Of wheels condemned,	527 or 52 per cent. other causes	27 173 163	51 562	
Total wheels condemned,	1 012	56 503 248	55 833	
Cost per wheel				$8.72
Cost per mile				.00016
WHEELS IN FREIGHT SERVICE.				
Of wheels condemned,	13 or 32 per cent. sharp	750 172	57 706	
Of wheels condemned,	2 or 5 per cent. worn out	30 626	15 313	
Of wheels condemned,	26 or 63 per cent. other causes	981 962	37 767	
Total wheels condemned,	41	1 762 760	42 994	
Cost per wheel				$8.72
Cost per mile				.00020

SOUTHERN PACIFIC COMPANY.—SERVICE OF ALL NEW NUMBERED GROUND AND UNGROUND 33-INCH CAST-IRON CAR WHEELS FOR FOUR YEARS ENDING APRIL 30TH, 1885.

		Total Mileage.	Average Mileage.	
Total number in service	33 228			
Passenger wheels condemned	3 433	200 991 211		
Freight wheels condemned	2 754	58 137 028		
Total wheels condemned	6 187	259 128 239	41 883	
Cost per wheel				$8.54
Cost per mile				.000204
Of wheels condemned, 1 144 or 18 per cent. sharp		71 280 087	62 308	
Of wheels condemned, 492 or 8 per cent. worn out		32 896 907	66 865	
Of wheels condemned, 4 551 or 74 per cent. other causes		154 951 245	34 047	
Wheels in Passenger Service.				
Of wheels condemned, 1 039 or 30 per cent. sharp		66 311 981	63 823	
Of wheels condemned, 387 or 11 per cent. worn out		27 942 379	72 203	
Of wheels condemned, 2 007 or 59 per cent. other causes		106 736 851	53 182	
Total wheels condemned, 3 433		200 991 211	58 547	
Cost per wheel				$8.54
Cost per mile				.000146
Wheels in Freight Service.				
Of wheels condemned, 105 or 4 per cent. sharp		4 968 106	47 315	
Of wheels condemned, 105 or 4 per cent. worn out		4 954 528	47 186	
Of wheels condemned, 2 544 or 92 per cent. other causes		48 214 394	18 952	
Total wheels condemned, 2 754		58 137 028	21 110	
Cost per wheel				$8.54
Cost per mile				.000405

SOUTHERN PACIFIC COMPANY.—SERVICE OF ALL OLD NUMBERED GROUND 33-INCH CAST-IRON CAR WHEELS FOR FOUR YEARS ENDING APRIL 30TH, 1885.

		Total Mileage.	Average Mileage.	
Total number in service	5 136			
Passenger wheels condemned	361	13 870 159		
Freight wheels condemned	1 749	31 388 377		
Total wheels condemned	2 110	45 258 536	21 450	
Cost per wheel				$4.72
Cost per mile				.00022
Of wheels condemned, 188 or 9 per cent. sharp		6 503 538	34 593	
Of wheels condemned, 128 or 6 per cent. worn out		3 832 441	29 941	
Of wheels condemned, 1 794 or 85 per cent. other causes		31 922 557	19 466	
WHEELS IN PASSENGER SERVICE.				
Of wheels condemned, 63 or 17 per cent. sharp		2 777 447	44 086	
Of wheels condemned, 47 or 13 per cent. worn out		1 749 656	37 227	
Of wheels condemned, 251 or 70 per cent. other causes		9 343 056	37 223	
Total wheels condemned, 361		13 870 159	38 421	
Cost per wheel				$4.72
Cost per mile				.00012
WHEELS IN FREIGHT SERVICE.				
Of wheels condemned, 125 or 7 per cent. sharp		3 726 091	29 809	
Of wheels condemned, 81 or 5 per cent. worn out		2 082 785	25 713	
Of wheels condemned, 1 543 or 88 per cent. other causes		25 579 501	16 577	
Total wheels condemned, 1 749		31 388 377	17 946	
Cost per wheel				$4.72
Cost per mile				.00026

SOUTHERN PACIFIC COMPANY.—SERVICE OF ALL NUMBERED 33-INCH CAST-IRON CAR WHEELS FOR FOUR YEARS ENDING APRIL 30TH, 1885.

		Total Mileage.	Average Mileage.	
Total number in service	38 364			
Passenger wheels condemned	3 794	214 851 370		
Freight wheels condemned	4 503	89 525 396		
Total wheels condemned	8 297	304 376 766	36 685	
Cost per wheel				$7.57
Cost per mile				.00020
Of wheels condemned, 1 332 or 16 per cent, sharp		77 783 625	58 396	
Of wheels condemned, 620 or 8 per cent. worn out		36 729 348	59 241	
Of wheels condemned, 6 345 or 76 per cent. other causes		189 863 793	29 954	
WHEELS IN PASSENGER SERVICE.				
Of wheels condemned, 1 102 or 29 per cent. sharp		69 089 428	62 695	
Of wheels condemned, 434 or 11 per cent. worn out		29 692 035	68 415	
Of wheels condemned, 2 258 or 60 per cent. other causes		116 069 968	51 403	
Total wheels condemned,	3 794	214 851 371	56 629	
Cost per wheel				$7.57
Cost per mile				.00013
WHEELS IN FREIGHT SERVICE.				
Of wheels condemned, 230 or 5 per cent, sharp		8 694 197	37 801	
Of wheels condemned, 186 or 4 per cent. worn out		7 037 313	37 835	
Of wheels condemned, 4 087 or 91 per cent. other causes		73 793 895	18 055	
Total wheels condemned,	4 503	89 525 405	19 881	
Cost per wheel				$7.57
Cost per mile				.00038

SOUTHERN PACIFIC COMPANY.—33-INCH CAST-IRON CAR WHEELS. SERVICE OF ALL NUMBERED WHEELS FOR SEVEN YEARS ENDING APRIL 30TH, 1888.

NEW WHEELS, NOT GROUND.

Total number put in service			45 800
Total number condemned			11 834
Of the number condemned,	1 573 were sharp, or	13 per cent.	
Of the number condemned,	1 194 were worn out, or	10 per cent.	
Of the number condemned,	9 067 were on account of other causes, or	77 per cent.	
Total number condemned,	11 834		

NEW WHEELS GROUND.

Total number put in service			12 750
Total number condemned			2 793
Of the number condemned,	637 were sharp, or	23 per cent.	
Of the number condemned,	447 were worn out, or	16 per cent.	
Of the number condemned,	1 709 were account of other causes, or	61 per cent.	
Total number condemned,	2 793		

OLD WHEELS GROUND.

Total number put in service			5 700
Total number condemned			2 901
Of the number condemned,	295 were sharp, or	10 per cent.	
Of the number condemned,	208 were worn out, or	7 per cent.	
Of the number condemned,	2 398 were account of other causes, or	83 per cent.	
Total number condemned,	2 901		

Frederic A. Delano, Chicago, Burlington and Quincy Railroad.—I inclose herewith a brief paper on the subject of rail sections, which gives perhaps as concisely as possible the views I have arrived at on a subject which I have studied to the exclusion of everything else for over a year and a half.

I also send you an engraving of a rail section, Fig. 11, designed by me last October for an 85-pound rail, which combines to the fullest extent I was able the qualities which I deemed essential to a good wearing rail.

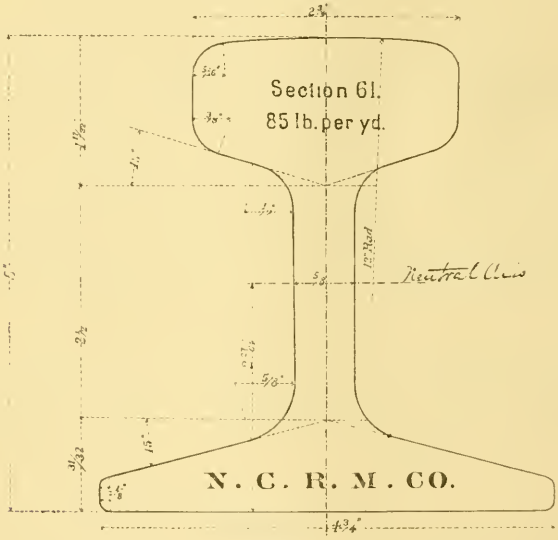


Fig. 11.

In considering the question of rail sections, I have thought that it was not simply a question of mathematics and applied mechanics, but one in which the metallurgy of steel and the usual methods of manufacture play an important part. I am most strongly opposed to the type of section illustrated by your Fig. 2 of the standard for the Lehigh Valley, and of course even more so to the Cumberland Valley section, Fig. 3, and while I like in the main Mr. Hawks' design for the Michigan Central, I am still inclined to think it can be improved on.

I adopted in my section a 12-inch top radius, and five-sixteenths corner radii, as I found that rails on our road usually wore to practically that shape on tangents.

I approve the vertical sides too of Mr. Hawks' design, but I think his corner fillets on the edge of the flange possibly a little too small.

In regard to the breadth of head, I have gone, as you will see, a little further than Mr. Hawks.

I agree entirely with the conclusions of the Committee on the subjects alluded to in the first five questions of the circular of March 18. They are practically exactly the conclusions which I came to after very careful study of the rails on some 500 miles of main line on the Chicago, Burlington and Quincy Railroad. In other words, I think that as the wheel can roll only on one diameter, the corner radius of the rail head and the fillet radius of the wheel should be as dissimilar as possible.

In regard to the proposition of a flat rail top for a cylindrical wheel advised by Mr. Whittemore, I do not find it so easy to come to a decision. Mr. Whittemore's paper suggests a very interesting problem. From the consumer's standpoint, there is only one way in which a flat topped rail can be properly rolled, and that is by making the rail unsymmetrical, as shown in the sketch inclosed, Fig. 12, but I am in doubt whether the difference between a curve of 12-inch radius and a curve

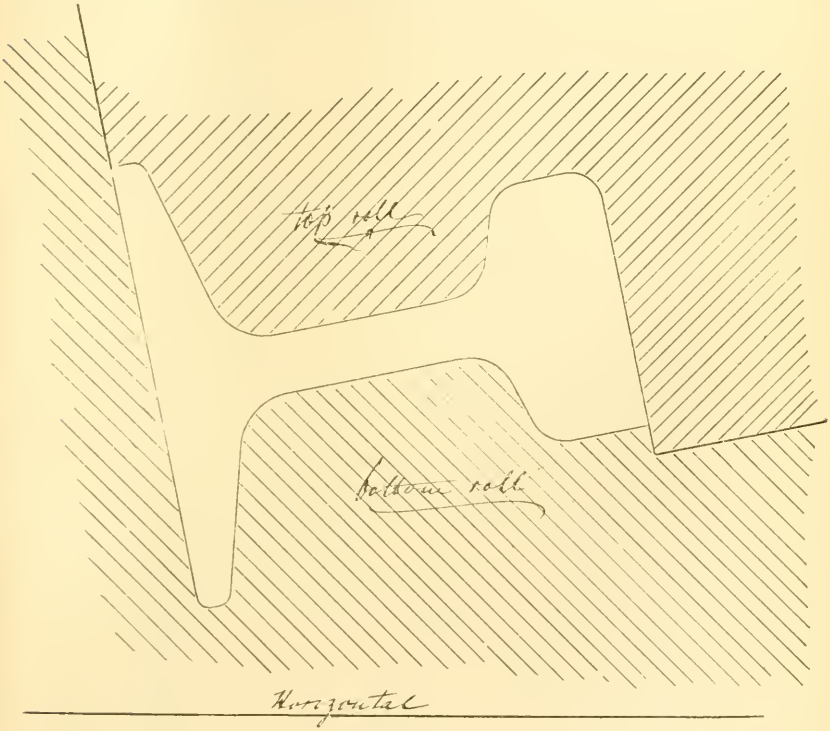


FIG. 12.

which the rail head would assume, even though it started flat, is sufficient to be of very great importance. I do not agree with Mr. Whittemore at all in the idea that the split heads have been produced by the curve

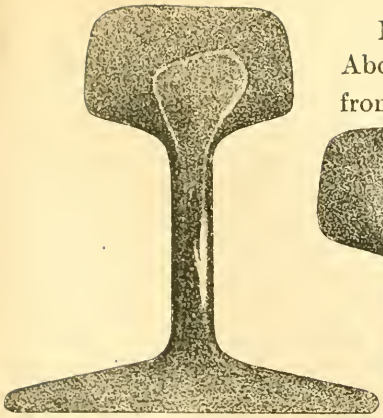
top of the rails, and I inclose etching prints, Plate LXXIII, from several pieces of 56-pound rail, which had never been in track. These pieces, as labeled, came from a single rail of some 150 feet length, rolled from one ingot, and show very well, I think, that the split in the rail head is probably due to an incipient crack which probably exists in some 15 or 20 per cent. of all the rails in track. I also inclose two other etching prints from rail ends near the top and near the bottom respectively of another ingot, which confirms the information of the other prints. (Plate LXXIII, Figs. 6 and 7.) The recent criticism made by Mr. Sandberg that the flat top rail could never be laid in the track absolutely correctly so as to have the tire bear on all parts of the flat surface, is a very just one, and the only theoretical difference I can see between the wear of an absolutely flat top rail and a rail as flat as a 12-inch radius, is, that in one case the flat top rail would be worn on each side of the center by the wheels, which are necessarily slightly hollowed, while in the other case the crown of the rail head would be worn down until it assumed the shape of the average wheel. With rails of so many different widths of head, and with the different track gauges in use and the differences in wheel gauge, there is a considerable play for the wheel on the rail, and so long as this exists the wheel is bound to be hollowed, though in this hollow there may be a flat cylindrical surface of as much as 1½ inches in width. I agree entirely with Mr. Whittemore that the wheels should be cylindrical and not coned in the slightest degree, and I hope that the Master Car Builders' standard will soon be modified in this respect, and at the same time increase the throat fillet to ¾ of an inch radius.

Before closing, I must ask your pardon for treating in too hurried a manner so important a subject. It is one in which I am very much interested, and on which I have spent, first and last, a good deal of time. On account of pressure of other work, I cannot devote to this as much time as I should like, and I am obliged to send you this too hasty letter, which sounds, I confess, a little egotistical, or else remain silent.

The following are extracts from the paper above referred to.*

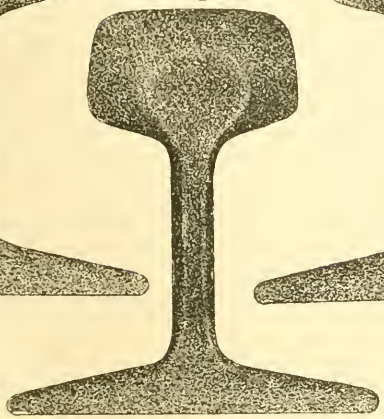
"The designing of many rail-sections by engineers who are far from being experts in the metallurgy of steel has led to the general acceptance of erroneous principles as the rules for such designs. Until very recently it has been assumed that the more metal there was put in the head, to wear away, the longer the rail would wear; and it has been the practice, in adopting heavier sections, to put all the additional weight on top of the rail-head of a very much lighter section. Careful investigation on a number of railroads has shown that of the rails weighing 65 pounds per yard and over, the greater part have to be removed after comparatively trifling abrasion, solely because they are no longer fit for a smooth track. This is evidence of error in the principles of the design of the section.

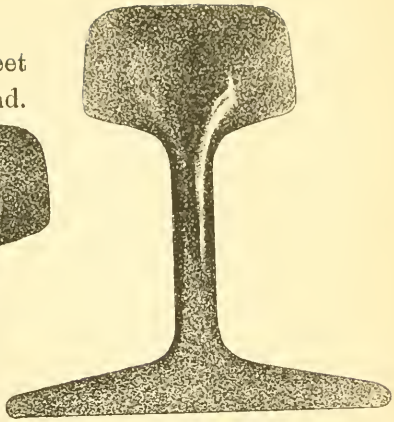
* Rail Sections, by Frederic A. Delano, Trans. Am. Inst. Mining Engineers, Vol. XVII.



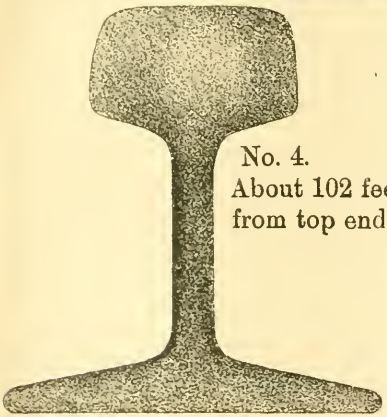
No. 1.
About 5 feet
from top end.

No. 3.
About 70 feet
from top end.



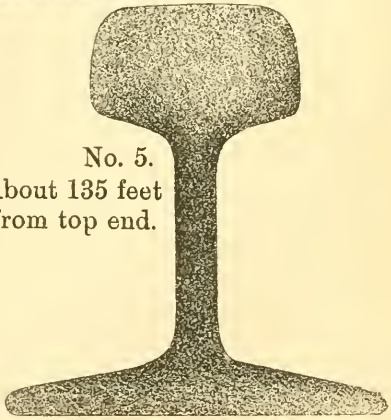


No. 2.
About 37 feet
from top end.



No. 4.
About 102 feet
from top end.

No. 5.
About 135 feet
from top end.



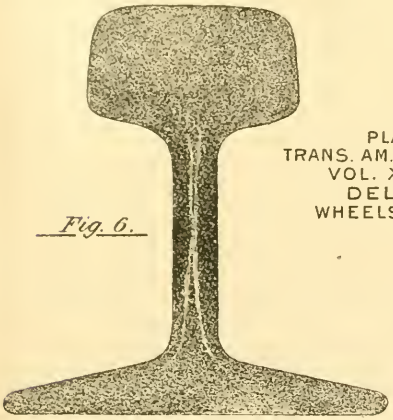


Fig. 6.

PLATE LXXIII.
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XXI. Nº 425.
DELANO ON
WHEELS AND RAILS.

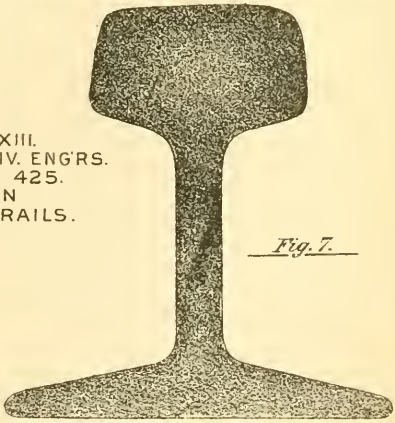


Fig. 7.

From near the top

From near the bottom

of the same ingot.

"It is generally admitted that, as a rule, rail-sections designed since 1879, for rails of 65 pounds and over, have not given entire satisfaction. There are several reasons which may account for this: first, a constant increase, during this period, of wheel-loads on engines and cars, together with generally higher speeds; secondly, new methods in manufacture, cheapening the cost of production and possibly producing an inferior article; thirdly, mechanical conditions in the rail itself, due to the shape of the rail-section.

"1. The first cause is undoubtedly a real one. I think there is evidence that the elastic limit of much of the rail-steel now in use is often actually exceeded by the strains of practice, causing a flow of metal, which is then abraded by the wheel flanges. Granting the existence of this cause, there are two ways of ameliorating the result; one is to increase the bearing-surface of wheels on rails; the other, to raise the elastic limit of the metal.

"2. With regard to the second cause, it is held by many that the enormous production, low cost, and 'improvements' induced by competition have involved inferior quality of product. Many well-informed men are content to rest on the simple assertion that the old John Brown steel gave remarkable satisfaction, because it was made of good iron and did not contain more than .08 per cent. of phosphorus, any analysis showing more being grossly inaccurate. Low phosphorus steel can be made to-day with more certainty and at less cost than ever before. The only question is, whether the elimination of phosphorus is going to be worth what it costs. The fact that as many Bessemer charges are now blown in one hour as were formerly blown in a day, is pointed at as evidence that the same care cannot be given to the operation; but I think men really conversant with the turning-out of a large product will maintain that almost absolute uniformity in the iron is necessary to keep things running with the clock-work regularity required for such a rate of production. Indeed, large product and general smoothness in mill-operation are, to my mind, an indication of a uniform product, so far as metal in the rail itself is concerned, and seem to me, therefore, not objectionable on that score. With the rapid rolling, however, particularly the rapid blooming or breaking-down of large ingots, I find fault, as I have explained at some length in my former paper, already cited.

"3. That mechanical conditions in the rail itself, due to the general design of the rail section, can have any important effect on the service-value of rails has not been generally admitted, so I wish to lay particular stress upon this cause and attract to it the recognition it deserves. Speaking in general terms, that form of rail is, in my opinion, faulty, which has a deep massive head, a wide, thin base and a thin web, connected to the head and the base by small fillets only. The objection to this form and the advantages which might be obtained by different proportions may be concisely enumerated as follows:

"*a.* Such a form necessitates rolling at a higher temperature, and, what is of chief importance, finishing at a higher temperature than permits the securing of a well-forged, compact rail.

"*b.* This type of section probably encourages the use of a higher percentage of manganese to reduce the number of second-quality rails; and this production of second quality rails, as well as the additional manganese, figure in the cost of the rails to the consumer.

"*c.* Rails with heads out of proportion to the rest of the section require a great deal more cold-straightening, because it is impossible to

know how much bending or cambering such a rail needs in its hot state in order that it shall cool straight. This is certainly a very important consideration.

"*d.* The same sort of disproportion must produce corresponding disproportions in the rate of cooling, causing internal strains which are only partly dispersed or effaced by the small connecting fillets.

"*e.* A section having these internal strains exaggerates the effect of any impurity which may chance to have segregated in the ingot, while a section not liable to these strains would safely admit of a generally higher carbon steel as well as a considerable latitude in composition.

"*f.* It is not economical to provide for an abrasion of some ⅝ of an inch, or even more, from the top of the rail, if it is found that rails generally fail long before this amount has been abraded. Instead of providing more height to wear away we can do better by providing more breadth of bearing-surface, and metal of a higher resistance within the elastic limit.

"*g.* Inasmuch as the funnel-shaped cavity, dispersed blowholes and possible impurities exist in the ingot in the axis of its length, it follows that these defects will exist (especially in rails made from the upper part of the ingot) on an axial line which passes through the rail-section. It follows that if the rail-section has 50 to 55 per cent. of its metal in the head, the poorest metal is inclosed in the head, where it receives the least amount of forging; whereas, if the amount of metal in the head does not exceed 40 to 45 per cent., this poor metal occurs in the thoroughly-worked web portions.

"It seems to me that rail-sections should be made to certain definite proportions, regardless of the weight per yard. It is often said that the larger sections will admit the use of a higher carbon-steel; but this assertion requires the proviso that the heavier section shall be similarly proportioned. One 74-pound section which I have in mind could not be safely made of as high a carbon steel as the 62-pound section from which it has been schemed.

"While I am deeply interested in the recently published researches of Mr. D. J. Whittemore, Chief Engineer of the Chicago, Milwaukee and St. Paul Railroad, and agree entirely with him in his conclusions that wheels should be cylindrical and the bearing surface should be increased, it seems to me that a rail head of 12 inches radius is flat enough. Since there is a lateral play of fully ½ an inch for the average car-wheel on an average track (not to mention a 4 foot 9 inch gauge) both the wheel treads and the rail heads must wear to a curve. On the other hand, if the rail head begins with a slight top-curve, it would take no longer than with the flat topped rail to assume the curve of the average wheel.

"The problem is to increase the life of rails without greatly enhancing the cost. It might be possible to make a 60-pound rail of steel worth $75 per ton give as good service as an 85-pound rail made of $30 steel; but the discovery would not prove remunerative.

"With regard to internal fillets and flat fishing angles, I have held the extreme view, that the fillets should be liberal and the angles of enough slope (certainly not less than 15 degrees), to allow really colder rolling than is anywhere the practice in this country."

P. H. Dudley, Engineer of Tests, New York Central and Hudson River Railroad.—The broad assumption in the conclusion of the Committee as to causes of sharp flanges is not warranted from the evidence submitted.

Switching engines of the four wheel type without trucks to guide them and having the usual flange play usually wear the flanges of all the drivers. Reduce the amount of flange play to ¼ of an inch and the flange wear of the drivers is nearly checked. This was shown by lessening the flange play of the drivers of the switching engines in the Grand Central Yard previous to the year 1876, and it has since been continued. The flange play on the engines of the New York Central has also been lessened, the flange wear being decreased.

The majority of drivers of passenger locomotives, with proper leading trucks, do not wear the flanges of the driving wheels sharp. Freight car trucks with short wheel bases and large flange play, have a much larger percentage of sharp flanges than passenger coaches with longer wheel bases.

The flange play of ⅜ of an inch and about ¼ of an inch of journal play, is too much for a freight car truck, as it permits the outside leading wheel flange of the truck to impinge on the rail forward of a perpendicular from the center of the wheel, and abrasion of wheel and rail takes place; the ratio increasing as the gauge widens by wear of wheel and rail.

The axles, instead of running parallel to the radius of the curve, point forward of its center.

If the wheel runs normal to the curve, the abrasion is slight and the lines can only be traced on the side of the head of the rail; if the short wheel base and flange play permit a large angle, the abrasion is faster, the lines become more distinct.

On an outside rail on a sharp curve on a heavy down grade, the abrasion is very severe and rapid, over $\frac{5}{16}$ of an inch being worn away from the side of the head of rail in nine months' service, the tonnage passing over the rail being 4 350 000 tons.

The experience with perfect wheels, if any exist, is very limited. Under my own car, wheels with perfectly cylindrical treads of exactly the same diameter upon the same axle, in a four wheel truck, 7-foot wheel base, have run over 100 000 miles without showing any tendency to sharp flanges.

In the six wheel truck, 11-foot wheel base, the two outside pairs of wheels have only ¼ of an inch play between the flanges and rails; these have run 70 000 miles, the flanges are worn but are not sharp. The limited amount of play in this long truck was not to prevent flange wear but to check the oscillations of the truck.

Flange wear causes train resistance, consequently increased coal consumption.

Flat top rails were the first used by many American roads: Boston and Lowell Railroad, Western Massachusetts Railroad, Reading Railroad and Cleveland and Erie Railroad, and the inverted U rail of the Baltimore and Ohio Railroad; something similar to the type of

rail with two legs as suggested by Mr. Whittemore. Of most of those sections I have seen rails which have been in service, and all were worn rounding on the head. It was then deemed advisable by the civil engineers and rail makers to make subsequent rails with the curve on top when new. The radius has been made much too short, 6 inches being very common, until within a short time. Experience has shown that it is advisable to lengthen the radius and at the same time broaden the head of the rail to give greater bearing surfaces to both wheel and rail. With the worn wheels which must pass over these rails, experience shows on the broad head that it will be some time before the curve will be worn much longer than 12 inches radius; after more of these broad headed rails are in the tracks, the wheels will be worn to a larger curve, and then the rail can be made with a longer radius for the curve on the head of the rail.

William Forsyth, Mechanical Engineer, Chicago, Burlington and Quincy Railroad.—I agree to the preliminary conclusions relating to wheel wear, page 28; items 2, 3 and 4, but not to 1. No. 5 applies to chilled wheels, not steel tires. The two kinds of wheels should be considered separately. Soft tires will wear sharp flanges and have a short life.

In regard to rail wear, I inclose a letter from Mr. E. W. Penfield, who has charge of our rail records, and I give you his opinion on this subject in preference to my own.

Chicago, May 1st, 1889.

William Forsyth, Esq., *Mechanical Engineer:*

Dear Sir,—Referring to the circular sent out by the American Society of Civil Engineers, March 18th, which you forwarded to me some time since, I would say that in the main the opinions set forth in the Committee's preliminary report are confirmed by my experience.

I inclose two different rail sections, Figs. 13 and 14, taken from track after over seven and one-half years' wear and 30 000 000 tons service, in which you will notice that $\frac{5}{16}$ inches upper corner radius is a trifle nearer the line than $\frac{1}{4}$ inch radius.

The vertical sides of the head, I have, as you know, advocated for a long time. I do not like the small lower corner radius, except when a splice bar is to be used, which latter I do not think is the best form of joint. We should not deform the rail to accommodate the joint.

The depth of the Michigan Central rail head is 60 per cent. of the width. If the width was increased so that the percentage should be 55, the head would be improved.

The $\frac{1}{4}$ inch fillets from the web to the base and head are radically wrong.

In order to avoid as much as possible internal strains in the metal in the different parts, these fillets should be as large as practicable, say of ½-inch radius when a splice bar is to be used, or ⅝-inch radius where

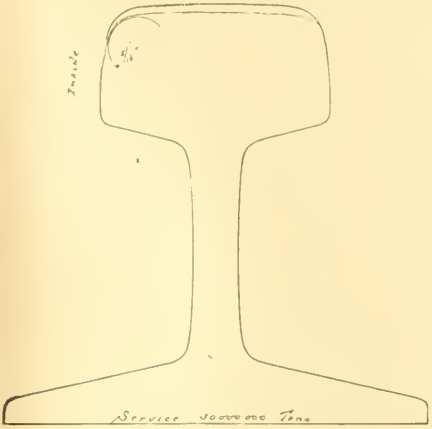


FIG. 13.

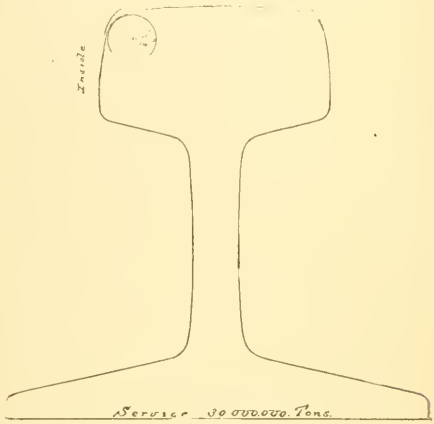
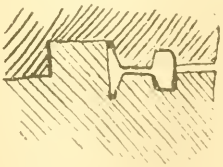


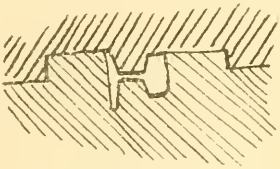
FIG. 14.

some form of under joint is adopted. In which latter case also the upper fishing angle might be materially increased with advantage.

I do not agree with Mr. Whittemore concerning the flat top for the rail, for the following reasons: The tread of the wheel always wears in a curve, on account of the truck shifting from one side to the other of the track. The longer the service the more concave this curve. As soon as the tread commences to get hollow, the bearing on a flat topped rail would be necessarily on one or the other (or both) of the upper corners and not at all in the middle, and the rail soon has a slight top curvature. With our present system of rolling, the rolls are divided in the middle of the head, as per sketch, and it would be difficult to have the metal fill out the section were it not for this slight curve on the top.



The only means of avoiding this is by dividing the rolls at the top the same as at the bottom, thus: which would make a one-sided rail, one of the upper corners being square. This would cause extra expense in laying. Also, I think a 12-inch radius is quite as good as a flat top.



Now that the subject of upper corner radius has been acted upon, I hope that before long the fillet radius of the wheel flange will be changed back to ¾ inch, and thus get all the possible benefit from what some have long thought to be the better lines for adoption.

Yours truly, EDWARD W. PENFIELD.

Mr. George Gibbs, Mechanical Engineer (Chicago, Milwaukee and St. Paul Railroad), states, in addition to his recorded responses to questions of circular, that he considers that the number of sharp flanges is considerably influenced by the contour of wheel tread and also rail head.

He considers cylindrical wheels undesirable on account of tendency to flange wear, and absolute flat tops to rails as aggravating this tendency and also defeating the object sought, of increasing contact area from causes of imperfect surfacing of track. Say, 12-inch radius to rail top and same to main portion of wheel tread he believes will provide the desired contact under practical working conditions; attention being given to radii of fillet to prevent flange wear as noted.

Milton G. Howe, M. Am. Soc. C. E.—My opinion is that the life of both wheels and rails would be prolonged if the rails were made flat on top and the tread of wheels cylindrical. The load and wear would be distributed over the entire width of rail head instead of a part of it. This condition, under the present system, is only reached when the head of the wheel is worn hollow to approximately fit the convex top of rail. In the meantime there is evidently unnecessary wear, which is objectionable.

J. D. Hawks, M. Am. Soc. C. E.—The Michigan Central section for 80-pound rail embodies my idea as to correct rail section. Since that section was published I have seen a number of sections for different railroads practically like it, and inclose you blue print of a couple received to-day from Colonel W. R. Jones, of Carnegie Bros.*

It will be seen from the sections that the small lower corner radius is not objected to by the mill people, as Colonel Jones uses a radius of $\frac{1}{16}$ of an inch, instead of three thirty-seconds, as in the Michigan Central rail. I inclose plan (Fig. 15) showing section of old Canada Southern 60-pound rail, and also, in dotted lines, section of a rail I found in the scrap pile the other day. Of course it cannot be expected that we shall be able to get any such wear from all our rails. This particular rail was sound from end to end, and no more worn at the joint than elsewhere. The broad head idea, however, may be carried too far in this climate.

Fig. 16 shows sections of an old Michigan Central iron rail that made trouble in winter by reason of wet snow packing on the head of rail and lifting locomotive truck wheels off the track.

I see no advantage in making the rail top flat. The wheels, having play, would soon round it to about 12-inch radius. The track of a wheel is an irregular line along the rail top, unless some portion of the top is noticeably higher than the rest. This may happen by rail coming from rolls a little full on top (which is not objectionable if not too

* These are included in the illustrations of the discussion by Mr. W. R. Jones, immediately following this discussion. (Plate LXXIV.)—Ed.

much so), or by rail being laid brand side in when brand side should be laid out, or by changing rail from one side of the curve to the other. In such cases it takes some little time to wear rail, so top will wear bright the full width, but nothing like splitting of the rail follows ; in

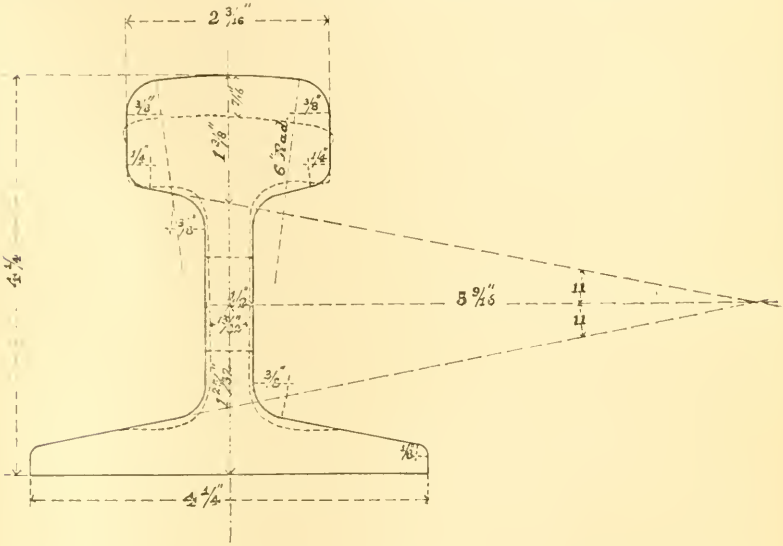


FIG. 15.

fact I have never seen a sound steel rail split in a main track proper. We can split rails for a trifling distance from the end in stub switches when space at end of rail is great, or on our ferry boats where there is something of a jump from the table to the boat, but only then after long hammering.

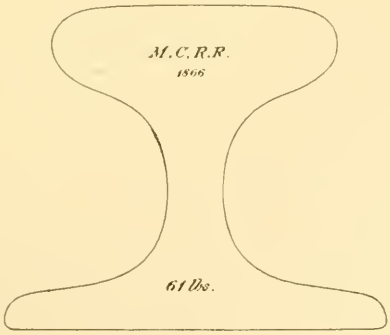


FIG. 16.

I was on the Lake Shore and Michigan Southern, when that road laid its first steel rail, and have paid particular attention to the rail

question from that day to this, and with the same conditions before me, as mentioned by Mr. Whittemore, I draw an entirely different conclusion. A faulty ingot may produce a rail that will not show a defect for years. but the defect is there, and when the rail is sufficiently worn or unusually strained the defect will show.

I probably have had as much trouble with soft rails as any one, but can say that with the softest of the "pewter rails," the ends did not split even when mashed down a ½-inch. It was a very serious matter when rails were giving out inside of a year with reasonable usage by reason of being too soft, but our trouble from that source is happily ended. We have been using the long six hole angle splice for five years, and during that time have not taken out a rail because of battered end when the long splice was used.

Breaking at the bolt holes is also a thing of the past with joints laid with the long splice, and split rails are now few and far between as compared to ten years ago. I believe there is no more theory about track work than about medicine, and so do not claim the above facts prove that the long splice is entitled to the whole credit for the different behavior of rails during the past five years. I think the splice has something to do with it, but believe the rail men are making better rails.

Some years ago I was led astray by the theory that a wheel should fit the rail, and so fitting would lessen the number of sharp flanges. I first discovered there must be something wrong with the theory when I found sharp flanges only on one of the wheels on an axle, never on both. I then had holes cut in the floor of my car over the wheels and window glass fitted to them to keep out dust, and spent many hours watching the action of wheels on the rails, and my conclusion is that the ¼-inch radius for top corner of rails will not hurt the wheels and will give us a considerable addition to the "precious metal," on the rail top, also that the rail wears much faster after it fits the wheel than before.

William R. Jones, General Superintendent Edgar Thomson Steel Works.—I pass over the first six interrogations of the circular and the one marked "A," and consider the questions as being strictly in the province of railroad officials.

From the manufacturers' standpoint, I would urge that the 12-inch top radius be adopted. It is a difficult matter to roll flat heads and get smooth surface, and also a difficult matter to maintain a reasonable height in section of rail. The standard practice in mills is when new rolls are prepared to have height of rail $\frac{1}{64}$ of an inch less than templet, and when rolls are dressed in order to insure smooth heads the height of section is gradually increased, and the limits in height of section from a set of rolls is represented, at first by section being $\frac{1}{64}$ of an inch low, and the ultimate height $\frac{1}{32}$ of an inch higher than templet. With the 12-inch radius as rolls are dressed, and as each dressing will represent a reduction of ¼ of an inch in diameter, that part of the roll forming top of head

must be turned smooth, and the 12-inch radius in the top of head helps to compensate for metal turned off rolls, in dressing. I append drawing, Plate LXXIV, showing the section of rail in the pass before the finishing pass, which is technically termed the leading pass, and also of the finishing pass.

A is the section of rail in leading pass.
B section of rail in finishing pass.
C is cold or finished section.

I give the figures in width of flanges and height of rail, to show the allowance we make in finishing pass for shrinkage. You will notice that the head of rail in *A* has a decided bevel. This is done so that as rolls are reduced in diameter we can smooth that part of the roll that forms the head which is so essential to get smooth rails. As a maker of rails I would strongly urge the adoption of the 12-inch radius.

The question of vertical sides is a debatable one. I would say to those roads where tangents are the rule and curves the exception and of great radii that straight sides are admissible, but on roads where curves are the rule, and tangents almost unknown, then sides of heads would better be made with at least a 4-degree angle. Drawings *D* and *E*, Plate LXXIV, of rails in Pennsylvania Railroad track, on the Brinton curve, show the form of head assumed by wear of rail.

The small lower corner radius is essential, in order to get all the bearing possible for fish-bars. The addition of metal by reason of small radius in corner is an infinitesimal percentage in weight of rail, but is a large percentage in bearing surface for fish-bars. As a rule, the fish-bar's bearing on the under side of head has been totally inadequate, and fish-bars have been of the most trifling character, considering the important relation they bear in maintaining a good roadway.

The broad, shallow head I consider a decided step in the right direction. The mistake of the past has been in massing metal in vertical section of head. The testimony is that the original 60-pound section, Pennsylvania Railroad standard, has worn far better than the 67-pound section of the same road. I receive the same testimony from other railway officials who have used the two sections. I also append drawings *F* and *G*, Plate LXXIV, showing the difference in metal in head of the two sections.

It is a fixed rule in mechanics that, where we find any part of machinery that wears too fast, we endeavor to remedy the evil by increasing the wearing surface.

One of the greatest evils we have to contend with in rolling rails is the unequal distribution of metal, requiring the rails to be curved when hot to allow for shrinkage, owing to the mass of metal in the head and the sometimes wonderfully thin flanges and webs.

I would remark that, when wonderful records of rails are poured into the manufacturer's ears, the sections of such rails do not show deep

heads, thin webs and thin flanges, but on the contrary they represent rails where the metal was well and equally distributed. In connection with this subject, I was informed by Mr. Gunn, Engineer of the Kentucky Union Railway Company, that rails that this Company had furnished to the Cincinnati Southern Railway Company had been removed from the track not because the head had worn out, but because the flanges had worn out, and became so thin that the rails were too weak for safety. Here is, to me at least, a new development.

I append drawing *H*, Plate LXXIV, showing a section of rail taken out of track of Baltimore and Potomac Railroad for the same reason, which shows the wear on flanges. I have prepared for Mr. Richards, Assistant Chief Engineer, Pennsylvania Railroad, two designs of 85-pound sections and two designs of 90-pound sections,* Figs. *J*, *K*, *L*, *M*, Plate LXXIV. It will be observed that I have a broad, comparatively shallow head, good wide, heavy flanges and strong webs. These large sections admit of this. The edges of flanges are square, with slight radii on corners. The object of square edge of flanges is to give the best attainable contact surface for spikes. The rounded edges soon wear into spikes, thus allowing rails to spread the amount represented by wear in flanges and spikes. This form of flange will reduce this evil to the minimum.

I do sincerely hope that your Society may be able to formulate a series of standard sections and recommend their adoption. It will secure better work at the mills, for where now, owing to the many and wonderfully and fearfully constructed sections, we are compelled to keep a ten-acre lot full of rolls, we could afford to keep duplicate rolls, or even triplicate rolls of each standard section. Then mills could afford to carry a stock of standard rails subject to immediate shipment when necessary.

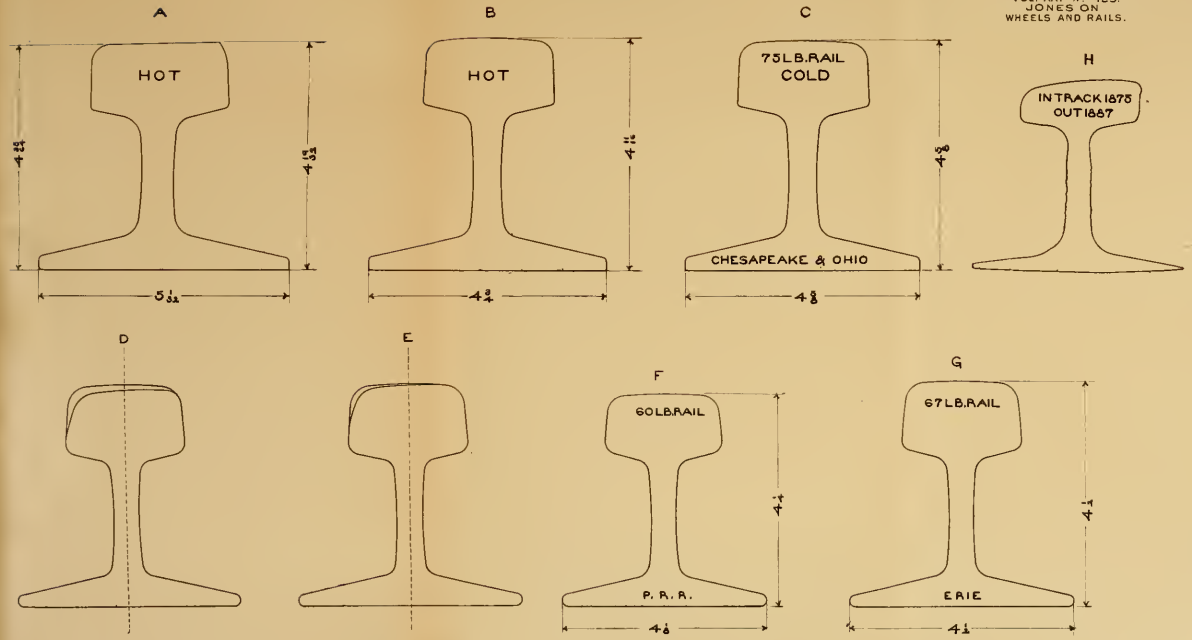
Pardon me for speaking on one important subject: The rolling of steel at the lowest possible temperature has many warm advocates. Conceding that many mills roll too hot, yet it must be borne in mind that there are six mills which now roll rails at the lowest heat possible.

Walter Katté, M. Am. Soc. C. E.—I am not inclined to admit the conclusion that the tendency to "sharp flange" is a natural phenomenon existing in all perfectly running wheels, but am rather of the opinion that it results very largely from some imperfection in the wheels themselves; probably to a very large extent, wheels out of true circle, not regular in the profile contour of their flanges and treads and being set "out of square" in their truck frames.

The present standard 80-pound rail of the New York Central and Hudson River Railroad, of which I inclose a cross section (Fig. 17),

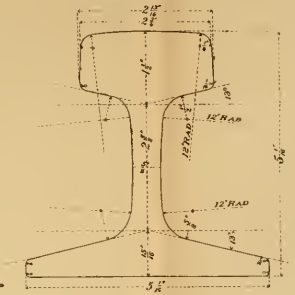
*The difference between the two 85 and 90 pound sections is in the angle of sides of head and radii of fillets connecting web and flanges.

PLATE LXXIV
TRANS. AM SOC. CIV. ENG'RS
VOL. XXI Nº 425.
JONES ON
WHEELS AND RAILS.



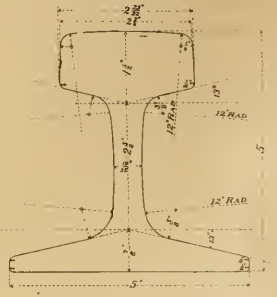
J. No. 1 90 LBS.

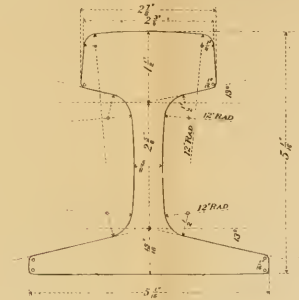
Head 40,56 per cent.
Web 22,61 "
Flange 36,83 "



L. No. 1. 85 LBS.

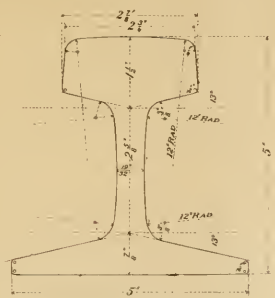
Head 43,08 per cent.
Web 22,56 "
Flange 34,36 "





K No. 2. 90 LBS.

Head 41,35 per cent.
Web 21,82 "
Flange 36,83 "



M No. 2. 85 LBS.

Head 43,72 per cent.
Web 21,92 "
Flange 34,36 "

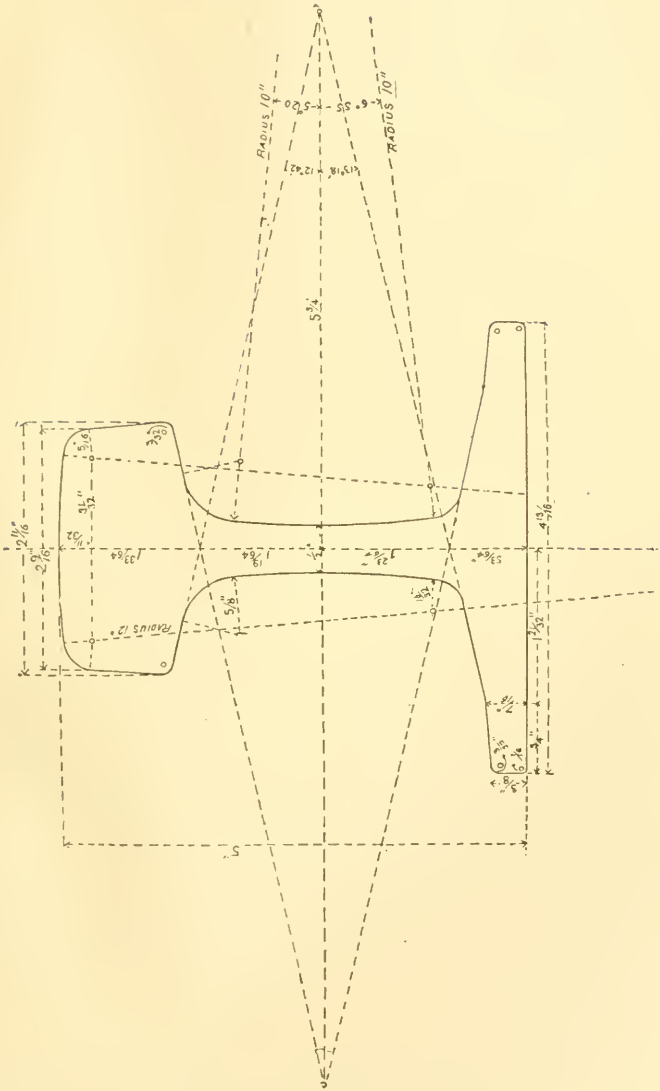


FIG. 17.
New York Central and Hudson River Railroad. Standard 80-pound steel rail, 1888.

has a top width of $2\frac{9}{16}$ inches sides, including outward inclination of $\frac{1}{16}$ inch on each side, makes the width at bottom of head $2\frac{11}{16}$ inches, with a head depth of $1\frac{33}{64}$ inches. This rail is giving very satisfactory results under exceptionally heavy traffic.

I regard the entirely flat top as the theoretically correct form, but necessarily with it must be used cylindrical, not coned wheel treads; but until all coned wheels are withdrawn from the service I think the 12-inch top radius must be maintained.

I do not advise recommending an entirely flat top until all coned wheels have gone out of the service.

I would furthermore add, that for the purpose of obtaining such results as I could from observations upon the rails and wheels of the New York Central and Hudson River Railroad and the West Shore Railroad, I addressed the following list of inquiries to William Buchanan, Esq., Superintendent Motive Power, New York Central and Hudson River Railroad; J. M. Boon, Esq., Superintendent Motive Power, West Shore Railroad; William D. Otis, Esq., General Roadmaster, New York Central and Hudson River Railroad.

First.—What is the percentage of wheels taken out of service for "sharp flange" as compared with removals for all other defects, of all classes of rolling stock, since the adoption of the present standard 65 and 80-pound steel rails?

Second.—What was the same percentage prior to the adoption of these rails?

Third.—Do your observation and records show that the present rail shapes (that is "sharp corners" and nearly flat "tops") are more (or less) productive of "sharp flange" and "hollow tread" in the wheels than the older and more rounded heads?

In reply to which Mr. Buchanan answered as follows:

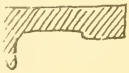
"In reply to yours of December 12th, will say that the percentage of wheels taken out of service for sharp flanges as compared with the removal for all other defects of all classes of rolling stock since the adoption of our present standard of 65 and 80 pound rail, is 10.56 per cent. and the percentage prior to the adoption of these rails was 13.14 per cent. My observation and records would indicate that the present rail shape 'sharp corners' and nearly flat top is less productive of hollow treads in the wheel than the older and more rounded heads.

"It is my opinion that the shape of the corner of the rail has but little bearing upon the subject. Sharp flanges are seldom found on two wheels that are on the same axle; they are more generally caused by trucks being out of square, 'not true,' also wheels of different diameters being on same axle, as well as curvature of the road."

Mr. Boon answers as follows:

"In answer to your inquiry: the average of wheels removed for sharp flanges is about 20 per cent. of the removals from all causes. In my opinion the throat of the flange should be made to conform to the shape of the head of the rail; or, in other words, the radius of the curve of the corner of rail and throat of flange should be the same. If they

are not, the flange will wear to fit the rail. This wearing will of course be a loss to both rail and wheel. Some years ago I fitted up a lot of wheel chills for a foundry. We had wheels with different radius of throat to rail corner. I made templates and watched result. In every case the wheel was, in a short time, to the radius of the corner of rail head. I believe it makes but little difference in the wear of either wheel or rail whether the corner of the head of rail be large or small radius provided the curve is the same on both. I furthermore believe that the shape of the head of the rail has but little to do with the making sharp flanges. If it had, when one wheel on an axle showed a sharp flange, both wheels should show the same defects. As a matter of fact, they do not. One wheel will be sharp flange and almost invariably the opposite wheel will show a full flange—showing the sharp flange was caused by defect in wheels or truck. When both wheels have sharp flanges, it will be found that they are too wide in the gauge, the flanges wearing sharp from being forced between the rail by the weight on them. We occasionally have a sharp flange on a steel-tired engine truck or car wheel. They will be worn this shape: the throat of the tire worn to the same curve as corner of rail head. We put this wheel in lathe and turn to this shape: do not turn out the point where the corner of rail head had worn. I have placed wheels treated in this way in front of engine truck on West Shore engines and watched them. In about one year it would be the same as opposite wheel. Now if the rail had worn this sharp flange in the first place, why should it not continue to wear sharp when it had no protection at all, but merely an $\frac{1}{8}$ or $\frac{1}{4}$ inch raise on tire at outside edge of throat? Again, on the sharp curves of the West Shore if the rails wore the flanges, our driving-wheel flanges would wear sharp. In fact they do not. I do not know when I have seen a sharp flange on one of our driving-wheels, and they all come here for turning. Occasionally we see one of the flanges showing signs of wear on one side of engine or one on each side on different axles. We know then that the engine is out of square or the distances between wheel centers is not exactly the same. This is corrected and the wear of flange stops. The trouble we have on West Shore is, the flange becomes heavier and closes up the gauge. Sharp flanges are caused by wheels of unequal diameter on same axle, one wheel wearing faster than the other; trucks being out of square; center pin being out of the center; distance between wheel centers not being the same; side bearings being fast, not permitting truck to curve. The great number of sharp flanges are found on freight equipment, the trucks of which, as a general thing, are too light for the work, of poor design and construction. Locomotive and passenger equipment furnish the least sharp flanges, because the trucks, etc., are of better design and construction."



Mr. Otis replies through his road engineer, Mr. George R. Huse, as follows:

"My own observation, and conversation with different road-masters on the line, give evidence of the superior qualities of the old 'English' rail, but whether due to the quality of rail steel or the form of head I cannot say, but I think much the most is due to the quality of the metal and manner of working. I have been much interested of late in some articles in the *Engineering News* relative to the effect upon the wheel tread and flange of the two styles of rail head. The sharper

cornered and flat-top rail seems to make the best showing so far as wear of wheels is concerned, and plates of both sections of rail worn down correspond very closely in wear, while the flange shows worn sharper with the round head than with the flat." Mr. Otis adds "that he does not think that round heads wear as fast, especially on curves."

These replies I submitted to the review of Mr. Theodore Voorhees, M. Am. Soc. C. E., Assistant General Superintendent New York Central and Hudson River Railroad, which he gives in the following words:

"In reference to inclosed letters, I beg to indorse Mr. Buchanan's statement in full. I do not fully agree with Mr. Boon, inasmuch as I believe we get best results from a rail with a sharp corner in the head, not necessarily the same as the angle of the flange of the wheel. In other words, would prefer the wheel and rail should not exactly fit."

In conclusion, therefore, I would say that the final result I have arrived at in my own mind after a very careful study of all that has been said and written, together with my own observations and experience on this subject, is that the true theoretical form of the "relation of wheels and rails" is that of a cylinder rolling on a plane, which, reduced to practical form to give the best economical results, is the following:

Wheel.—Cylindrical tread.

Fillet radius, not less than $\frac{3}{4}$ inch.

Rail.—Flat top, ultimately, 12 to 14 inches radius for the present.

Top corner radii not to exceed $\frac{1}{4}$ inch.

Width of head as large as it can be made within the limits of perfect rolling, and not deeper than $1\frac{1}{2}$ to $1\frac{5}{8}$ inches for an 80-pound rail.

Stem not less than $\frac{1}{2}$ to $\frac{5}{8}$ of an inch.

Width of base, not less than the height.

Thickness of the edges of the bottom flange not less than $\frac{1}{4}$ inch, and for 80-pound rail $\frac{5}{16}$ or even $\frac{3}{8}$ of an inch is not too much.

Olin H. Landreth, M. Am. Soc. C. E.—Sharp flanges are not eliminated by any form of wheel or rail under discussion.

I do not consider that the statistics presented warrant the conclusion as a general rule "that when the wheel is sound and the quality even, about one-half the wheels get sharp flanges."

I consider "that wheels which fail from flange wear are among the most durable, if not the most durable, approximating if not exceeding the life of those which fail from long continued tread-wear alone," but I do not consider that this fact warrants the further conclusion that "no appreciable money loss results from wheels running to flange instead of failing from tread-wear only, since the money loss incurred is not limited to wheels withdrawn on account of sharp flanges, but includes the following losses: 1st. The loss of the remaining unknown life or mileage of the wheel withdrawn for sharp flange. 2d. The loss due to side rail-wear corresponding to flange-wear. 3d. The loss due to tread-

wear of three wheels of the truck caused by lateral sliding friction due to the tendency to run to flange. 4th. The loss due to lateral top rail-wear corresponding to 3d. 5th. A loss due to motive power expended in producing the wear of items 1st, 2d, 3d and 4th.

While I am not prepared to reject as untrue the fact "that the rate of wheel wear grows very materially greater as the rail corner is rounded off to fit the flange," I still consider that the data and facts presented, as well as my own observation, leave the truth of this proposition unproven. My reasons are—first, I have plotted the values of the tonnage and rate of rail-wear given in Tables Nos. 8 and 9, and they show such large discrepancies between individual results as to attach great uncertainty to the slight tendency observed toward an increase of rate of wear with increased age—*e. g.*, the plotted curve of wear on tangents and on 3-degree curves shows discrepancies so great as entirely to mask any tendency in either curve toward an increase or a decrease of rate of wear with age. The curves of wear for 1-degree and 4-degree curves show tendencies to increase of rate of wear on a portion of each curve and equal tendencies to decrease of rate on equal portions of the curves. The plotted curve of wear for 5-degrees curvature shows a tendency to a decrease of rate of wear with age, and the curve for 6 degrees shows a tendency to increase of rate. Second—Even if the slight tendency to increase where shown could be accepted as conclusive, this could easily be accounted for by the wearing through of the hardened skin covering the rail, since its existence is well known, and since there is no tendency to a decrease of wear with age shown by the plotted curve of wear on tangents; this does not therefore disprove the existence or effect of the hardened skin, as is claimed in the last sentence on page 30 of the preliminary report.

I consider that flat-top rails (flat when laid) would be "likely to be effective in reducing rail-wear," but I do not "advise recommending an entirely flat top," because I believe that would be overdoing the proper remedy, and would not insure so low a rate of wear as a rail-top having a radius of curvature greater than 14 inches, but not entirely flat. My reasons are based on the following considerations, which are confined to top rail-wear alone, and exclude side rail-wear as in no way effecting the special question of top curvature. When a rail has worn to a permanent form of curvature it has reached a condition of nearest possible approach to a uniform distribution of wheel-load, and hence the lowest maximum wheel-pressure across the rail-top, and therefore has attained its minimum rate of wear. Could a rail have been given at first a curvature which should remain permanent in radius, it would. therefore, insure a minimum rate of wear throughout the life of the rail, Such a curve should be sought, and when determined should be adopted in the manufacture of the rail. A top curve which should remain permanent throughout the life of the rail would doubtless differ from the

permanent curve assumed by a rail that had worn to its permanent form from a curvature originally sharper or flatter, since the form of the original curve would effect the form of the tread-wear of the wheels rolling on it, and this form of tread-wear would in turn effect and determine the form of the ultimate curve assumed by the rail-top. Car wheels rolling over rails ranging in original curvature from 6 to 10 inches top radius ultimately wear all rails on which they run to an average top radius of about 14 inches on tangents and about 10 inches on curves. Were all rails of a system laid originally flat and all car wheels running over them originally cylindrical, it appears impossible to escape the conclusion that the rails would ultimately wear to a curved form and that the permanent curve assumed would be flatter in curvature than if the rails had been laid with the usual 6 to 10-inch radius. The grounds for these conclusions are :

First.—It would be impossible to lay rails or mount wheels or maintain either so truly as to insure a uniform bearing of the cylindrical wheel on the flat-top rail; owing to this, there would result an augmented wheel-pressure and an augmented wear toward either one edge of the rail or the other, or both edges successively, according to the condition and rigidity of the rail fastenings, and a curved form of the rail-top would result.

Second.—Even were this impossible uniformity of wheel pressure across the rail attainable, the different portions of the rail-top are not equally strong against wear under heavy compression; the portions near the edges having less metal outside them would be deformed under lighter pressures than the central portions, and the same effect, namely, a curved rail-top, would be the result.

Third.—The curve thus produced would gradually become sharper until a permanent form should be reached, which, however, would be flatter than if the rail had been laid with the usual curvature of 6 to 10 inches radius, since the wheels starting originally with straight cylindrical treads, and being worn more by the outer portions of the rail-top than by the inner, would remain straight across the tread, or, if worn curved, would assume a flatter curve than if the rails had been laid with the usual sharp top curve; hence the permanent curve assumed by the rail—which is dependent on the curve of the wheel-tread—would also be flatter than the permanent curve which the rail would have assumed had it been laid with the usual sharp curve—*i. e.*, the permanent curve would be flatter than 14 inches on tangents or 10 inches on curves. The exact value of this radius it is impossible to determine in the absence of experience with flat-top rails, but it is hardly probable that it would be less than 20 inches under ordinary conditions of track, or 30 inches where unusual precision in track-laying or great rigidity of rail-fastenings should exist. If now a rail of 6 to 10 inches original radius wears flatter and assumes a permanent curve of about 14 inches radius, and a flat-top

rail wears sharper and assumes a permanent curve of from 20 to 30 inches, say 25-inch radius, it follows that the curve sought, viz., one that would wear neither flatter nor sharper, should have an original curve radius of between 14 and 25 inches; the precise value of this "radius of no variation," while falling between the above limits, could be precisely determined only by experience. I am strongly inclined to the opinion that rails laid with such a top curve—*i. e.*, one of about 20 inches radius—would result in a lower rate of top rail-wear than either of our present forms or Mr. Whittemore's proposed flat-top rails, and would give equally low rates of side rail-wear.

Mr. Sandberg objects to the flat-top rail from the "producers' point of view," for the following two reasons:

1st. The heads of flat-top rails would be more porous than curve top, because the flat top necessitates less pressure or work in the last roll-groove, and hence less closer grained than the curved top.

2d. The heads of flat-top rails would not deliver so well from the rolls, and more "wasters" would be the result.

The first objection appears to me to touch the true root of the evil of undue rail-wear, viz., a lack of density and hardness in the heads, but should not be directed against flat-top rails alone and made a ground for their rejection, for the difference of density of rail head due alone to the form of curve of rail top must be quite insignificant since it is measured by the versed sine of one-half of the top curve, which in the flat-top rail would be zero, and in Mr. Sandberg's rail of 6 inches radius, $\frac{1}{12}$ of an inch. In either form the density is mainly given—as in the perfectly flat base of the rail—by the pressure or work of the rolls acting upon the semi-fluid metal and forcing it against the vertical surfaces of the rail-grooves, which act simply as moulds to shape the rail base and head. As the rail metal at its passage through the last grooves is far from being perfectly fluid, the final pressure on the rail-head and rail-base is far from being equal to that produced by the rolls on the web and sides of the head. This remarkable fact, that the portion of the rail on which the least pressure and work is expended is the very portion on which the heaviest and most severe service falls, points to the desirability of considering if modifications or additions to our present mode of rolling rails may not be possible.

The second objection, concerning the difficulty of delivering flat-top rails from the rolls, is doubtless a valid one, but one which would be obviated by giving the rail any appreciable curvature which should release the horizontal grip on any one portion of the rail head as soon as that portion should have passed the point of contact with the rolls, or, as Mr. Sandberg says, "as long as the rail is not flat;" hence a 20-inch radius would not be subject to this objection.

A. P. Man, Jr., M. Am. Soc. C. E.—I think this matter of shape of head has not been discussed (except corner) sufficiently in this article to decide

as between wide or deep head. It is entirely a question as to where the metal is needed for wear. According to this discussion seven-eighths of all rails are not on outside of curves. Is it wise to put the metal on as if all were on the outside of curves? Such may, however, be deemed the weak point to be protected, perhaps. I incline to favor the deep head rather than the broad, even if a fish-plate only be used at joints on inside of rail and a heavily reinforced angle plate to match on the outside. Is there any important objection to such an arrangement?

Hunter McDonald, M. Am. Soc. C. E.—The tendency of all well mounted and matched wheels is to hollow treads, which, of course, have a curved surface on account of lateral play; a flat-top rail will not reduce this tendency.

When treads are worn slightly hollow the bearing will be on the corners of the flat-top rail alternately, seldom on both corners at the same time on account of greater width of concave surface of wheel than head of rail.

The crushing force of load will be more disastrous to the rail if applied to unsupported corners than to the supported middle of rail.

The curved-top rail will practically afford greater bearing surface than the flat-top.

Most of the defects in sections, illustrated in Mr. Whittemore's paper, are defects of construction and not directly the result of curved-top rails alone.

The tendency of flat-top rails will practically be to increase the corner radii.

Cylindrical wheels will soon wear hollow, and most wheels now in use are already more or less hollow.

Unless the change were made simultaneously on all systems, Mr. Whittemore's desired result could not be reached, because the flat-top rail would hollow the tread of cylindrical wheels, and the already hollow treads would round the corners and top of the rail.

Mansfield Merriman, M. Am. Soc. C. E.—As I have cast my vote in the final report of the Committee on Wheels and Rails in opposition to the flat-topped rail proposed by Mr. Whittemore, it may be well to give a reason for the faith that is in me, which is based not only upon the facts set forth by that committee, but on the fundamental laws of sliding friction. It is now, of course, well known that the old laws of friction, as stated by Morin, are but rude approximations, and it is well established by experiments made during the past twenty years that the co-efficient of sliding friction varies not only with the velocity but also with the intensity of the normal pressure. When the pressure per square inch is high, the co-efficient of friction, and hence the total resisting force of friction, is less than when the pressure per square inch is

low. The application of this principle to the contact between wheel and rail seems to indicate that the top of the rail should not be flat, since on account of the difference between the wheel gauge and the track gauge there is a lateral sliding of from ½ to ¾ of an inch. Previous discussions have pointed out the fact that probably this lateral sliding is one of the main causes of the wear of the rail. Now, if the line of contact across the rail is large, say 2 or 2¼ inches, the pressure per square inch is less than if that line of contact is 1½ or 1 inches, so that in the former case the co-efficient of friction is the greater, and hence the wear of the rail is greater, as also is the amount of fuel required to pull the train. If this sliding friction were the only thing to be considered I should advocate a small radius for the top of the rail, but as the wear due to pounding and crushing is also large, we are between two fires, and must select a mean radius which is not small and also not very large. It appears to me that, considering all the elements of the question, the committee's proposition for a 12-inch radius is as fair a mean as we can arrive at under our present knowledge of the subject.

James A. Millholland, General Manager George's Creek and Cumberland Railroad.—I fully agree with the committee that, as the bearing surface on the top of the head of the rail is narrowed by a rounding off of the upper corner of the head, the wear upon the head grows very materially. My position, however, is that as the corner is rounded to fit the wheel fillet, from a former sharpness, the wear upon that corner does not grow as rapidly, as it approaches a fitting with the wheel fillet, as it did when it was relatively sharper.

S. Fisher Morris, M. Am. Soc. C. E.—I see no objection to the Michigan Central rail, but would modify it by making the top entirely flat, which is the form I recommend. I think that a flat top will reduce rail wear and give longer life to both rails and wheels.

John Newell, M. Am. Soc. C. E.—My views on the subject of rail sections are expressed in the accompanying sections for 65 and 70 pound rails, Plate LXXV, the latter the heaviest weight used upon our line, the Lake Shore and Michigan Southern.

An inspection of these will give my views better than I can in the limited time I have to devote to the subject.

Ernest Pontzen, Cor. M. Am. Soc. C. E.—The tendency to sharp flanges is a consequence of the rigid connection of the axles of the truck or car. The wear of flanges increases with the distance apart of the rigid axles; it will diminish when the tread of the wheels is coned and the top of rails curved. If the rails are soft, the sharpening of flanges will progress less quickly.

If the rail corner and the side face of the head fits the fillet of the flange, the wear of rail and of wheel will be great, as there will be friction on all points of contact which are not situated on the rolling line.

If the shape and material of the wheel treads and the distance between the axles were altered, it is probable that the radii observed on worn rails would not be the same as those named (12 inches and $\frac{1}{4}$ inches).

It is most important to have the fillet of flange well chilled; as that can be secured better by a large fillet radius, I would admit a $\frac{3}{4}$-inch fillet radius.

I would admit the 12-inch radius or even a flatter top, but only if the tread of the wheels is cylindrical. In case of conical tread I would reduce the radius of the top to about 9 inches. In the first case the flatter top reduces the chances of producing grooves in the tread; in the second case, the shorter radius seems to me to be suitable for maintaining the wheels in proper position without the aid of the flanges, and it also prevents more effectually the squeezing out of metal on top of the rail. Such deformation of the top of rail results at once in a reduction of duration of rails and of wheels, as the metal thus protruded increases the wear of the fillet of flanges.

Some difficulty will be met in rolling rails with flat top. I would for these reasons give preference to rails with a curved top, the radii being selected in accordance with the shape of the wheel tread (cylindrical or conical).

I had wished to furnish some drawings taken from rails used on our railroads, but could not in time for publication in the present discussion.

The distances between the axles of our cars (four-wheeler or six-wheeler) are somewhat greater than in American rolling stock, as the greatest number have been built without trucks. I hope that in time the utility of trucks will become more appreciated in Europe.

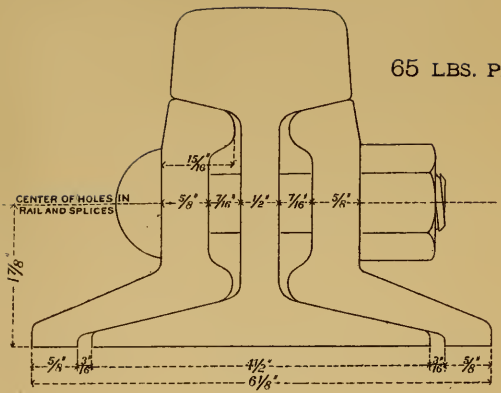
Messrs. Thomas Prosser & Son.—We take pleasure in handing you a circular that we have issued regarding flange and tread of wheels and tires, and believe that if all the roads adopt the Master Mechanics' and Master Car Builders' standard flange and tread for all tires and wheels it would greatly help to reduce sharp flanges and unevenly worn rail-heads.

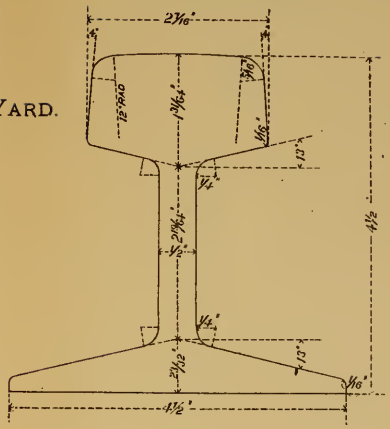
As we understand the matter, it is the desire of your Society to adopt a standard rail-head which shall be suitable for the standard flange and tread adopted by the Master Mechanics' and Master Car Builders' Associations, as we presume that there is no question about many accidents having occurred from the flange and tread not fitting the rail perfectly.

In our circular we state that, unless otherwise requested, we will in future furnish all locomotive driving wheel tires, leading locomotive truck wheels, tender wheels and car wheels, with the flange and tread adopted by these associations, as it seems to be the general disposition

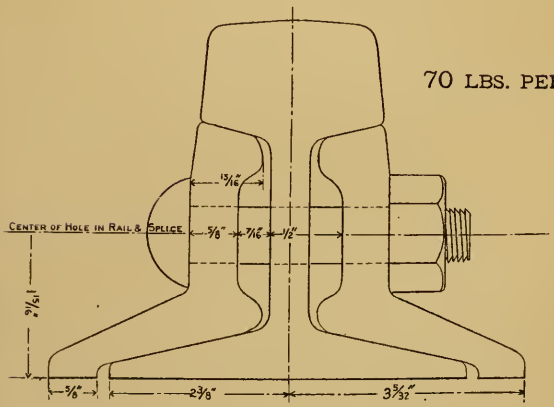
PLATE LXXV
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XXI. Nº 425.
NEWELL ON
WHEELS AND RAILS.

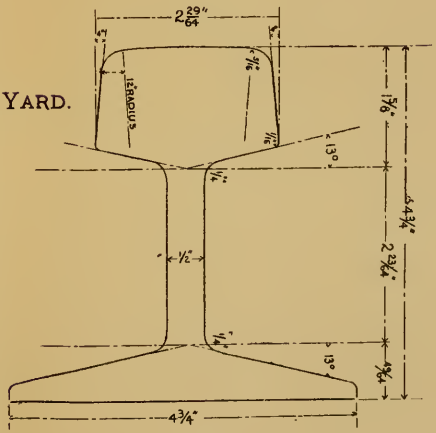
65 LBS. PER YARD.





70 LBS. PER YARD.





LAKE SHORE & MICHIGAN SOUTHERN RAILROAD.

of railroad officials to adopt this flange and tread as a standard, and most of the orders we have lately received so specify.

A few months ago we had on file between seventy-five and one hundred different styles of flanges and treads, and are pleased to report that with hardly an exception the parties using them have notified us that they have adopted as a standard the flange and tread adopted by the Master Mechanics' and Master Car Builders' Association, and it is hoped it will be made the "standard" in every sense of the word.

S. W. Robinson, M. Am. Soc. C. E.—From the study of ball bearings, as consulting engineer for the mountings of the Lick telescope, I am certain that Mr. Whittemore is correct in keeping the load within the elastic limit of wheel and rail, and that the top should be very nearly or quite flat for best results in service. Although I think a perfectly flat top cannot be maintained in service, yet I believe we should seek to obtain as nearly that as possible.

A. A. Robinson, H. V. Hinckley and Lewis Kingman, Members Am. Soc. C. E. (by joint letter).—No doubt sharp flanges are a natural and inevitable phenomenon to some extent, but we believe it is well to have the rail and wheel sections correspond as far as practicable at the place where the most wearing is done, and that, as in any machinery, a respectable "fit," supplemented with an avoidance of surplus loose motion, will not increase the wear.

The committee think wear is largely due to trucks being out of square and to unequal diameter of wheels. Tangent rail wear, however, on the Santa Fé (of which Figs. 1, 2 and 3, Plate LXXVI, are samples of sections taken at various points on the system) does not show on the side of the rail-head where we should expect to find some of it if (in connection with the flanges in actual service, Fig. 7) these causes of wear were so prominent.

As to quality of wheels and defects therein having much to do with their wear, there is no doubt.

As to the conclusion of the committee, that no money loss results from wear running more to flange than to tread, we do not fully agree with them. If the head of rail has insufficient height the rapid wear of flanges means the early contact of splices and flanges, and it is not impossible that this contact on curves has been the cause of some of the many "derailments, cause unknown."

With a high-headed rail, flange-wear is also evidently not desirable, for on any rail-head we do not want the flange and splice to touch till all the available wear has been utilized from the crown of the rail. On the Santa Fé, as a matter of safety to passenger traffic, the wheel tires of all passenger equipment and of over 90 per cent. of engines are steel.

This accounts for the nearly vertical flange indicated by heavy line in Fig. 7, Plate LXXVI (which shows sample sections from wheels in actual service on the Santa Fé), and the nearly vertical wear on side of rail heads on the Raton Mountain (Figs. 5 and 6), where maximum grade is 185 feet per mile, and where engine and passenger car wheels are all steel. We call attention to this fact in connection with Figs. 5 and 6, because when splice and flange touch, the rail is no longer fit for service either under passenger traffic or otherwise.

On account of heavy mountain grades the Santa Fé can not go back to cast-iron wheels for passenger service, and with steel prefers that wear shall not be excessive upon flange as a matter of economy to both wheel and rail. As to radius of upper corners of rail the Santa Fé has always used $\frac{3}{8}$ of an inch. We prefer the smallest radius practicable for both wheel and rail.

Fig. 8, Plate LXXVI, shows the Santa Fé 71-pound rail, and its other sections are similar.

Fig. 9 shows the recently adopted 65-pound rail of the Denver and Rio Grande.

The Santa Fé rail has nearer the contour of the flange, and consequently must lose more metal in service before it will lose the same amount of gauge.

On the other hand, the Rio Grande rail has more metal in the top of the head to wear, and is stiffer.

Rails made with heads tapering like those of the Santa Fé may get the same splice-bearing by using vertical sides and $\frac{1}{16}$ radius, or a point for lower corner of section of rail head. This change we are ready to adopt. We want the rail and wheel to have the same radius, and are not in favor of a $\frac{5}{8}$-inch radius for wheel fillet and a $\frac{3}{8}$-inch rail corner to cut into it.

With the wheel section, which we understand to be the present standard, the result would be as shown in Fig. 10. We prefer, however, that the radius of wheel fillet and upper rail corner both be $\frac{3}{8}$ of an inch. Then if vertical sides be advocated we are ready and glad to adopt them.

Whether rail top be flat or not, the tread of a cylindrical wheel will always wear hollow, and the hollow will not fit a flat rail top, but, on account of centrifugal force on curves and the oscillations on any track, will be a curved line substantially as with old style coned wheels. With flat-top rail the bearing of load would be at extreme corner of head instead of on crown.

All Santa Fé rail-heads have about 14 inches radius. With the coning of wheels that have in years past gone over the system, the general shape of the crown has been preserved (see Figs. 1, 2 and 3), and we prefer to retain a radius of either 12 or 14 inches for top of rail, though possibly 10 inches would be best. The radii of worn wheels seem to average about $\frac{3}{8}$ of an inch, varying from $\frac{1}{4}$ to $\frac{3}{8}$ and more.

Considering rails and wheels together (*i. e.*, both of them), the minimum wear appears to be when radii of parts of contact are nearly or quite alike, nor is there any doubt in our minds as to the advisability of making them so at the start.

The wear of the gauge will be less as we approach this relation at the beginning, and will be less after this condition is attained than before; therefore let us have as near this condition as possible at the start.

At all events, we hope we shall not drift back from $\frac{5}{8}$ to $\frac{7}{8}$ of an inch radius for wheel fillet.

If there is, as the committee suggests, "no particular necessity for having any single standard radius for rail or wheel," there certainly does exist a necessity for uniformity in sections and in the gauging of track and rolling stock. Whatever radii be adopted, we want four points settled:

First.—A method of gauging.

Second.—A definite place on the head of rail to which to measure the gauge.

Third.—An allowable variation each way from standard gauge.

Fourth.—An allowable variation between wheel-gauge and track-gauge.

In our opinion the matter of determination and preservation of gauge is one of the most important points in this whole matter.

The rolling stock that passes over the Santa Fé system varies in wheel-gauge at least $1\frac{3}{8}$ inches by actual measurement.

The difference in rail heads allows at least $\frac{1}{2}$ an inch more variation in gauging, and some roads do and some do not widen gauge on curves and at switches, so that the total variation of our "standard" 4 feet $8\frac{1}{2}$ inch gauge (not mentioning 4 feet 9 inch) is more than the width of the rail head and is a constant element of danger, often resulting in spreading or tearing up of track and springing of angles at switches, and the spreading of track on curves, and consequent derailment and wrecks.

Whatever will tend to reduce this variation of gauge to a minimum will have our hearty support, and the first act to that end is the adoption of standard sections and methods of gauging. We object to the extreme slope of sides of rail, as in the Lehigh Valley standard, on account of loss of metal in top of head, where most needed for stiffness and wear.

We object to the Cumberland Valley rail and wheel sections for the same reason, and the additional difficulty of placing split switches without cutting main track rail.

The advantages of a curved rail top are:

1st. Bearing over web of rail.

2d. Better contact of rail with wheel.

3d. Less resultant "flow of metal" on rail corners.

We prefer Fig. 8, either as it stands or with vertical sides and points for lower corners, but are willing to compromise and adopt Fig. 9, or even 10, if by so doing we can secure the general adoption of a standard.

Better that any one of the four sections (Figs. 8, 9 or 10, or 8 with vertical sides) be adopted than to be using all the present varieties in the same service.

C. P. Sandberg, C. E.—On the whole the rail section shown in Fig. 1 of the report, the adopted standard of the Michigan Central, is in my opinion decidedly the best of the three, and with very slight modification would meet with my entire approval, but not so the two others. I have replied to each question with "yes" or "no," but will now give my reasons for such replies.

One-quarter-inch radius is much too small for any rolling stock not provided with bogies, and absolutely dangerous for derailment with fixed axle journals, as used in Europe. It may do for America with your bogie system on both engines and carriages, but even there it is hard to see what will be the gain by such a small radius, and why not have half an inch, as I have adopted in my standard sections, of which millions of tons have been made during the last thirty years. This reply will also serve for "A," Figs. 2 and 3.

Twelve-inch top radius.—For reasons given in my reply to Mr. Whittemore's paper,* a 12-inch radius gives a less compact head than one of half that. However, as stated before, steel rolls better than iron, anything between 6 and 10 inches would do. Since my reply to this question I have seen the Manager of the Dowlais Iron and Steel Works, Mr. E. P. Martin, who rolled that very flat-topped rail,† and he entirely agreed with my statement, adding "they may perhaps retort upon you that if it is so difficult about making a flat top, how about making a flat bottom?" to which I replied that no one is having a round bottom but himself.

Vertical Sides.—I entirely approve of the same. I have kept them on my standard sections all through from the beginning. In Europe the reverse inclination is used to that in America (see section of Russian and German rails in my last paper "On the use of heavier rails")‡ This partly offers the bulk of the rail head to greater wear, and partly clearance of side friction from the tire flange, but gives a somewhat smaller support to the fish against the rail head; however, the difference is so small that the gain by the lately adopted sections Nos. 2 and 3, is hardly worth while mentioning, compared with the disadvantage of the above-mentioned main points. Besides the side friction, which in No. 3

* Transactions, Vol. XXI, p. 166, October, 1889.

† See Plate LXIV, Transactions, Vol. XXI, p. 168, October, 1889.

‡ Proceedings Institution of Civil Engineers, Vol. XCV.

PLATE LXXVI
TRANS. AM SOC CIV ENGRS.
VOL. XXI Nº 425
ROBINSON, HINCKLEY & KINGMAN
ON WHEELS AND RAILS.

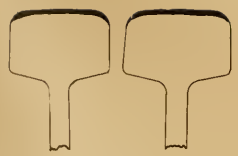


Fig. 1.

Fig. 4.

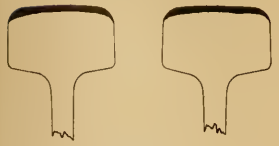


Fig. 2.

Fig. 5.

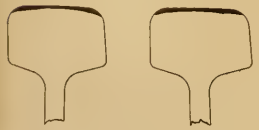
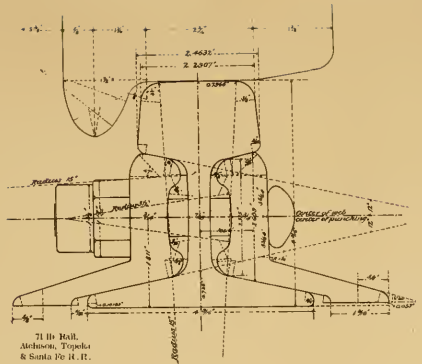


Fig. 3.

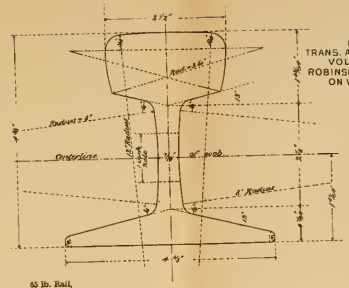
Fig. 6.

Sections of Worn Rails, Atchison, Topeka & Santa Fe R. R.



71 lb Rail,
Atchison, Topeka
& Santa Fe R. R.

Fig. 8.



65 lb. Rail,
Denver & Rio Grande R. R.

Fig. 9.

Sections from Wheels in actual service,
Atchison, Topeka & Santa Fe R. R.

Fig. 7.

Fig. 10

must be enormous, the gauge may alter by the wearing of the rails, and it cannot but be regretted that such a large quantity of rails have been made to this section in America.

The very small lower corner, $\frac{1}{8}$ to $\frac{3}{32}$ of an inch.—I approve of this entirely, as giving more bearing to the fish, and taking advantage of the metal, steel, being rolled to a sharper corner without the slightest difficulty. In fact I find the sharp corners give less wasters or cracks than the rounded, just the reverse to what it used to be in iron, and why should we then not take advantage of the new metal, steel, when it is an advantage to both producer and consumer?

A most important measure is missing on all the three drawings, namely, the width of bearing surface between the rail head and fish, and in practice it is absolutely necessary that the fish-plate touching surface of the rail should be as straight as a line, not rounded as it is inclined to be with the usual mode of rolling, this being all the more important when using the three tie fishes of 44 inches in length, as they would be no good unless a perfect fit to the rail be effected from the commencement, and as the straightening of such a long fish affords considerable difficulty, this may be one reason for its slow adoption.

The very broad head.—I entirely approve of it for two reasons: first, that the broad head would be more compact than a narrow thin one; secondly, that it would offer more wearing surface to both rail and tire, and give increased traction power for the same weight of engine. For this reason my "Goliath" rail is designed $2\frac{5}{8}$ inches, and the Belgian Goliath is $2\frac{7}{8}$ inches, and I do not see the reason why it should not go up to three inches, or, approaching the wearing surface of the tire, say $3\frac{1}{4}$ inches, all for heavier rails where the substance or depth of rail at the edge would not be less than $1\frac{1}{4}$ inch, so as to allow at least $\frac{1}{2}$ an inch wearing surface from the top.

Another important question missed on the three drawings is the thickness of flange, this measure being entirely left out. In this respect Fig. 1 seems to be the best, Fig. 2 entirely condemnable owing to the thinness of the flange, which would necessitate rolling the steel of such softness as to endure but little wear, which is now sadly proved in America, and from which a wrong conclusion is often drawn, viz.: that it is the fault of the big head, that actually gives no more wear than the small head. I have devoted two pages (14 and 15) in my paper to this important subject.

R. I. Sloan, M. Am. Soc. C. E.—I inclose herewith a section of rail, Fig. 18, which was taken up after about four years use on the Manhattan Elevated Railway, at the Coenties Slip curve, in New York City. The lines of the top of the rail show that it had worn nearly flat. In my opinion, formed from my observation on this road, it does not make much difference whether the top of the rail is flat or slightly curved; the wheels wear the surface down to accommodate themselves.

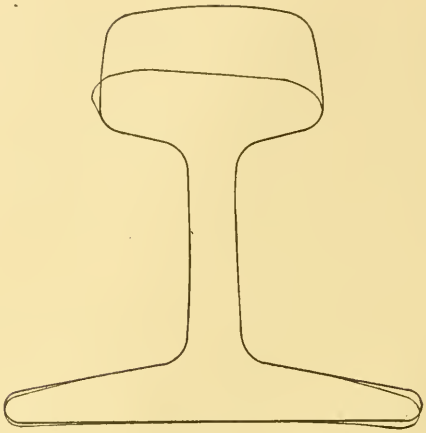


FIG. 18.

63-pound rail used on the Manhattan Railway at Coenties Slip, showing section October 12th, 1883, when laid, and section September 5th, 1887, when taken up. Radius of curve 125 feet (outside rail).

The following is a statement of the traffic over this rail:

Third avenue traffic from October 12th, 1883, to September 5th, 1887:—1 423 days, 501 trains, 2 048 cars per twenty-four hours.

Second avenue commenced running April 12th, 1885. Traffic from April 12th, 1885, to September 5th, 1887:—876 days, 395 trains, 1 523 cars per twenty-four hours.

	Cars.	Engines.
Third avenue	2 914 304	712 923
Second "	1 334 148	346 020
	4 248 452	1 058 943

	Tons.
Average weight of cars, 14 tons	59 478 328
" " engines, 17 "	18 002 031
Total	77 480 359

A. J. SWIFT, M. Am. Soc. C. E.—States that he does not advise recommending an entirely flat-top rail, because he thinks coning desirable to steady cars, and that coned wheels do certainly bring about curved tops on rails.

Also that any deductions made from wear caused by wheels of varying gauge and template cannot be a safe guide for the future, when ⅝ of an inch fillet will probably be universal and a uniform gauge may be expected and hoped for.

G. H. THOMSON, M. Am. Soc. C. E.—I do not consider that the evidence submitted is sufficient to warrant the assumption that "the tendency to sharp flange is a natural phenomenon, which must exist in all perfectly running wheels," for the reason that the tendency is not inherent in the wheels themselves but is from the slewing of the trucks, causing them to run "out of square," and this is due to the short wheel base. If the distance between the wheels of an ordinary freight car truck were reduced say to 38 inches, the phenomenon of "sharp flange" would be more frequent, for the reason that the flanges of the wheels would then impact the rails at an angle more favorable toward the production of "sharp flanges." If the angle of impact be improved by increasing the distance between the wheels, the number of sharp flanges would decrease. Minor causes, such as mis-mated wheels, soft chilling, etc., localize the sharp flange and determine the life and wear, in a measure, of the wheel.

The Dudley section laid in 1884 upon the New York Central is, although soft (.32 carbon), as a rule attesting the mechanical wisdom of a 12-inch top radius, and in the first year of its use double flanges on driving wheels were the rule rather than the exception. Its radius after five years' use is still 12 inches.

I object to the wide and thin flange of the Michigan Central section, (the thickness of the rail base is not given, but is understood to be $\frac{5}{16}$ of an inch, and the section rolled .45 to .50 carbon).

I would prefer the $\frac{1}{4}$ inch radius under the head increased to $\frac{1}{2}$ or $\frac{5}{8}$ of an inch.

I object to the vertical sides of the head.

The above objections hold for this section at 80 pounds and at .45 carbon.

A comparison of the Michigan Central section with P. H. Dudley's section of the same weight, designed in 1883, is herewith presented. It does not appear that the Michigan Central section is as stiff as the Dudley section; nor can it be rolled harder or at a better finishing heat, and after wear and oxidation the latter section would still be the stiffer one.

COMPARISON OF RAIL SECTIONS.

	80-pound Dudley, 1883. Square inches.	80-pound Michigan Central, 1889. Square inches.	Difference. Square inches.
Head area	3.44	3.29	— 0.15
Neck "	1.60	1.72	+ 0.12
Base "	2.73	2.86	+ 0.13

	Dudley. Per cent.	Michigan Central. Per cent.	Difference Per cent.
Head area	44.2	41.8	— 2.4
Neck "	20.6	21.8	+ 1.2
Base "	35.2	36.4	+ 1.2
	100.00	100.00	

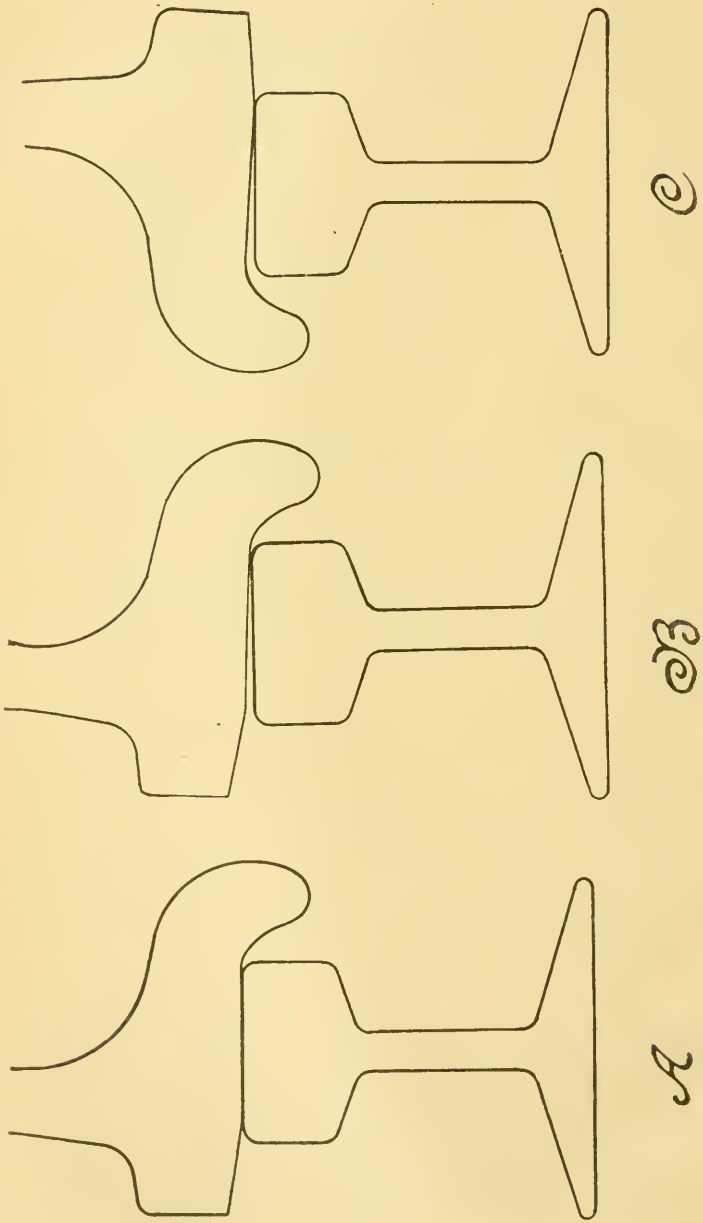


Fig 19.

	Inches.	Inches.	Inches.
Head top radius	12	12	0.0
" corner radius	$\frac{5}{16}$	$\frac{1}{4}$	$-\frac{1}{16}$
" lower "	$\frac{3}{32}$	$\frac{3}{32}$	0.0
" depth	$1\frac{33}{64}$	$1\frac{1}{2}$	$-\frac{1}{64}$
" top width	$2\frac{9}{16}$	$2\frac{1}{2}$	$-\frac{1}{16}$
" bottom width	$2\frac{11}{16}$	$2\frac{1}{2}$	$-\frac{3}{16}$
Height	5	5	0.0
Neck thickness	$\frac{1}{2}$	$\frac{9}{16}$	$+\frac{1}{16}$
" top radius	$\frac{5}{8}$	$\frac{1}{4}$	$-\frac{1}{8}$
" bottom radius	$\frac{11}{32}$	$\frac{1}{4}$	$-\frac{3}{32}$
Base width	$4\frac{13}{16}$	5	$+\frac{3}{16}$
Base thickness, least	$\frac{3}{8}$	$\frac{5}{16}$	$-\frac{1}{16}$

I do not think that at the present time the absolutely flat-top rail will reduce rail wear, especially on trunk lines, interchanging with foreign roads, with all kinds of wheels, "double flange," etc.

I think there are objections to the absolutely flat-top rail. In a straight axle with the wheels and the flat-top rail recommended by Mr. Whittemore in his invaluable paper, that coincidence of face line of wheels and top of rail sought (shown at "*A*" on Fig. 19) obtains only when both rails are absolutely level, or lying within the same plane, but this perfection of track surface is unknown; lower one rail $\frac{1}{8}$ of an inch, and the concentrated bearing (shown on Fig. 19 at "*B*" and "*C*") results, with probable consequent crushing rather than wearing of the rails.

Also on curves, the "rocking" of the outside rails, even if level, or inclined in the same plane, would tend to shift the bearing toward the corner of rail. It seems to me that the cylindrical wheel and rail of very broad head and of large radius (say 20 inches or more) would be the best economy, but only when an absolutely uniform track surface can be cheaply maintained. This cannot be done now with T-sections and angle joints, but if some new sections permitting a broad head be devised, and if the joints can be got rid of, the problem is solved, and the art of railroading will experience a most decided advance in rail economy. With top radius large and rails slightly out of surface as referred to each other, the tread line of cylindrical wheels will be tangential to some part of the rail top, and therefore the wheels will be always in relatively wide bearing.

I do not at present advise recommending an entirely flat top under the conditions the flat-top rail would be subject to on most railroads.

In conclusion, I will add that too much in the way of protest cannot be said against the use of 60 000 capacity coal cars as referable to rail endurance and economy.

W. Howard White, M. Am. Soc. C. E.—By counting up the rail sections on pags 38 and 39 (pp. 244 and 245, *ante*), of the preliminary report of the committee, which show a corner radius of 0.26 or under, I find fif-

teen out of thirty-two in this category, and it therefore does not seem to me proven that the head generally wears to about ¼ of an inch radius.

The head radius seems to be very various. It will average 12 inches, perhaps.

Have never made any extended measurements which would throw light on this point, but think that where much side wear takes place the corner generally wears to a greater radius than ¼ of an inch.

I approve the rail section shown (Michigan Central) in all respects, except the vertical sides, and disapprove these because they do not allow as much bearing for the splice as would be available were they inclined.

I disapprove the splice shown as having too much metal in the lower flange and web, with the consequent result of depressing the vertical axis and leading to excessive fiber elongation in the upper side of splice when reacting from load and to consequent rupture. I much prefer the Sayre type of splice as remedying this defect and that of the insufficient bearing for rail head.

I inclose a form of rail section, Fig. 20, which appears to me the best answer I can give to the last circular of the committee, which, however, I answer also categorically.

From my observation the faults of our rail section and joints are as follows:

First.—Too little bearing for the wide treads of the wheels, and consequent grooving or double flanging of wheels and rapid wear of rails.

Second.—Too little bearing for the splice bars on the head of the rail.

Third.—Too narrow a bearing on the tie, and consequent rapid cutting down of the latter.

Fourth.—Too great a bottom flange of the splice bars, leading to a low position of the neutral axis of the bar and to a great separation of the neutral axis of the rail and splice bar.

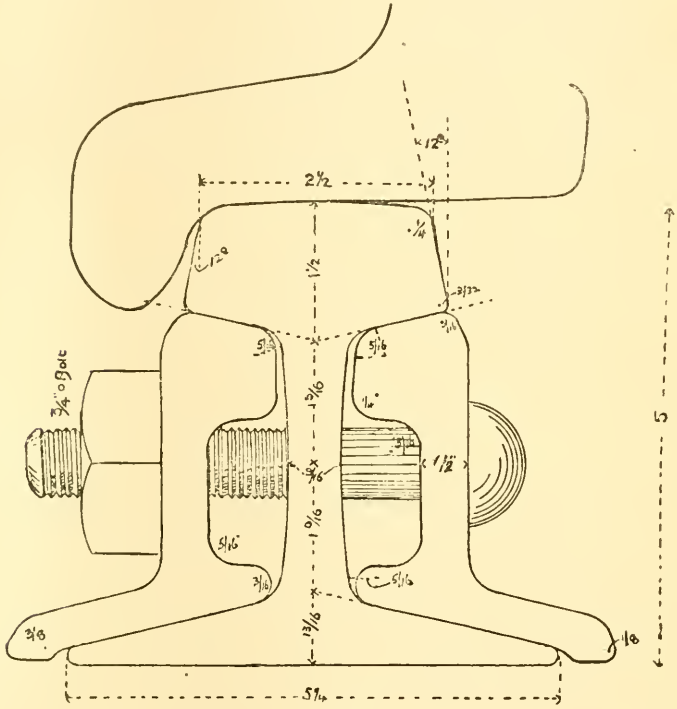
The result of the latter is injurious to the splice, by diminishing its efficiency through its unsymmetrical form.

The effect of the want of coincidence of the neutral axes of rail and splice is somewhat difficult to predicate, but the difference of proportional elongation of the top and bottom members of each under similar stresses cannot but be unfavorable to the member which is always weaker, viz., the splice.

The section I present, it will be observed, is made up of the Michigan Central and Lehigh Valley splices by adopting what I consider the good features of each.

I retain the broad, flat and shallow head of the former while flaring its sides to get bearing, and thinning the base and adding ¼ of an inch to its width.

I think the records will show that few, if any, steel rails have failed from being too shallow in the head or too thin in the base, though some of these rails have been worn down in use far below the point at which the surface of the head becomes too uneven for good riding.


12°
2½
¼
12°
1½
3/32
5/16
5/16
3/16
3/16
3/16
3/16
1 3/16
1 3/16
1

what is necessary to provide for corner curves, where the wear must necessarily be unequal through the oscillation of wheel on rails by difference of gauge of wheels and track. Beyond these corner curves the rail should be of such width as to permit a bearing subject to practicable uniform pressures, and this bearing, I contend, should be straight and not curved, and that such a bearing is entirely practicable whenever rails and wheels are properly constructed.

I shall be prepared to believe that there should be no anxious effort to increase the contact between wheels and rail whenever it can be proven that the sharp edge of a razor can be drawn across one's throat with the same immunity against injury as its back, and not before.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

426.

(Vol. XXI.—November, 1889.)

EXPERIMENTS RELATING TO HYDRAULICS OF FIRE STREAMS.

By JOHN R. FREEMAN, M. Am. Soc. C. E.

WITH DISCUSSION.

Although the hydraulics of fire streams may seem but an unimportant special branch of the science of hydraulics, yet when we consider that in the United States alone there is property to the value of upward of one hundred million dollars destroyed by fire each year,* and that probably twenty-five million dollars per year more is expended in maintaining fire departments, investigation in detail of the scientific principles governing the customary means of delivering water for extinguishing fires appears amply justified. That the skill of the engineer may find a most worthy field of action in the prevention of loss by fire is illustrated in the unparalleled immunity from serious fire loss enjoyed by the great cotton factories of Lowell, where for many years the arrangements for supplying water for extinguishing fire have been under the direct care of the honored Past President of our Society, Mr. James B. Francis.

The experiments to be described were mostly made in the latter part of the year 1888, and it is believed that in completeness and in effort to

* See article by Edward Atkinson, on Slow Burning Construction, *Century Magazine*, February, 1889.

secure a high degree of accuracy they form the most reliable series of experiments on fire streams yet undertaken.

The subjects investigated were :

First.—Co-efficient of discharge of nozzles of various forms.

Second.—Loss of pressure by friction in fire hose with interior surface of various degrees of smoothness.

Third.—Effect of curves in line of fire hose upon loss of pressure.

Fourth.—Effect of reduction of area of waterway at couplings upon loss of pressure in hose.

Fifth.—Height and distance reached by jets of water under various pressures.

Sixth.—Influence of size of jet upon height attained under a given pressure.

Seventh.—Comparative efficiency of various kinds of nozzles.

Eighth.—Distribution of velocity in jets.

Finally, realizing the aversion of the busy, practical man to algebraic formulas and to lengthy computations, the final practical results have been embodied in tables covering the range of sizes and pressures commonly met in practice. Much care has been taken to secure accuracy in these tables, the computations and stereotype proofs having been verified by a second party.

As a sort of preliminary index to the results attained, we may say:

1st. Discharge.—(*a.*) The fire-stream tables heretofore in most general use were found to underestimate discharge of nozzles of a given size under a given pressure by from 15 to 20 per cent. (Page 327.)

(*b.*) Uniformity of co-efficient of discharge for smooth nozzles of different forms, and with the ordinary difference in smoothness of finish by different workmen, found such as to enable measurements of discharge of water under high pressure—as, for example, the delivery of a pumping engine—to be made with great accuracy by means of nozzles; the degree of accuracy thus obtainable being fully as great as by weir measurement. (Pages 330 to 333.)

(*c.*) The very accurate measurement just referred to requires the pressure to be measured at point near base of nozzle or play-pipe, so called, by means of carefully made piezometers, and requires careful calipering of the orifice of discharge. The values for discharge given in tables on pages 422 to 433, may in some cases serve for these accurate measurements, but since the exact size of orifice is likely to differ by a few

thousandths of an inch from those given in tables, or since it may be expedient to attach piezometer upon a waterway of other than $2\frac{1}{2}$ inch diameter, special computation will generally be necessary.

For quick approximate measurements of discharge, the tables given on pages 444 to 447 have been computed from the results of the experiments.

2d. Friction Loss in Hose.—(*a.*) The actual diameter of each piece of hose was measured. It was found that hose of various makes, all nominally $2\frac{1}{2}$ inch, varied from $2\frac{7}{16}$ to $2\frac{3}{4}$ inches in internal diameter. This alone would make about 50 per cent. difference in loss of pressure by friction, and indicates that previous data, based solely on nominal diameter, is not reliable for determining comparative loss in different kinds of hose. (Page 352.)

(*b.*) The character of the internal surface of fire hose was found to affect the loss of pressure by friction to a much greater degree than generally supposed. (Page 347.)

(*c.*) As a result of these experiments, certain leading makers of ordinary rubber lined cotton fire-hose have been induced to slightly modify process of manufacture, and without materially adding to the cost have increased smoothness of interior so as to decrease the friction loss, in certain grades of hose which are much used, by 40 per cent. (Page 347.)

(*d.*) The degree of roughness of interior surface of waterway was determined and recorded by means of plaster casts and photographs with an exactness such as to give information of quantitative value as to co-efficient of roughness in the Kutter or other formulas. (Figure 47.)

(*e.*) Certain of these hose experiments are of interest as data for the study of the flow of water in pipes under very high velocities, and indicate that while loss varies nearly in simple ratio to the square of the mean velocity, yet there is a well defined divergence from this law. (Page 356.)

3d. An Increase in Size of Fire Hose recommended.—(Page 366.) Curves of any radius possible without cramping the hose were found to retard flow but little. The natural ordinary sinuosity of a line of ordinary fire hose was found to increase friction only about 5 per cent., and some rather curious results were obtained, indicating that within certain limits the loss due to a given amount of curvature was greater for a large radius than for a small. (Page 361.)

4th. The loss of pressure for sudden reduction and enlargement of area of water-way (as by use of couplings of small diameter) found

to agree fairly well with the theory that it equals the head corresponding to the difference in velocity due to passing from contracted channel to the larger section. (Page 368.)

5th. JETS. — (*a.*) Previous experiments on jets were very few, and records of these previous experiments gave only the height or distance attained by the extreme scattering drops ; a fact of almost no value to the practical fire engineer. In these experiments much care was taken to also note the extreme limit at which the jet would serve as a practical fire stream. (Page 373.)

6th. JETS.—(*a.*) The Box formula for height of jets found approximately correct for fire streams of ordinary size, but found not to express accurately the variation in height due differences in diameter of jet. Diagrams for practical use in computing height to which jets will rise under a given pressure are presented. (Figures 56 and 57.)

7th. Jets from about forty different nozzles were compared in the endeavor to find the form giving the most perfect jet. (Page 393.)

(*a.*) Smooth nozzle was found to give a jet a little superior to that from a ring nozzle, by comparison so arranged as to give thoroughly reliable results. (Page 406.)

(*b.*) "Uniform acceleration nozzle" was found to give no better jet than conical nozzle. (Page 401.)

(*c.*) A smooth conical nozzle was found to give the best results of any tried. (Page 394.)

8th. Distribution of velocity found to be uniform throughout cross-section of jet from the best nozzles, except in immediate vicinity of walls of orifice. (Page 411.)

9th. Actual velocity of discharge in central portion of jet from both sharp-edged and conical orifices was found to equal the full theoretical velocity, and that co-efficient of mean velocity of discharge is less than unity is found to be due to retardation close to walls of orifice. (See page 414.)

In making tests of the delivery of various pumps, the writer had noted a serious discrepancy between the plunger displacement and the discharge as given by the ordinary Ellis tables for fire streams, and in the case of some three or four large pumps, which were new and apparently in excellent condition (one being an excellent two-million-gallon water-works engine), this difference appeared greater than could with reason be charged to "loss of action" in the pump. Being thus

led to examine the Ellis tables more closely and to compare different authorities, such disagreement was found that recourse to accurate experiment seemed necessary, if we were to have a reliable basis for estimates regarding fire streams.

The writer's interest in the matter thus grew out of a desire for better information upon points connected with his duties as hydraulic engineer to an association of insurance companies.

The expense of the experiments was borne mostly by certain insurance companies, known collectively as the Associated Factory Mutual Insurance Companies of New England (and Pennsylvania). These companies are, in point of fact, associations of leading manufacturers for mutual protection against fire, and the business of these companies is conducted on the theory that their organization is as much for the purpose of preventing or reducing loss by fire, as it is for the purpose of adjusting or distributing the loss when a fire does occur.

The experiments were made at Lawrence, Mass., on the premises of the Washington Mills Company, who very kindly placed at our disposal their excellent new fire pump and hydrant system, comprising fire pumps of a total nominal capacity of three thousand gallons per minute and 16-inch pipes. This hydrant system was also connected with the city water-works by a pipe 16 inches in diameter, fed in turn through a main distribution system, whose pipes are of unusually ample size, and supplied by gravity from a forty-million-gallon reservoir. The Lawrence City Water Board generously co-operated in the experiments by permitting the free use of this supply.

This gravity supply gave at the site of the experiments a hydrant pressure of about 73 pounds per square inch, and in nearly all of the experiments, or in all cases where this pressure was high enough to serve the purpose, this "City Reservoir water" was used because of the superior steadiness of the pressure.

In cases where a higher pressure was required the water was supplied by one of the mill fire pumps; in some experiments a Fales & Jenks' No. 4 rotary fire pump, and in others a No. 1 Holyoke rotary pump being used. These pumps drew Merrimack River water from the main canal, and the "City Reservoir water" was also taken from the Merrimack about 1½ miles up stream.

In these experiments where pump pressure was used, a large air chamber for removing the hammer of the pump was very simply extemporized by first drawing off the water from the fire-pipes within the

adjacent mill, and after these were thus filled with air, then again opening the gates to them from the ground mains.

During none of the experiments was the water turbid or noticeably different from its ordinary normal condition. Analyses made by the chemist of the Massachusetts State Board of Health at various times during the experiments showed water drawn from the same source to contain a residue on evaporation of about four parts per one hundred thousand. The specific gravity of a natural water may be affected by dissolved gases in such a way as to in part neutralize the effect of the solid matter contained, and it is probable that this water differed in weight from distilled water no more than would correspond to $\frac{3}{100}$ pounds per square inch in a column 1 000 feet high.

The units of pounds per square inch for pressures, and of United States gallons of 231 cubic inches per minute for discharges, were adopted, by reason of these being the units most commonly used by fire engineers, pump builders and water-works superintendents, although the writer recognizes that in general it is desirable for the hydraulic engineer to state water pressures in feet head, and discharges in cubic feet per second.

In computing velocities the excellent tables in Francis' "Lowell Hydraulic Experiments" were used.

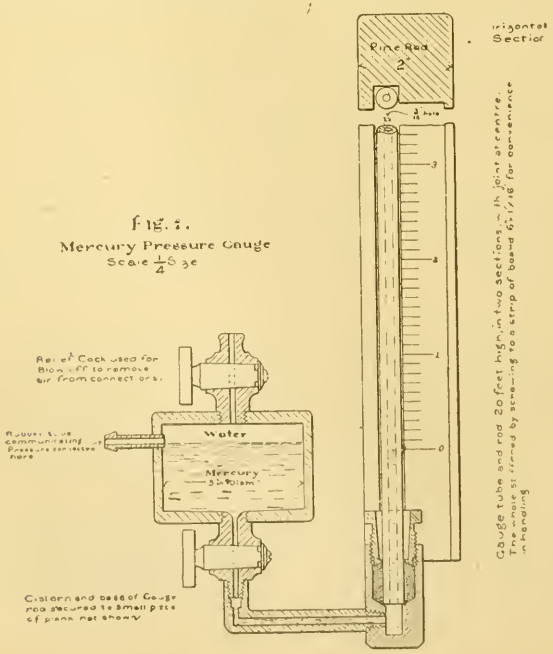


Fig. 1.
Mercury Pressure Gauge
Scale $\frac{1}{4}$ size

Measurements of Water Pressure.

The writer believes, as the result of considerable experience, that the ordinary Bourdon gauge is not by itself a sufficiently accurate and unvarying instrument for first-class scientific work such as requires accuracy within 1 or 2 per cent., by reason of the frequent derangements and change of scale to which even the best Bourdon test gauges are liable. Therefore the standard gauge adopted for measurement of pressures in these experiments was an open mercury column. Two semi-portable gauges of this kind (see Fig. 1) were constructed especially for these experiments, and these mercury gauges were used almost exclusively in all the following experiments on friction loss in hose, discharge of nozzles, comparative efficiency of nozzles, and height of jets.

Two excellent Crosby test gauges and one Ashcroft standard test gauge were at hand, but were used only in some of the minor determinations, and were frequently verified by comparison with the mercury columns and corrected accordingly.

These mercury columns were each 20 feet in height, reading to about 117 pounds per square inch pressure. Their scales were graduated to read directly in pounds per square inch, and to include the correction for lowering of surface of mercury in cistern.

The mercury was procured from Mr. Huddleston, the well known barometer maker, who stated it to be pure. The value 13.563 for specific gravity of mercury at 60 degrees Fahr. was adopted. (Authority, Regnault, P. A., 62–50. See Clark's Constants of Nature.) The weight of a cubic inch of water at 0 degrees C. was taken as 0.03610 pounds, on authority of J. B. Francis, "Lowell Hydraulic Experiments," page 29; and, therefore, by comparison we derive weight of 1 cubic inch of mercury at 60 degrees Fahr. = 0.4896 pounds. Height of mercury column corresponding to 1 pound per square inch = 2.0425 inches. The diameter of glass tube was about $\frac{3}{16}$ inches or about the same as for an ordinary barometer and diameter of cistern 3 inches.

Therefore, to compensate for lowering of mercury surface in cistern, scale of this instrument was graduated with 1 pound per square inch = 2.0343 inches.

Error due to capillarity in the $\frac{3}{16}$ inch tube was avoided by setting scale to read zero when pressure was zero, amount of mercury in cistern having been previously adjusted so that its surface was just at level of entrance into cistern of tube transmitting the pressure. Since there was occasionally a slight loss of mercury while forcing water through tubes and cistern, for making sure of absence of air bubbles in tubes, note was made before and after each set of experiments of reading of mercury column with pressure removed, and mercury was added to bring column up to the zero level if necessary, or a slight correction made to the observed reading.

For the benefit of future experimenters in hydraulics, I would say that these semi-portable mercury columns proved very satisfactory in

their operation. Very little of the difficulty of leaky joints noted by M. Darcy was met with, mainly because we profited by his experience, used a less cumbrous form of apparatus, and avoided joints wherever possible, making glass tube in only two pieces, and joining these by heavy rubber tube containing cloth insertion, tightly wired on. Joints in iron fittings need to be very much closer to hold mercury than to hold water, and as the ordinary taper-screw pipe-joint will be found to give trouble, nothing but an accurate "shoulder fit" should be used. Cisterns were made of wrought-iron with welded joints, and interior of cistern and iron connections was japanned after putting together, to increase tightness and prevent rust.

As stated above, the graduation assumed the diameter of the glass tubes to be $\frac{3}{16}$ inch. These tubes were afterwards accurately calibrated by noting weight of mercury required to fill them to certain heights, and corresponding corrections deduced. For gauge A maximum correction at 100 pounds $= -0.08$ pounds, and for gauge $B + .01$ pounds. This result was exactly confirmed by placing the two gauges side by side, and subjecting both to identical pressures through nearly their whole range.

In a gauge of this construction the lowering of the surface of mercury in cistern lowers the datum of the gauge; but the variation due to this cause for 50 pounds pressure, for instance, would be only 0.02 pounds per square inch, and thus almost inappreciable. Variation of 20 degrees Fahr. in temperature of mercury, from 60 to 80 degrees, would affect indication of gauge only about $\frac{1}{1000}$ part, while it would affect a water column $\frac{2}{1000}$.

All things considered, it would seem reasonable to expect these mercury gauges, including corrections applied, to measure a pressure correctly within one-tenth of 1 per cent.

The datum plane to which all heads and pressures were referred was a level plane passing through center of orifice of discharge of nozzle. It was in the first place established and marked at convenient points near by all gauges and nozzle rest by means of a very accurate engineer's level.

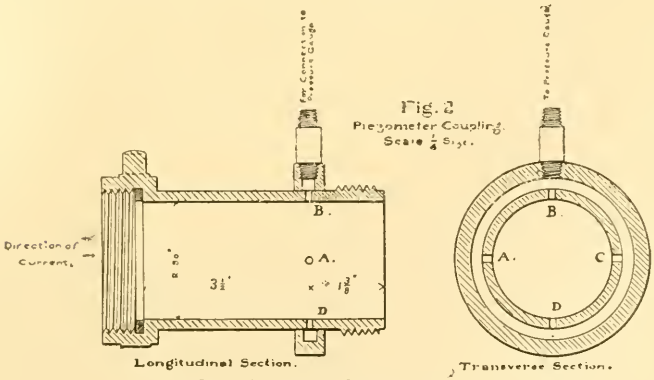
The intention was to set center of nozzle orifice and the zero point of all gauges exactly at this level; but as it was rather difficult to do this with great exactness, the gauges and the nozzles were set to coincide with the datum marks approximately, and then, as a further check intended to lessen errors possible from capillarity, air bubbles, defective graduation or error of measurement, etc., their relative heights were very carefully verified by water leveling, immediately after each series of experiments, before disturbing apparatus. That is, the gauges were compared at various pressures by closing end of hose at nozzle, so there should be no current, and so that the hose thus served meanwhile merely as a free channel of communication between the gauges, which, if correct and set exactly level, should under these circumstances read alike.

The elevation of orifice of nozzle, relatively to zero of gauge connected therewith, was similarly verified by water levels, by connecting an open glass tube about 1 inch diameter (or large enough to avoid sensible error by capillarity) to nozzle by a rubber tube 1 inch in diameter by 1 foot long. Then, after filling the hose from the hydrant, and carefully closing hydrant gate so water would just rise to some certain mark on the glass tube—say, for instance, 1.000 foot above level of center of orifice—the pressure gauge was read at same time. By using several repetitions these comparisons furnished very accurate means for determining, under the exact conditions of the experiments, the slight errors of adjustment of gauges. Corrections as thus determined have been applied in all cases. Great care was taken to avoid errors due to air bubbles in connections.

Although a difference of pressure of .01 pound was revealed by the gauges, and the observations were recorded to hundredths of a pound, yet, owing to possible errors in the gauge itself, such as those mentioned on page 8, and slight errors in determining the level of the gauge relative to datum plane, I consider that any set of readings may, perhaps, be in error by not more than 0.15 pound, except that in some cases, when supply came from the pumps, this limit of error may have been slightly increased by uncertainty due to fluctuations of pressure.

Piezometers.

For measuring the water pressure within the hose at any required point, and for measuring the pressure existing at base of play-pipe, use was made of piezometer couplings of the form shown in Fig. 2.



A.B.C.D = Piezometer Orifices. – Both screw ends fit Standard hose couplings so this may be introduced at any joint in a line of hose or at base of play pipe, coupling thus forming part of the line

The one shown was of an internal diameter of 2.50 inches, and was used in all experiments on 2½-inch hose and nozzles adapted for the same. Three piezometer couplings of this size were provided. Others of 2¼ and 2 inch diameter were provided for use in experiments on hose of these sizes.

This piezometer coupling consisted mainly of a brass tube about 5½ inches long, which when in place formed part of the line of hose. The interior was bored out and finished with much care, and great care was taken that the piezometric orifices in the walls of the tube should be normal to the surface and without any burr at edge.

There were four of these ⅛-inch piezometric orifices, all in the same plane of cross-section, 90 degrees apart, and about 4 inches down from the up-stream end of the tube. All communicated with each other by an exterior channel or chamber encircling the tube, and the pressure gauge was connected to this chamber. This arrangement was devised with the object that if direction of current, by reason of bends or disturbances up stream, happened to impinge a little harder on one side of the conduit than the other, the piezometric reading should not thereby be rendered erroneous, but should indicate the mean pressure existing on the walls in that cross-section. We may remark that neglect of some such precaution may lead to very noticeable error. It may be well to add in this connection that certain anomalous results in various experiments upon flow of water in pipes which have made some observers question the general accuracy of piezometric indications may doubtless be due to surface not being exactly parallel to current or to a projection of the piezometric orifice within the main conduit. Thus, for instance, in the case of hose, if, with a good fire stream flowing, it was attempted to measure pressure through a piece of ⅛-inch gas pipe screwed into a hole drilled in a coupling and projecting ¼ inch inside, the indicated pressure might be from 1 to 2 pounds per square inch in error.*

Measurement of Quantity Discharged.

The number of gallons per minute discharged in all experiments on friction in hose and upon delivery of nozzles was measured in a tight and rigid rectangular tank holding about 1 350 gallons, built of heavy

* In this connection see the very complete experiments upon piezometers by Hiram F. Mills, Proceedings American Academy Arts and Science, 1878.

plank and smoothly lined with sheet zinc, with joints soldered perfectly tight. Its area was determined by the average of lengths and widths measured at about twenty different points, and the height of water in it, immediately before and after experiment, was determined by the glass tube of about 1 inch inside diameter (by this large diameter avoiding errors due to capillary attraction), to which was attached a scale very accurately graduated to hundredths of a foot, and read by estimation to $\frac{1}{1000}$ foot.

With the possible sources of error in view, I think that in general the error in any single measurement of quantity did not exceed one-third of 1 per cent. This tank is shown in Fig. 3.

The discharging nozzle was only 3½ feet distant from this tank. It pointed upward at an angle of 30 degrees, thus simulating natural conditions; and in the intervals between the experiments nozzle was deflected slightly to one side and the jet wasted and fell into the river south of the tank. When all was ready for the experiment, by deflecting the nozzle and hose pipe back into line, the water from it was caught in a hood which deflected it all downward into the tank. Thus to get the number of gallons discharge per minute the water was first allowed to get into a settled condition of steady flow in the hose, then by a slight motion of the hose pipe the jet was deflected into the tank, and when the tank was nearly filled was deflected out again, the time of fill being taken by a stop watch read to tenths of seconds. The error in measurement of time of filling could not, I think, often exceed one-fifth of 1 per cent., and generally was less.

Thus I think that in general we may rely upon a single measurement of the number of gallons per minute discharged by the nozzle as correct within one-half of 1 per cent., and that the average results of a series would be much closer.

Co-efficient of Discharge of Nozzles.

In many experiments the friction loss in a given sample of hose due to a certain rate of flow, and also the co-efficient of discharge of a given nozzle attached to this hose due to a certain pressure, were determined simultaneously. In such cases the mercury gauge, Fig. 3, served to indicate the pressure on the play-pipe actuating the discharge, and at the same time served also to show the pressure at the down-stream end of the line of hose.

In other experiments the sole attention was given to determining the pressure existing at base of a given play-pipe and the corresponding number of gallons per minute discharged; but in all experiments on discharge the nozzle and play-pipe were connected to a line of hose, and, as nearly as possible, under the practical conditions of use. It will be noted from Fig. 3 that for a distance of 3 feet up stream from play-pipe piezometer, hose was laid straight.

One observer observed and recorded the reading of mercury pressure gauge each half minute. Thus the value obtained for each experiment was the mean of from six to twelve independent observations and the error from slight variations of hydrant pressure was reduced to a minimum. Such fluctuations were small. When water from city reservoir was used, as was the case in all experiments except those with very high pressures, the general range of fluctuation in the course of an experiment did not exceed $\frac{1}{2}$ or $\frac{3}{4}$ pound, and between successive observations did not exceed from 0.1 to 0.2 pound. In those experiments under high pressures, when supply was furnished by a large rotary fire pump, the fluctuation between successive observations of pressure gauge at nozzle was often one-half pound.

Taking all the known possible sources of error into consideration, I think we may feel confident that the head acting on centre of orifice of discharge is determined within 0.20 pound for pressures from reservoir, and within 0.50 pound for pressure from pump, and that the averages of series of experiments are closer. An examination of the plotted results or of tables 1 to 4 shows the agreement of the different experiments to be well inside the limits of error just stated.

The orifices were all calipered by the writer with much care with fine micrometer calipers, and except in one or two instances where the orifice was apparently not quite cylindrical it is believed that the recorded figure is correct within $\frac{1}{1000}$ inch. In the undercut ring nozzles it was difficult to determine the diameter at the exact point governing the size of stream, by reason of the appreciable dullness of the edge; but these are believed to be not more than $\frac{5}{1000}$ inch in error, and the square ring nozzles not more than $\frac{3}{1000}$.

To sum the whole matter up, we may have confidence that in general our determinations of the co-efficient of discharge of nozzles were certainly correct within one-half of 1 per cent., and that probably the mean of the values found for a given nozzle was correct within a somewhat smaller limit.

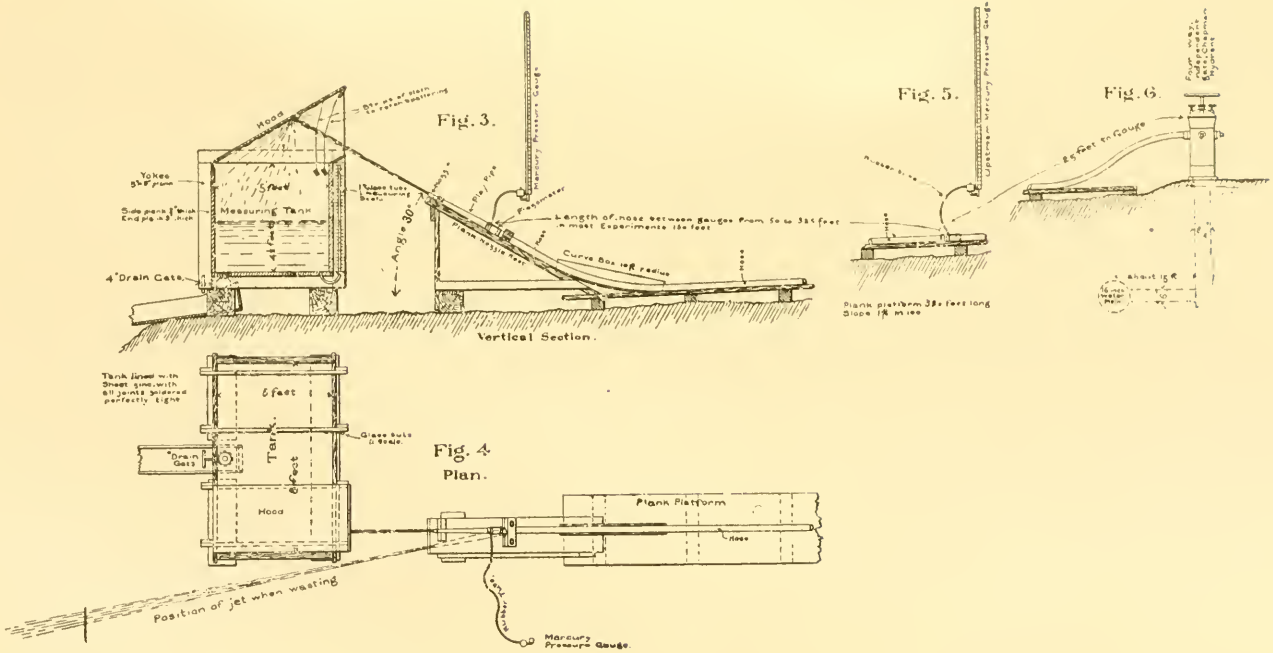


Fig. 3.
Hood
Yokes
Measuring Tank
4" Drain Gate
Angle 30°
Length of nose between Gauges From 50 to 325 feet
in most Experiments 180 feet
Curve Box 18 R radius
Vertical Section.
Fig. 5.
Fig. 6.
Fig. 4
Plan.
Tank lined with
Sheet iron with
all joints soldered
perfectly tight
6 feet
Tank
6 feet
Hood
Glass tube
Plank Platform
Position of jet when wasting
Mercury
Pressure Gauge.

The temperatures of water as given in the table are probably correct within one or two degrees; but temperature of air was observed with less care, and as at times one part of the apparatus was in the sun while another was in the shade, this is perhaps uncertain to the extent of 5 degrees.

The following tables, taken in connection with the drawings, explain themselves.

Table No. 1 embraces the ordinary range of sizes found in the market, and these nozzles were of the type of which it is believed more are now actually used than any other single style. All the subsequent experiments on jets indicated that this style (with corner at end of cone slightly rounded as in Fig. 16) equaled in efficiency any form of nozzle tried. In all the nozzles of Table No. 1 the corner at end of cone was left full and sharp, except so far as slightly rounded by the final finishing with emery cloth.

By reference to the drawings it will be noted that there was with each nozzle a slight enlargement of waterway at end of play-pipe within butt of nozzle; it being the practice of manufacturers to give a slight excess of size here, so as to be on the safe side in avoiding the projection of a sharp corner into stream as in Fig. 22.

From nozzle D (Fig. 15), Table No. 2, it will be seen that this slight recess or enlargement exerted no appreciable influence on the discharge, and in other experiments made on distance and character of jets, as shown in Fig. 55, it was found impossible to detect any difference in appearance of stream whether the recess existed or was filled by the thin hollow cone, as in Fig. 15. On the other hand, it was found by similar experiments that if at base of nozzle there was a slight projection into the stream, as shown in Fig. 22, nozzle K, not only was discharge slightly diminished (as shown in Table No. 2), but there was a very noticeable "tearing to pieces" of the jet.

The co-efficient of discharge of a nozzle throughout these experiments has for practical convenience been considered as that due to the play-pipe and nozzle combined, a taper play-pipe being really a part of the nozzle.

The reason for the slightly larger co-efficient of the $\frac{3}{4}$ and $\frac{5}{8}$-inch nozzles is due to the relative decrease of friction loss within the play-pipe consequent on area of waterway through play-pipe bearing a larger

TABLE No. 1.

Co-efficient of Discharge of Smooth-cone Nozzles of Various Sizes.

Reference Mark of Nozzles. See Figs.	Nominal Size, inches.	Actual Diameter of Orifice, inches.	Observed Pressure at Base of Play-pipe lbs. per sq. inch.	Actual Discharge, gallons per minute.	No. of Obs. on Curve near Point.	Corr. for Head due Vel. past Piez., lbs.	Static Pressure, pounds.	Theoretical Velocity due Pressure, feet per second.	Theoretical Discharge, gallons per minute.	Co-efficient of Discharge.	Mean Value Co-efficient.
(a)	$\frac{3}{4}$	.751	60.0	129.3	1	.46	60.46	94.83	130.9	.988	.983
			50.0	117.2	1	.38	50.38	86.56	119.5	.981	
			40.0	104.4	1	.31	40.31	77.43	106.9	.977	
			25.0	83.4	1	.20	25.20	61.23	84.5	.987	
(b)	$\frac{7}{8}$	.880	80.0	205.2	2	1.21	81.21	109.90	208.3	.985	.982
			60.0	177.0	3	.91	60.91	95.18	180.4	.981	
			50.0	162.0	1	.74	50.74	86.87	164.7	.984	
			30.0	125.0	1	.44	30.44	67.29	127.6	.980	
(c)	1	1.001	50.0	208.5	1	1.26	51.26	87.31	214.1	.974	.972
			40.0	186.4	1	1.00	41.00	78.09	191.6	.973	
			30.0	161.0	1	.74	30.74	67.62	165.9	.970	
(A)	$1\frac{1}{8}$	1.126	50.0	266.5	5	2.04	52.04	87.97	273.0	.976	.976
			40.0	238.2	4	1.64	41.64	78.70	244.3	.975	
			30.0	205.9	4	1.22	31.22	68.15	211.5	.973	
			20.0	169.0	2	.82	20.82	55.65	172.7	.979	
(e)	$1\frac{1}{4}$	1.250	54.0	344.2	2	3.40	57.40	92.40	353.4	.974	.971
			50.0	330.2	1	3.18	53.18	88.93	340.2	.971	
			40.0	295.5	2	2.50	42.50	79.51	304.1	.972	
			30.0	256.0	5	1.86	31.86	68.84	263.3	.972	
			25.0	233.4	4	1.56	26.56	62.84	240.4	.971	
			15.0	180.4	4	.94	15.94	48.69	186.2	.968	
(f)	$1\frac{3}{8}$	1.375	30.0	310.6	6	2.80	32.80	69.84	323.2	.961	.959*
			25.0	283.0	4	2.28	27.28	63.70	294.8	.960	
			20.0	252.0	2	1.82	21.82	56.96	263.6	.956	
(g)	play-pipe with noz. off	1.550	20.0	338.5	1	3.31	23.31	58.87	346.2	.978	.976
			12.0	261.0	2	1.95	13.95	45.55	267.9	.974	

Remarks.

Average temperature of water, 71 degrees Fahr. (range of temperature water, 69 degrees to 73 degrees); temperature of air from 60 degrees to 90 degrees. Nozzles (A) and (a) to (g), inclusive, were each attached to the 30-inch play pipe, shown in Fig. 13, when tested, and pressure gauge was connected to piezometer at base of play-pipe, as shown in the same figure.

This play-pipe was made from brazed copper, taper tubing, and was a good ordinary play-pipe taken from regular stock of A. J. Morse & Son, fairly smooth inside, but contained the ordinary roughness incident to a brazed seam, and was not specially polished.

The nozzles were all of brass, and were the ordinary smooth-cone nozzle, with Amoskeag thread, and were taken at random from regular stock of A. J. Morse & Son. The smoothness of interior was only the ordinary polish of a first-class brass finisher, the tool marks merely being polished out by emery cloth. The "straight part" was bored out to size by a reamer, and corner at junction left sharp, except as rounded very slightly by the emery cloth. These nozzles are shown in Figs. 8 to 13.

These experiments were made at various times between August 22d and October 15th. A large number of observations on these nozzles (a) to (g) were obtained incidentally to the experiments on friction loss in the various samples of hose given in Table No. 5. These were plotted on cross-section paper, on scale 1 pound = 1 cm., and 1 gallon per m. = 2 mm., and the values in the fourth and fifth columns are taken from the curve drawn through the plotted observations.

In Tables Nos. 2, 3 and 4 the quantities given are in each case the result of only a single experiment.

* In experiments on (f) the throat of play-pipe was really too small for a nozzle of this size, hence low co-efficient.

Ordinary Smooth Cone Nozzles.

Scale $\frac{1}{4}$ actual size

Each of these was tested on play-pipe shown in Fig. 13 and Coeff. includes effect of friction loss in play-pipe which is relatively less for the smaller nozzles

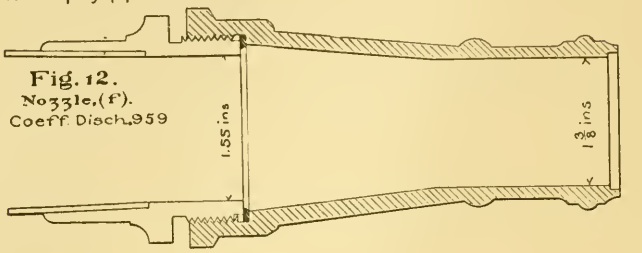


Fig. 12.
Nozzle, (f).
Coeff. Disch. .959

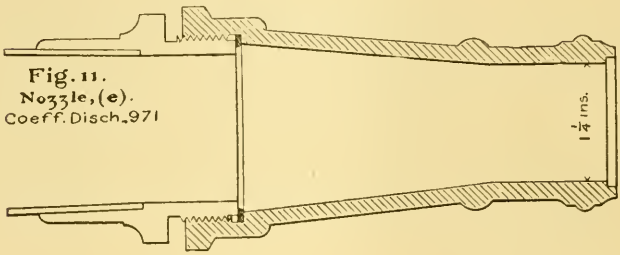


Fig. 11.
Nozzle, (e).
Coeff. Disch. .971

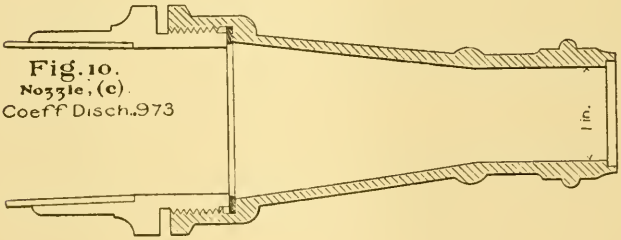


Fig. 10.
Nozzle, (c).
Coeff. Disch. .973

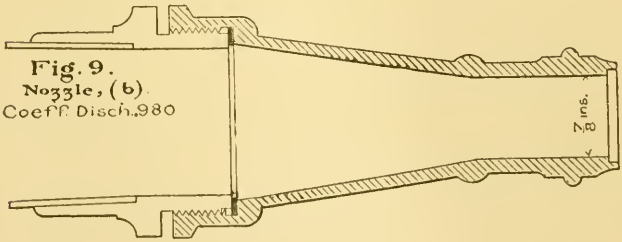


Fig. 9.
Nozzle, (b).
Coeff. Disch. .980

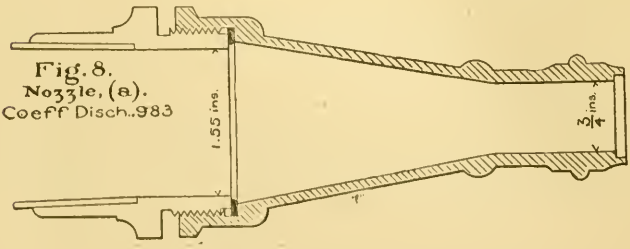


Fig. 8.
Nozzle, (a).
Coeff. Disch. .983

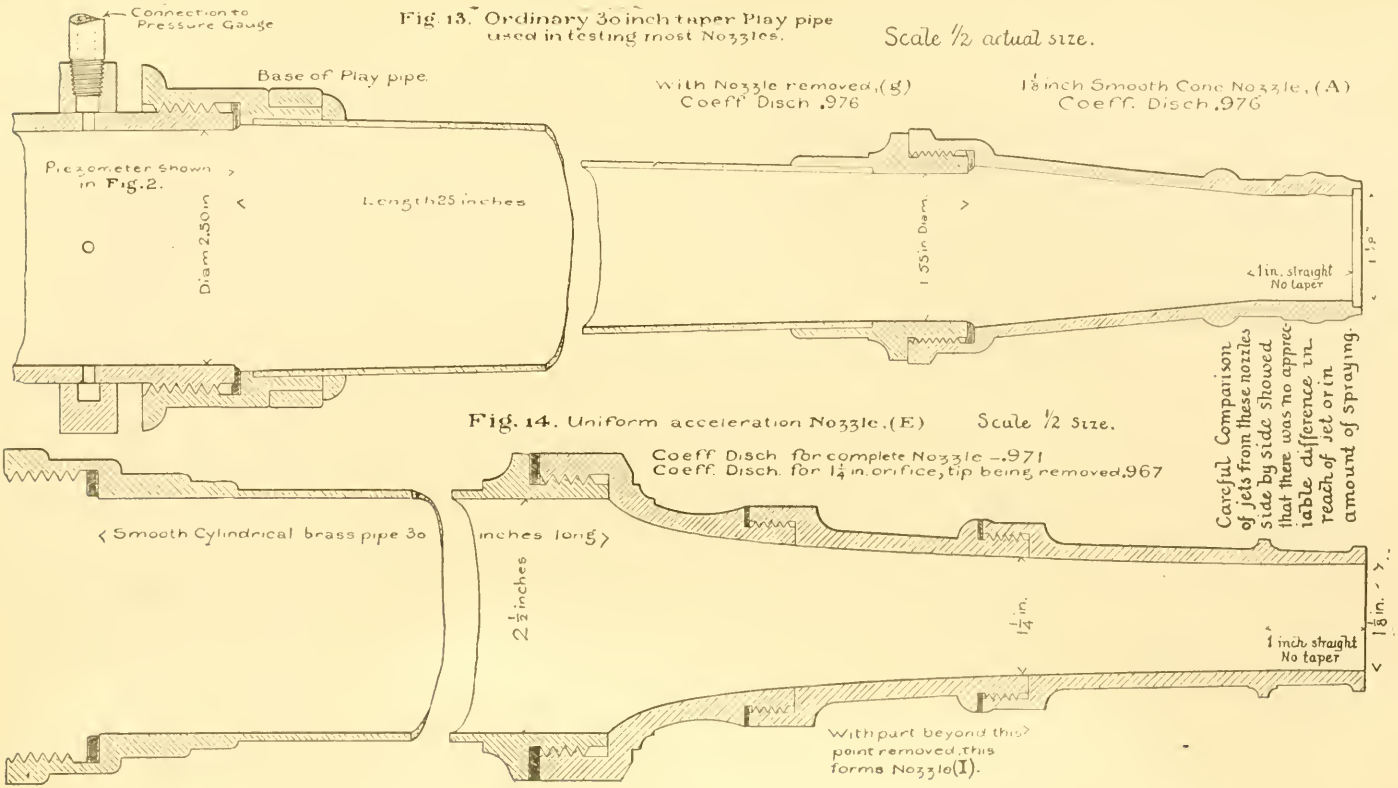


Fig. 13. Ordinary 30 inch taper Play pipe used in testing most Nozzles. Scale 1/2 actual size.

Fig. 14. Uniform acceleration Nozzle, (E) Scale 1/2 size.

Scale 1/2 size.

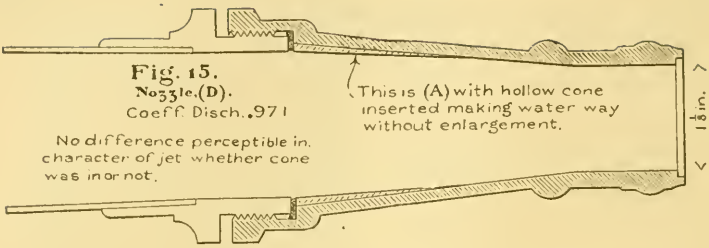


Fig. 15.
Nozzle. (D).
Coeff. Disch. .971

This is (A) with hollow cone inserted making water way without enlargement.

No difference perceptible in character of jet whether cone was in or not.

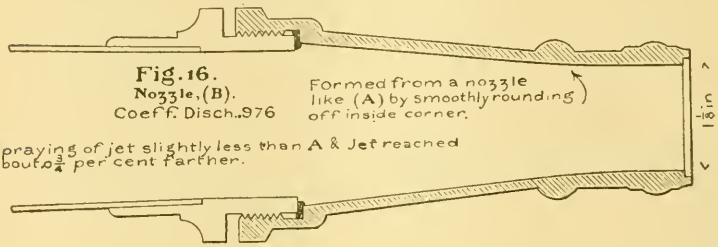


Fig. 16.
Nozzle. (B).
Coeff. Disch. .976

Formed from a nozzle like (A) by smoothly rounding off inside corner.

Spraying of jet slightly less than A & Jet reached about 0 3/4 per cent farther.

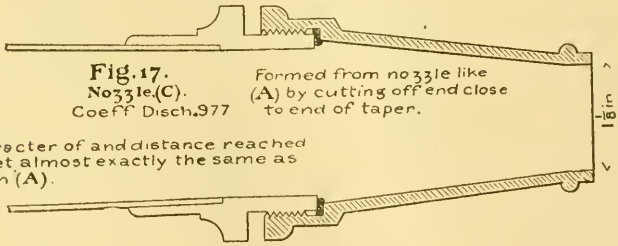
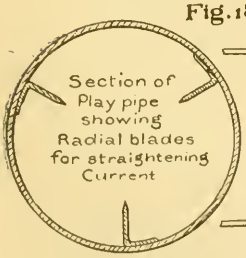


Fig. 17.
Nozzle. (C).
Coeff. Disch. .977

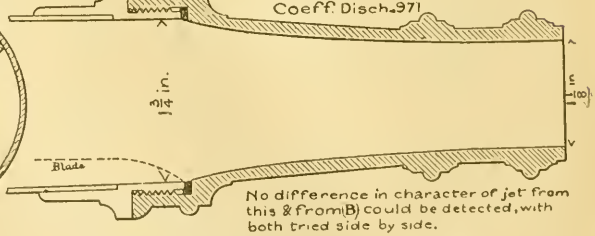
Formed from nozzle like (A) by cutting off end close to end of taper.

Character of and distance reached by jet almost exactly the same as from (A).

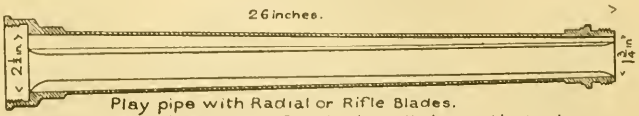
Fig. 18.



Nozzle. (F).
Small Uniform Acceleration.
Coeff. Disch. .971



No difference in character of jet from this & from (B) could be detected, with both tried side by side.



Play pipe with Radial or Rifle Blades.
The use of these blades found to be of little practical value

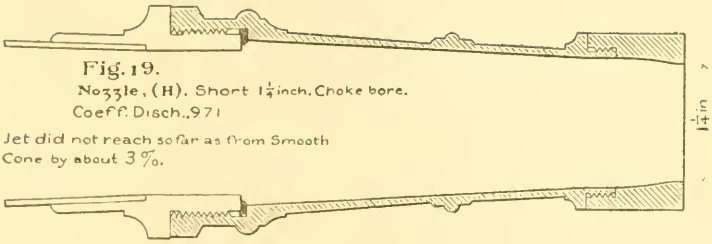


Fig. 19.
Nozzle, (H). Short 1¼ inch. Choke bore.
Coeff. Disch., 971
Jet did not reach so far as from Smooth Cone by about 3 %.

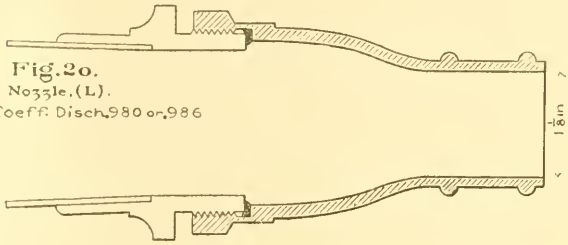


Fig. 20.
Nozzle, (L).
Coeff. Disch. 980 or .986

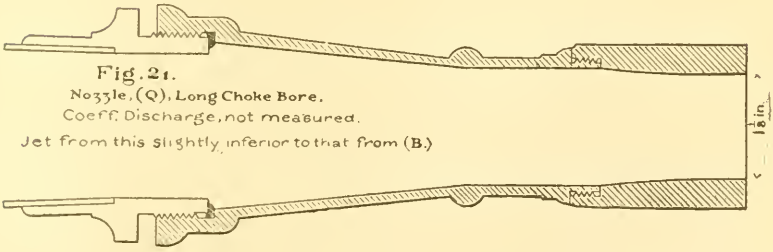


Fig. 21.
Nozzle, (Q), Long Choke Bore.
Coeff. Discharge, not measured.
Jet from this slightly inferior to that from (B.)

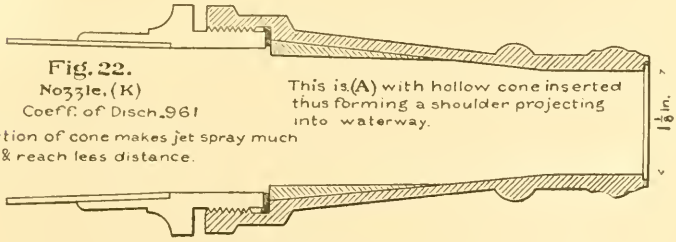


Fig. 22.
Nozzle, (K)
Coeff. of Disch. 961
This is (A) with hollow cone inserted thus forming a shoulder projecting into waterway.
Insertion of cone makes jet spray much more & reach less distance.

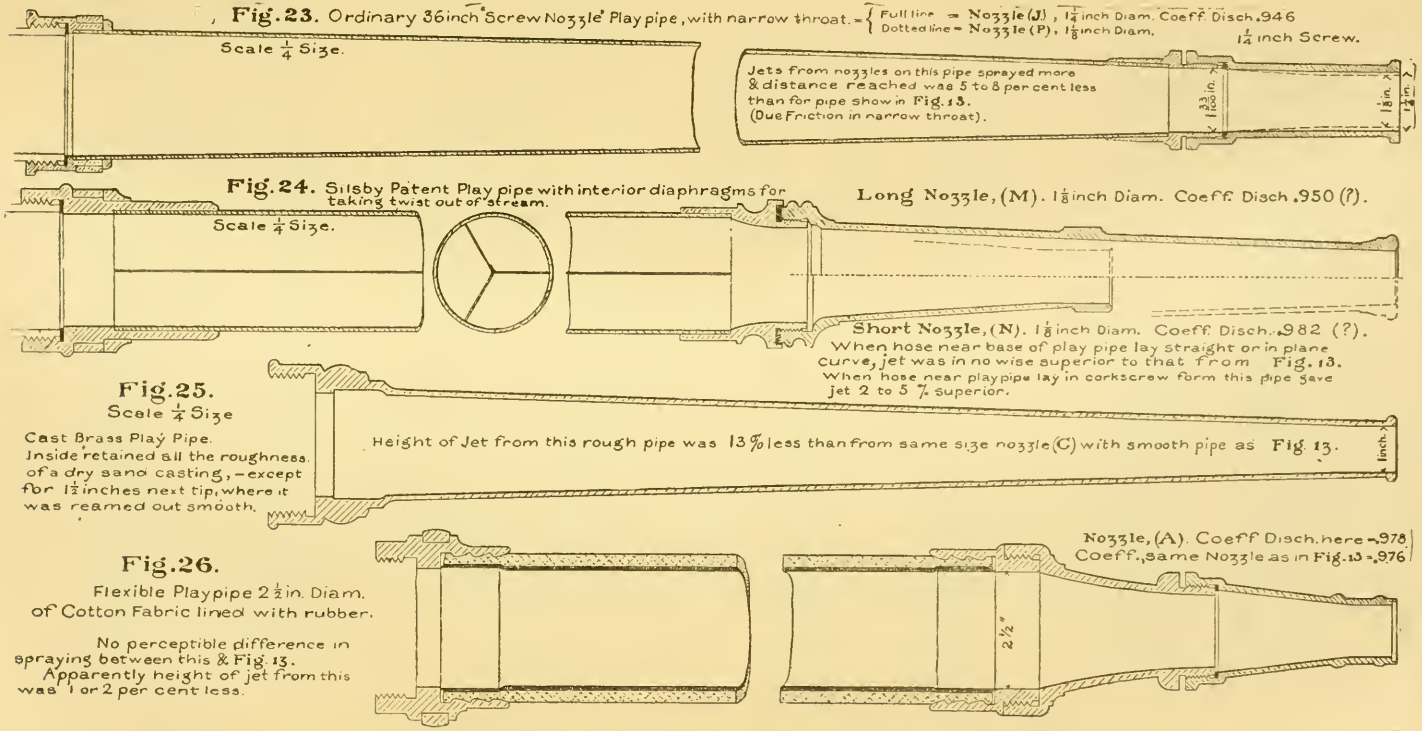


Fig. 23. Ordinary 36 inch Screw Nozzle Play pipe, with narrow throat. = { Full line = Nozzle (J), 1¼ inch Diam. Coeff. Disch. .946
Dotted line = Nozzle (P), 1⅛ inch Diam.
1¼ inch Screw.
Scale ¼ Size.
Jets from nozzles on this pipe sprayed more & distance reached was 5 to 8 per cent less than for pipe show in Fig. 13. (Due Friction in narrow throat).
1 23/32 in.
1⅛ in.
1¼ in.
Fig. 24. Silsby Patent Play pipe with interior diaphragms for taking twist out of stream.
Scale ¼ Size.
Long Nozzle, (M). 1⅛ inch Diam. Coeff. Disch. .950 (?).
Short Nozzle, (N). 1⅛ inch Diam. Coeff. Disch. .982 (?).
When hose near base of play pipe lay straight or in plane curve, jet was in no wise superior to that from Fig. 13.
When hose near playpipe lay in corkscrew form this pipe gave jet 2 to 5 % superior.
Fig. 25.
Scale ¼ Size
Cast Brass Play Pipe.
Inside retained all the roughness of a dry sand casting, – except for 1½ inches next tip, where it was reamed out smooth.
Height of Jet from this rough pipe was 13 % less than from same size nozzle (C) with smooth pipe as Fig. 13.
Fig. 26.
Flexible Playpipe 2½ in. Diam. of Cotton Fabric lined with rubber.
No perceptible difference in spraying between this & Fig. 13.
Apparently height of jet from this was 1 or 2 per cent less.
Nozzle, (A). Coeff. Disch. here = .978
Coeff., same Nozzle as in Fig. 13 = .976
2½"

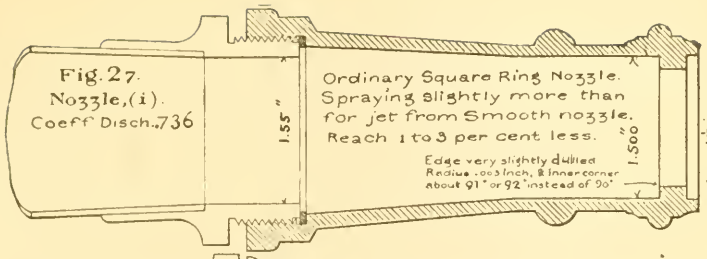


Fig. 27.
Nozzle, (i).
Coeff. Disch. .736
1.55"
Ordinary Square Ring Nozzle.
Spraying slightly more than
for jet from Smooth nozzle.
Reach 1 to 3 per cent less.
Edge very slightly dulled
Radius .003 inch, & inner corner
About 91° or 92° instead of 90°
1.500"
1 1/4 in.

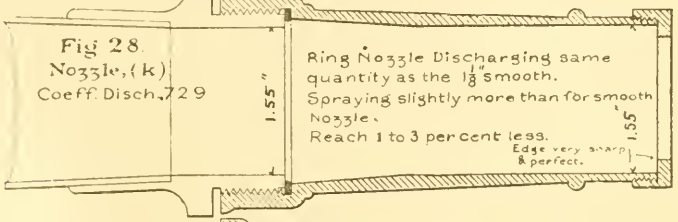


Fig 28.
Nozzle, (k)
Coeff. Disch. .729
1.55"
Ring Nozzle Discharging same
quantity as the 1 1/8" smooth.
Spraying slightly more than for smooth
Nozzle.
Reach 1 to 3 per cent less.
Edge very sharp
& perfect.
1.55"
1.314 in.

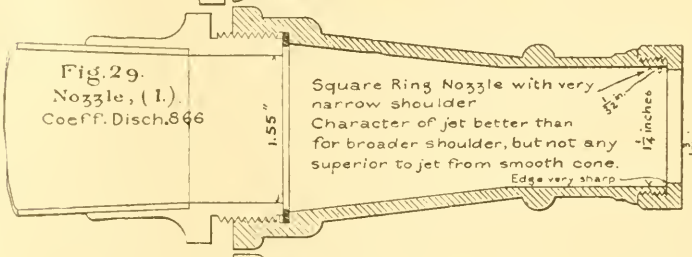


Fig. 29.
Nozzle, (l.)
Coeff. Disch. .866
1.55"
Square Ring Nozzle with very
narrow shoulder
Character of jet better than
for broader shoulder, but not any
superior to jet from smooth cone.
Edge very sharp
1/4 inches
1 1/16 in.

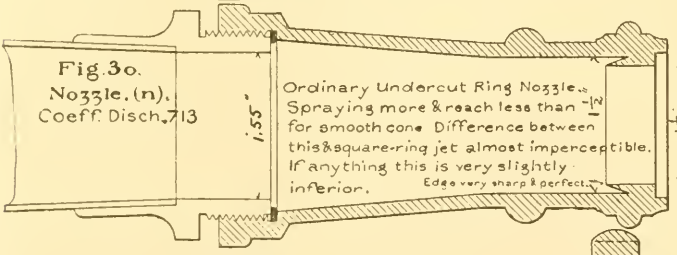


Fig. 30.
Nozzle. (n).
Coeff. Disch. .713
1.55"
Ordinary Undercut Ring Nozzle.
Spraying more & reach less than
for smooth cone. Difference between
this & square-ring jet almost imperceptible.
If anything this is very slightly
inferior.
Edge very sharp & perfect.
1/2
1 1/4 in.

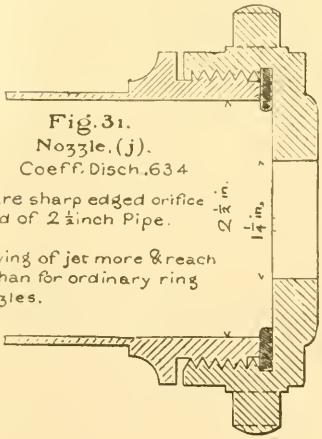


Fig. 31.
Nozzle. (j).
Coeff. Disch. .634
Square sharp edged orifice
on end of 2 1/2 inch Pipe.
(Spraying of jet more & reach
less than for ordinary ring
Nozzles.
2 1/2 in.
1 1/4 in.

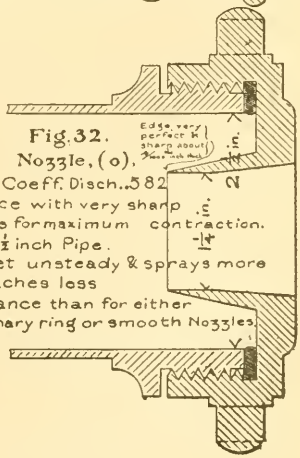


Fig. 32.
Nozzle, (o).
Coeff. Disch. .582
Edge very
perfect &
sharp about
1/32 inch thick
.5
1/4
Orifice with very sharp
edges for maximum contraction.
on 2 1/2 inch Pipe.
Jet unsteady & sprays more
& reaches less
distance than for either
ordinary ring or smooth Nozzles.

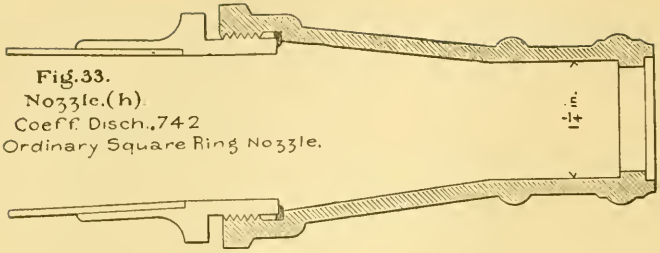


Fig. 33.
Nozzle. (h).
Coeff. Disch., 742
Ordinary Square Ring Nozzle.

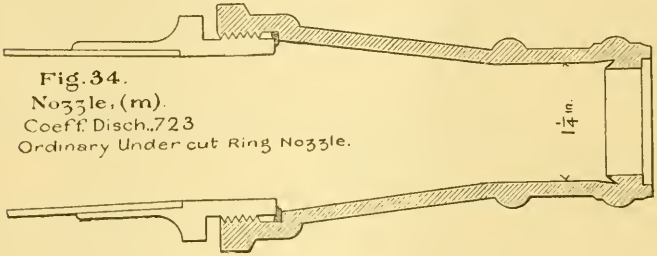


Fig. 34.
Nozzle, (m).
Coeff. Disch., 723
Ordinary Under cut Ring Nozzle.

Scale 1/2 size.

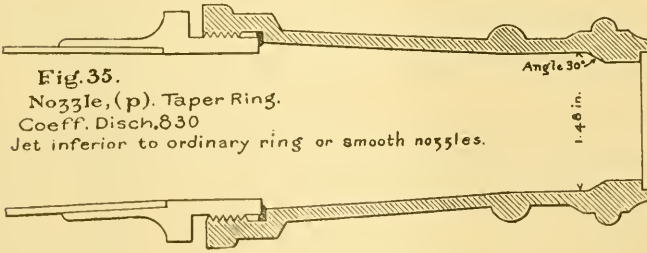


Fig. 35.
Nozzle, (p). Taper Ring.
Coeff. Disch. 830
Jet inferior to ordinary ring or smooth nozzles.

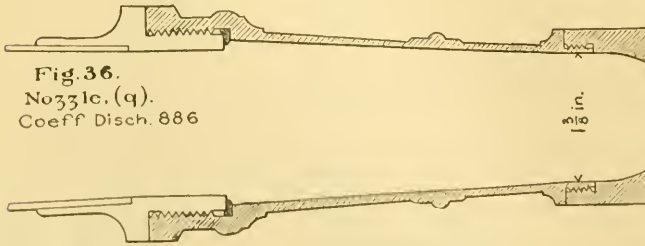


Fig. 36.
Nozzle, (q).
Coeff Disch. 886

Nozzles for Siamese.

Scale ¼ Size.

Fig. 37.
Nozzle, (S).

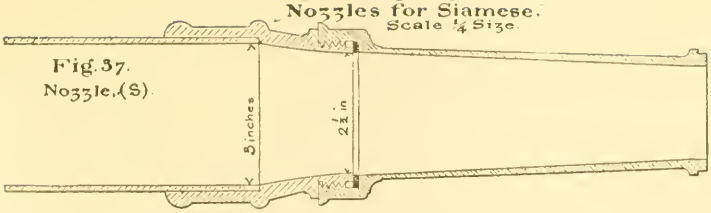


Fig. 38.
Nozzle, (u).

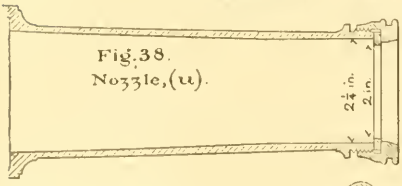


Fig. 39.
Nozzle, (x.)

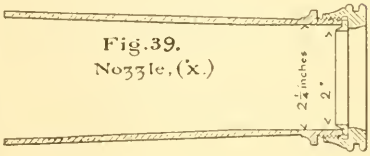


Fig. 40.
Nozzle, (v).

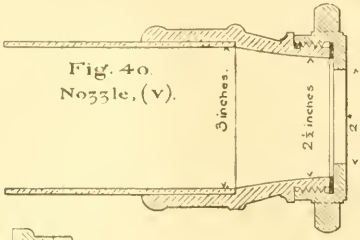


Fig. 41.
Nozzle, (w)

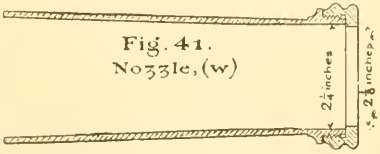
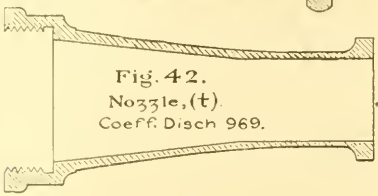


Fig. 42.
Nozzle, (t).
Coeff. Disch 969.



The smooth nozzles (S), & (t) are believed to have proved most satisfactory on the whole, but the large ring nozzles (u), (x), & (w), gave results equally good within 1% in distance reached

Transverse Section of Siamese Pipe.

Showing blades ⅛ inch thick for straightening Current. These extended whole length of Barrel

Fig. 45.

This Style gave best results

In pipe with Siamese these blades found to greatly improve jet & decrease spraying 25%

Piezometer connected here

Fig. 43.
Horizontal Section of Siamese.

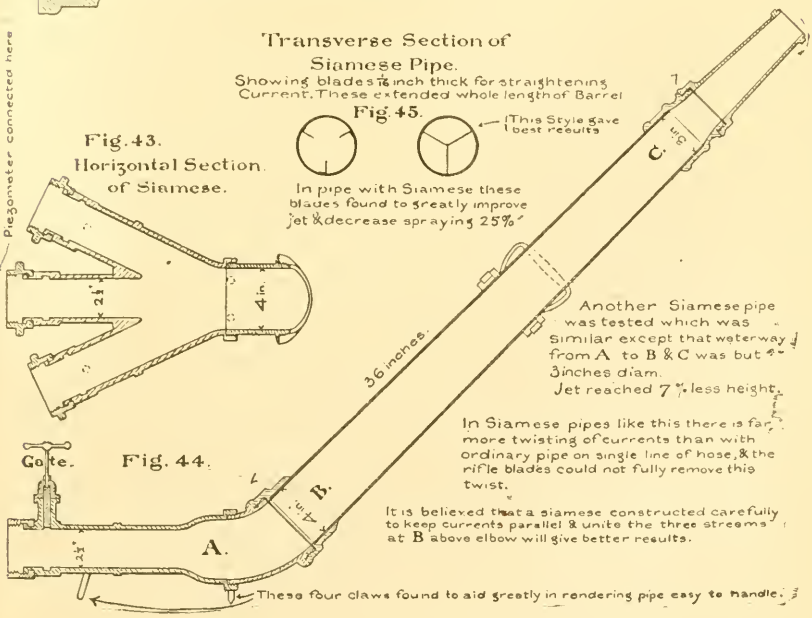


Fig. 44.

Another Siamese pipe was tested which was similar except that waterway from A to B & C was but 3 inches diam. Jet reached 7% less height.

In Siamese pipes like this there is far more twisting of currents than with ordinary pipe on single line of hose, & the rifle blades could not fully remove this twist.

It is believed that a siamese constructed carefully to keep currents parallel & unite the three streams at B above elbow will give better results.

These four claws found to aid greatly in rendering pipe easy to handle.

ratio to area of nozzle, and the smaller co-efficient of the $1\frac{3}{8}$-inch nozzle is due to the relatively small size of throat, or small end, of play-pipe.

Table No. 2 and the drawings on pages 318 to 322 are of interest, first, as showing that the co-efficient of discharge of nozzles is not very sensitive to peculiarities of form or finish of nozzle, but that nozzles of same general style made at different times and places may be relied upon to give for a given pressure and area of outlet discharges which are identical within the practicable limits of measurement; and the table is of interest secondly, as showing by agreement of independent observations the accuracy of the means adopted in making the experiments.

A comparison with co-efficients of discharge found by other experimenters is interesting. The most accurate set of previous experiments of which the writer has knowledge are those of Edmund B. Weston, M. Am. Soc. C. E., made at Providence in 1877, and reported in the Transactions of the American Society of Civil Engineers, November, 1884. The description there given merely says "the quantity of water discharged was measured," but gives no means of judging of the precision with which this was done. His pressures were measured only by good Bourdon gauges. His piezometer coupling was attached directly to base of play-pipe, but possessed only a single opening for communicating pressure to gauge, and indication may thus have been slightly influenced by any obliqueness of current due to bends in hose, etc. The degree of precision with which diameter was measured is not stated. As the mean of some thirty-five experiments in all, under heads ranging from about 17 to 57 pounds per square inch and averaging about 30 pounds, Mr. Weston obtained the following values for the co-efficient of discharge (see his diagram No. 5):

$\frac{7}{8}$ nozzle	.974
1 "	.963
$1\frac{1}{8}$ "	.975
Mean value	.971

These were cone nozzles somewhat similar to those shown in Figs. 7 and 13.

It is thus seen that although the conditions for accuracy were less favorable than in the experiments now presented, they agree remarkably well.

Some experiments of Hamilton Smith, Jr., M. Am. Soc. C. E., on discharge under extremely high heads, may be of interest in this connection. (See Hamilton Smith, Jr.'s "Hydraulics," New York, Wiley & Sons, 1886, pages 285 to 287, also page 207.)

His nozzles were such as are used in connection with "hurdy-gurdy" water wheels, and the part corresponding to play-pipe was a tapering pipe (length not stated) about 7 inches in diameter at butt and 4 inches at beginning of nozzle proper. Thus since co-efficient includes friction loss within this "play-pipe" pipe in both cases, Mr. Smith's co-efficient of discharge might naturally be very slightly larger than the writer's, by reason of this larger play-pipe, and the slight divergence or small increase of diameter near discharging orifice of most of his smooth nozzles would, though probably without effect, if anything, make values a very little larger.

For smooth conical nozzles of cast-iron, about 1 inch and 1¼ inch diameter, with interior surface very smoothly finished, length of nozzle being about 12 inches and converging from about 4 inches diameter at its base, and diverging very slightly at the end, he obtained an average value for co-efficient of discharge of 1.005, and for his nozzle C^1, in which the slightly divergent part next orifice was cut off, the convergence of interior of about 8½ degrees thus continuing to the very end, he found co-efficient to be 1.006, under a head of about 145 pounds per square inch.

He states, however, that by reason of imperfect calipering of orifices, and possible error incident to measurement of discharge, his values of co-efficient of discharge "may be in error as much as 3 or possibly 4 per cent." They thus are in general agreement with the results obtained by the writer, and are of interest since the heads under which Mr. Smith experimented were exceptionally great.

The tables giving discharge of nozzles corresponding to various pressures which are in general use, and which, copied and condensed into various shapes, are found in the hand-books of most pump makers and in various treatises on hydraulics and water supply, are those by George A. Ellis, C. E., formerly City Engineer and Water Registrar of Springfield, Mass.

Prior to the publication of these tables by Mr. Ellis there was no information relating specially to hose streams available in form convenient for practical use. The remarkably convenient form in which Mr. Ellis presented his tables has made them very popular; but the experiments on which they were based seemed to have been defective, from causes which do not appear, as the value adopted for co-efficient of discharge was 82 per cent.,* a result at variance with experiments of Weston, Smith and the writer, and also at variance with the values which the

* See G. A. Ellis' "Fire Streams," published by Clark W. Bryan, Springfield, 1878, page 20.

TABLE No. 2.

Co-efficient of Discharge of Smooth Nozzles of Similar Size but Different Form.

Mark.	Date of Experiments.	Time. H.	Time. M.	Approximate average Temperature of Air, Fahr.	Temperature Water, Fahr.	Nominal Size.	Kind of Nozzle.	Actual Diameter of Orifice, inches.	Observed Pressure at Base of Play-Pipe, pounds.	Actual Discharge, gallons per minute.	Corr. for Head due Vel past Piez., lbs.	Static Pressure, pounds.	Theoretical Velocity due Pressure, feet per second.	Theoretical Discharge, gallons per minute.	Co-efficient of Discharge.	Mean Value Co-efficient.
(A)	.....	.....		.....	71 deg.	1⅛ inches.	Ordinary Smooth Cone..	1.126	.....	.....	.....	.....	.....	.....	.....	.976

Above nozzle being ordinary form, mean value of co-efficient is brought over from Table No. 1, for comparison. In this test, nozzle was on conical pipe shown in Fig. 13.

The following is for the identical nozzle A, but tested on B. W. H. Flexible pipe shown in Fig. 26.

(A)	Sept. 27, A.M.	10	41	65 deg.	62	1⅛	Same Nozzle.....	1.126	39.97	238.9	1.63	41.60	78.66	244.0	.979	.978
	"	10	33	65	62	1⅛	" "	1.126	30.02	207.0	1.26	31.28	68.20	211.7	.978	

The following nozzle was of precisely same form as A, except that corner at junction of small end of cone and straight part was rounded off. See Fig. 16.

(B)	Sept. 25, P.M.	5	48	55	63	1⅛	Smooth Cone.....	1.126	39.97	238.5	1.63	41.60	78.66	244.1	.976	.976
	"	5	36	55	63	1⅛	" "	1.126	30.11	206.9	1.24	31.35	68.28	211.9	.976	

The following nozzle was precisely the same form as A, except that straight part of nozzle was cut off close to small end of cone. See Fig. 17.

(C)	Sept. 25. A.M.	3	28	60	63	1⅛	Smooth Cone.....	1.125	40.52	239 9	1.66	42.18	79.20	245.4	.977	.977
	"	3	36	60	63	.....	.....	1.125	30.08	206.7	1.24	31.32	68.24	211.4	.978	

The following is the same nozzle A with a small hollow cone inserted to precisely fill enlargement next end of play-pipe and make waterway smooth. See Fig. 15.

(D)	Oct. 10, P.M.	4	33	42	56	1⅛	Cone A, etc.....	1.126	40.09	237.8	1.63	41.72	78.78	244.5	.973	.971
	"	4	39	42	56	.....	.....	1.126	30.09	205.3	1.23	31.26	68.19	211.6	.970	

For next nozzle see Fig. 14. This nozzle was tested on straight cylindrical brass play-pipe, 30 inches long, inside very smooth.

(E)	Sept. 27, P.M	5	40	62	62	1⅛	Large Unif. Acceleration	1.133	44.80	254.1	1.85	46.65	83.29	261.7	.971	.971
	"	5	36	62	62	.....	" "	1.133	40.04	241.0	1.67	41.71	78.76	247.5	.974	
	"	5	32	62	62	.....	.....	1.133	30.13	208.0	1.26	31.39	68.32	214.7	.969	

See Fig. 18 for the following. This nozzle was tested on taper play-pipe 30 inches long, 2½ inches diameter at butt, 1¾ inches inside diameter at throat, and with "rifle" blades inserted as shown in Fig. 18.

(F)	Aug. 23, P.M.	4	01	64	69	1⅛	Small Unif. Acceleration.	1.122	51.99	268.4	2.07	54.06	89.67	276.34	.971	.971

The following nozzle was tested on same conical pipe shown in Fig. 13.

(H)	Sept. 22, P.M.	1	32	59	64	1¼	Short "Choke Bore"....	1.260	39.95	300.5	2.60	42.55	79.56	309.20	.972	.971
	"	1	28	59	64	.....	"	1.260	29.78	258.8	1.91	31.69	68.66	266.84	.970	

The following nozzle was slightly converging to the very end, which tends to make co-efficient slightly smaller than the preceding ones.

(I)	Oct. 10, P.M.	5	12	40	56	1¼	Large Unif. Accel. with end piece removed.	1.262	40.02	300.2	2.60	42.62	79.62	310.4	.967	.967
	"	5	17	.....	56	.....		.....	30.11	260.4	1.94	32.05	69.05	269.2	.967	

The following is the nozzle shown by the full lines in Fig. 23, and was tested on the play-pipe shown in same figure. The lower value of co-efficient is owing to increased friction, due narrowness of waterway near down-stream end of play-pipe, this being a cheap, light-weight pipe.

(J)	Sept. 27, A.M.	10	12	65	62	1¼	Smooth, with narrow throat.	1.250	44.87	305.2	2.68	47.55	84.10	321.7	.949	.946
	"	10	06	65	62	.....	"	1.250	39.89	286.9	2.34	42.23	79.25	303.1	.946	
	"	9	52	65	62	.....	"	1.250	29.99	248.2	1.78	31.77	68.73	262.9	.944	
	"	9	34	65	62	.....	"	1.250	20.00	203.0	1.20	21.20	56.15	214.8	.945	

The following experiment is upon same nozzle and play-pipe as A, but obstructed by inserting hollow cone, which formed a sharp cornered ridge, projecting into waterway one-tenth inch, as shown in Fig. 22.

(K)	Oct. 10, P.M.	4	21	42	56	1⅛	Nozzle A, obstructed....	1.126	40.06	235.8	1.60	41.66	78.71	244.3	.965	.961
	"	4	26	42	56	.....	.....	1.126	30.04	202.4	1.19	31.23	68.15	211.5	.957	

There is an uncertainty of, say, $\frac{3}{1000}$ inch in diameter of the following nozzle, by reason of its "straight part" being not exactly cylindrical or of uniform diameter throughout length, but co-efficient must have been actually .980, at least.

(L)	Oct. 10, P.M.	4	46	42	56	1⅛	Bottle shaped.....	1.127	39.96	241.0	1.67	41.63	78.68	244.6	.985	.986
	"	4	52	42	56	.....	.....	1.127	29.79	208.5	1.26	31.05	67.96	211.3	.987	

text-books of Weisbach and others indicate as probable. To get the true discharge 18¾ per cent. should be added to the discharges for smooth nozzles stated in the Ellis tables and their numerous reproductions.

Table No. 2 is of especial interest as showing the small effect which change of form has on the co-efficient of discharge; and it is the results in this table which furnish the basis for the statement that water under high pressures may be measured with extreme accuracy by means of smooth hose nozzles of ordinarily good construction, and that this method of measurement, providing an accurate pressure gauge is at hand, and is connected directly to base of play-pipe by a suitable piezometer, is equal to all the requirements of a pumping engine test, and equal in accuracy to the ordinary weir measurement.

It is of interest in this connection to compare the agreement of these co-efficients with the agreement between the original weir experiments. Thus see "Lowell Hydraulic Experiments," page 125, for instance; also Smith's "Hydraulics," page 93 *et seq.* It will be seen that the agreement is more close for the nozzles than for the weirs. It should be borne in mind, however, that the weir experiment was a vastly more difficult undertaking, and the attainment of such a degree of precision when dealing with such large quantities as used by Mr. Francis and Messrs. Fteley and Stearns, was a very remarkable achievement. The field of usefulness for the nozzle, as a means of gauging, is a restricted one, but in this restricted field it has, I believe, a very high value.

A Committee of the American Society of Mechanical Engineers has now under consideration recommendations designed to secure uniform accurate practice in the testing of pumping engines. The well known expert steam engineer, Mr. George H. Barrus, as a member of the Committee having this matter directly in charge, recently presented a paper to the Boston Society of Civil Engineers, in which it was recommended that for the many situations where it was inexpedient to erect a weir, the pump itself be used as a water meter, by correcting its plunger displacement for clearance and slip, and repeatedly measuring stroke in cases where its length is not positive. To me this appears rather too uncertain a method for general application in important cases, and it is therefore suggested that for tests in connection with direct pumping systems or other cases where the erection of a weir is inconvenient, the discharge may be measured by nozzles with fully equal accuracy.*

* The use of the Venturi meter recently invented by Mr. Clemens Herschel, M. Am. Soc. C. E., is also to be recommended as a method much preferable to using the pump itself as a water meter. (Transactions, Vol. XVII, page 228, and Vol. XVIII, page 133.)

To go a step further, it would be easy and not expensive to construct and set up in a pumping station a conveniently portable pressure box of plate iron with outlets having rounded entrance, to which any desired number of play-pipes and nozzles could be screwed on. Under 80 pounds pressure four 1¼-inch nozzles, connected direct without hose, would discharge the full capacity of a pump discharging "two and one-half-million gallons per 24 hours;" and if such a device were permanently attached to the pump it would, in connection with some tables which could be readily computed, furnish an inexpensive, quick and exceedingly valuable means for enabling the engine-man to determine at any time whether the per cent. of loss of action in his pump was getting too large, by wearing of valves or other cause.

The difficulty which will first suggest itself and which may possibly render this means of measurement sometimes impracticable, is that the irregularities of pressure due to the pulsations of the pump would affect the accuracy of the gauge readings. This can probably be obviated if a pressure box is used, by an air chamber of moderate size on the pressure box, in connection with the throttle valve between the box and the pump, which should be provided for regulating or adjusting the required back pressure on pump. Moreover, it may be remarked that 5 per cent. pulsation or variation in pressure on a nozzle makes very much less proportional difference in its discharge than does 5 per cent. variation in depth over a weir.*

With regard to the gauge by which pressure on nozzle should be measured in thus testing delivery of a pump, it may be said that while an open mercury column is the best, yet a first-class Bourdon gauge will measure the water delivered by the pumping engine just as well as it will the steam supplied to the engine, and in fact will do it better; for an error of one pound at a pressure of 100 pounds upon the nozzle will cause an error of only one-half of 1 per cent. in discharge.

To avoid possible misapprehension, we will state here that the tables at the end of this paper, pages 444–447, entitled "Pump Inspection

* November, 1889. I have since devised and now have under construction an arrangement somewhat similar to that shown in Figs 43 and 44, with piezometer like Fig 2, located a little above point *B*, Fig. 44, which will probably not weigh more than 50 to 75 pounds, or cost more than sixty dollars exclusive of gauge; a single one of which will, I think, serve to gauge the discharge of a pumping engine throwing two million gallons per twenty-four hours with an accuracy fully equal or superior to the best weir measurement. This device can be calibrated once for all, can be moved from place to place as occasion demands, and as its setting up involves only the laying of three short lines of common rubber lined hose and requires no foundation, it can be set up and adjusted ready for use in half an hour. This promises to be of so much value as a means of gauging that a communication will probably be made to the Society regarding what test may determine as to its practicability or lack of practicability.

Tables," are not intended for such accurate measurements as just referred to. These are intended for the quick and ready use of the Waterworks Superintendent, Insurance Inspector or other every-day use, where absence of accurate piezometric contrivances does not admit of measuring pressure close to base of play-pipe. The variation in loss of head, in different pieces of hose, makes such a gauging as by Table B No. 1 uncertain to the extent of, say, in general, 5 per cent.; while by properly measuring the pressure directly at base of play-pipe, a result certain within one-half of 1 per cent. may be attained.

I desire to state here that this matter of the degree of accuracy attainable in measuring flow of water by means of nozzles was not one of the main objects of the experiments, but has developed itself incidentally as the results were computed; otherwise care would have been taken to design, with a special view to use in gauging, certain standard forms easily reproduced, and to determine the corresponding co-efficients of discharge.

The writer has since the experiments designed a standard form of play-pipe for ordinary use in mill fire protection equipments, which is now kept in the market as a matter of regular stock and can be procured of almost any maker of fire department supplies. This is shown in Fig. 7. For this, with nozzles $1\frac{1}{8}$ or $1\frac{1}{4}$ inches in diameter, of ordinary good finish, co-efficient of discharge may be taken as .977, with very great confidence that this will not be more than one-half of one per cent. in error. A suitable piezometer and gauge must be used, however,

Proposed Standard Nozzle.

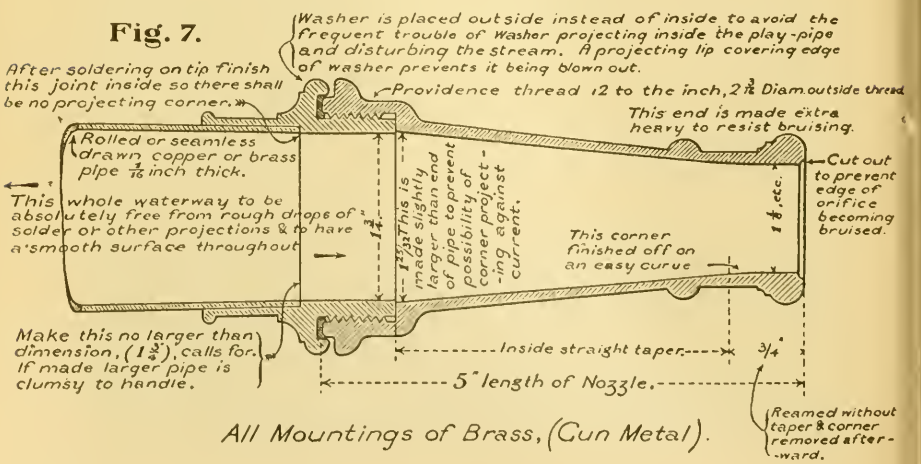


TABLE No. 3.—CO-EFFICIENT OF DISCHARGE OF RING NOZZLES.

Mark.	Date of Experiment.	Time. H.	Time. M.	Approx. Av. Temp. Air, deg. Fahr.	Temperature, Water, deg. Fahr.	Nominal Size, inches.	Kind of Nozzle.	Actual Diameter of Orifice, Inches.	Observed Pressure at Base of Play-pipe, Pounds.	Actual Discharge, Galls. per Min.	Corr. for Head due Vel. past Piez., lbs.	Static Pressure, Pounds.	Theoretical Velocity due Pressure, Feet per Second.	Theoretical Discharge, Gallons per Minute	Co-efficient of Discharge.	Mean Value Co-efficient.
(h)	Sept. 10, A.M.	8	41	70	69	$1\frac{1}{8}$	Square Ring.....	1.107	58.06	208.8	1.26	59.32	93.93	281.8	.741	.742
	Sept. 8, A.M.	11	19	67	69	$1\frac{1}{8}$	"	"	49.49	192.8	1.08	50.57	86.72	260.2	.741	
	Sept. 22, A.M.	12	00	59	64	$1\frac{1}{8}$	"	"	45.03	183.7	.98	46.01	82.73	248.2	.740	
	"	11	49	59	64	$1\frac{1}{8}$	"	"	30.11	150.2	.64	30.75	67.62	202.9	.740	
	"	11	42	59	64	$1\frac{1}{8}$	"	"	25.05	137.8	.54	25.59	61.69	185.1	.744	
	"	11	32	59	64	$1\frac{1}{8}$	"	"	19.96	123.3	.44	20.40	55.08	165.2	.746	
(i)	Sept. 10, A.M.	.....		70	69	$1\frac{1}{4}$	Square Ring.....	1.250	52.90	253.4	1.84	54.74	90.23	345.1	.734	.736
	Sept. 22, A.M.	11	19	59	64	$1\frac{1}{4}$	"	"	45.48	234.8	1.58	47.06	83.66	320.0	.733	
	"	11	14	59	64	$1\frac{1}{4}$	"	"	30.16	191.6	1.06	31.22	68.15	260.6	.735	
	"	11	7	59	64	$1\frac{1}{4}$	"	"	24.99	174.6	.87	25.86	62.02	237.2	.736	
	"	11	0	59	64	$1\frac{1}{4}$	"	"	20.07	157.4	.70	20.77	55.58	212.6	.740	
(j)	Sept 27, A.M	9	40	65	62	$1\frac{1}{4}$	Sq. $2\frac{1}{2}$ Cap.....	1.250	39.91	188.6	1.03	40.94	78.02	298.4	.632	.634
	"	9	36	65	62	$1\frac{1}{4}$	"	"	30.06	164.7	.76	30.82	67.71	259.0	.636	
(k)	Oct. 10, P.M.	3	45	42	56	....	Sq. Ring with Disch. Equiv to $1\frac{1}{8}$ smooth.	1.314	40.04	242.9	1.70	41.74	78.79	333.0	.729	.729
	"	3	55	42	56	....		"	30.01	210.6	1.28	31.29	68.22	288.4	.730	
(l)	Oct. 10, P.M.	.....		42	56	$1\frac{3}{16}$	Square Ring.....	1.188	40.16	236.1	1.60	41.76	78.81	272.3	.867	.866
	"	2	58	42	56	$1\frac{3}{16}$	"	"	30.12	204.0	1.10	31.32	68.24	235.8	.865	
(m)	Sept. 22, P.M.	12	28	59	64	$1\frac{1}{8}$	Undercut Ring.....	1.128	44.87	186.6	1.00	45.87	82.59	257.3	.725	.723
	"	12	24	59	64	$1\frac{1}{8}$	"	"	40.00	174.7	.89	40.89	77.99	242.9	.719	
	"	12	18	59	64	$1\frac{1}{8}$	"	"	35.19	165.2	.78	35.97	73.14	227.8	.725	
	"	12	15	59	64	$1\frac{1}{8}$	"	"	29.75	151.3	.65	30.40	67.24	209.5	.722	
	"	12	6	59	64	$1\frac{1}{8}$	"	"	20.02	124.3	.47	20.49	55.20	172.0	.723	
(n)	Sept. 22, P.M	12	56	59	64	$1\frac{1}{4}$	Undercut Ring.....	1.250	44.98	225.6	1.47	46.45	83.12	317.9	.710	.713
	"	12	52	59	64	$1\frac{1}{4}$	"	"	39.89	213.1	1.32	41.21	78.29	299.5	.712	
	"	12	43	59	64	$1\frac{1}{4}$	"	"	30.11	185.8	1.00	31.11	68.02	260.2	.714	
	"	12	34	59	64	$1\frac{1}{4}$	"	"	19.88	151.5	.67	20.55	55.29	211.5	.716	
(o)	Sept. 27, A.M.	9	21	65	62	$1\frac{1}{4}$	Undercut $2\frac{1}{2}$ Cap.....	1.251	40.09	173.5	.85	40.94	78.02	298.9	.580	.582
	"	9	15	65	62	$1\frac{1}{4}$	" "	"	30.03	150.8	.65	30.68	67.54	258.8	.583	
(p)	Sept. 22, P.M	1	5	59	64	$1\frac{1}{4}$	Taper Ring.....	1.247	39.90	247.1	1.76	41.66	78.71	297.7	.830	.830
	"	1	1	59	64	$1\frac{1}{4}$	"	"	29.81	213.7	1.33	31.14	68.05	257.4	.830	
(q)	Sept. 22, P.M.	1	22	59	64	$1\frac{1}{8}$	Short "Choke Bore"	1.131	45.09	231.0	1.54	46.63	83.27	260.8	.885	.886
	"	1	17	59	64	$1\frac{1}{8}$	"	....	40.12	217.4	1.38	41.50	78.57	246.0	.884	
	"	1	13	59	64	$1\frac{1}{8}$	"	....	29.88	188.7	1.03	30.91	67.81	212.4	.888	

REMARKS.

All these nozzles excepting (*j*) and (*o*) were each attached to the 30-inch play-pipe shown in Fig. 13 when tested, and pressure gauge was connected to piezometer at base of play-pipe, as shown in the same figure. For description of this play-pipe see Table No. 1.

Nozzles (*j*) and (*o*) were each attached to play-pipe shown in Fig. 14. The nozzles themselves are shown in Figs. 31 and 32.

Nozzles (*h*), (*i*), (*m*), (*n*) are varieties found regularly in the market.

Nozzles (*j*) and (*o*) were manufactured for these experiments, with a view to determining effect of great contraction on character of jet.

Nozzles (*l*), (*p*) and (*q*) were manufactured for these experiments, with a view to determining effect of slight contraction on character of jet.

In Nozzle (*k*) the projection of shoulder was greater than in (*h*) or (*i*), which accounts for the less value of co-efficient.

In measuring discharge of (*j*) a packing washer, as shown in Fig. 31, was inadvertently used of diameter smaller than main pipe.

to insure this degree of accuracy in the gauging, and diameter of outlet must be callipered with exactness and care.

As a means of gauging water and for diameters of an inch or two, the nozzle has a very great advantage in accuracy over the square edged orifice, since the edges of the latter are never absolutely square and the slight rounding makes the measurement of its diameter uncertain to a degree which may readily introduce an error of 1 per cent. in computation of discharge.

Discharge of Ring Nozzles.

(See table on page 333 *and figures on pp.* 323 *and* 324.*)*

The preceding table shows that for the forms of ring nozzle commonly met in practice, viz., nozzles (*h*) and (*i*), the co-efficient of discharge may be taken as .74. Thus we see that for the ring nozzle, as for the smooth nozzle, the Ellis tables are seriously in error; they being based on a value of .64 for the co-efficient. To get the true discharge $15\frac{5}{8}$ per cent. should be added to that given by the Ellis tables. In nozzle (*k*) the shoulder is rather deeper, and one with shoulders deeper than this is seldom found in practical use. The nozzles *M* and *N* are of a form believed by some good firemen to throw a better jet than any other form, and are thus frequently met in practice. The co-efficient is seen to differ less from the square ring than might perhaps have been expected. Nozzles (*l*), (*j*) and (*o*) are not styles in practical use, but were constructed for the purpose of investigating the effect of various degrees of contraction upon smoothness and reach of jet.

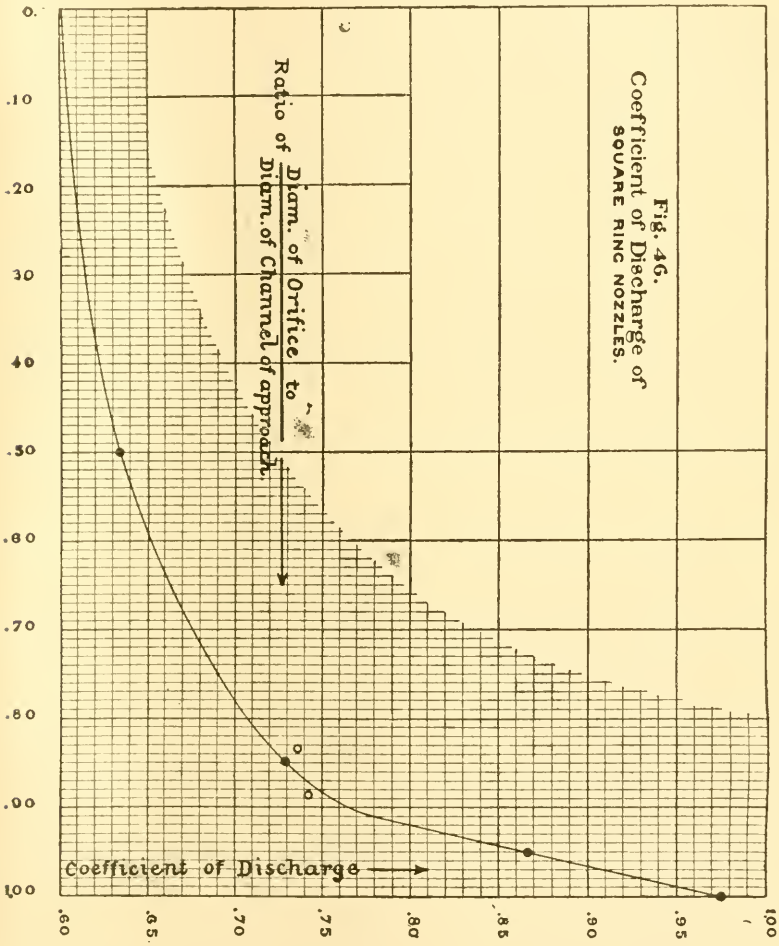
The results given in Table No. 3 cover quite a range of contractions, and are of some interest in helping define the influence of ratio between diameter of orifice and diameter of chanuel above it, upon the co-efficient of contraction.

This is a subject on which precise information is less complete than desirable. About the only published data which the writer now recalls is that referred to in "Weisbach's Mechanics" (see Cox's "Weisbach," Vol. I, page 841).

Professor Weisbach there gives certain corrections for incomplete contraction, based upon experiments of his own, which he says can be applied with great accuracy. These do not fit the experiments in Table No. 3 very well, differing from 1 to $7\frac{1}{2}$ per cent., as will be seen by table at top of page 336.

The results of these experiments on contraction are shown graphi-

cally by Fig. 46, and a curve is there drawn which, until the results of more complete experiments are presented, may be found of use in indicating proper value of co-efficient for similar problems. In this connec-



tion a study of the curves of distribution of velocity are of interest. (See Figs. 61 and 62.) Perhaps for a diaphragm at end of a long tube of uniform width, the co-efficient might be different.

This subject and that of effect of diaphragms in pipes are fruitful ones for investigation in detail, and might be taken up with advantage

in a well equipped hydraulic laboratory, such as that proposed in the new building of the Massachusetts Institute of Technology, and the ratios accurately determined once for all.

INFLUENCE OF RELATIVE AREA OF ORIFICE TO AREA OF CHANNEL OF APPROACH IN MODIFYING CO-EFFICIENT OF DISCHARGE OF NOZZLE.

Nozzle.	Diameter Orifice.	Diameter Channel.	Ratio Diameters.	Ratio Area of Orifice to Area of Channel.	Actual Co-efficient Discharge.	Ratio to Co-efficient Discharge for Complete Contraction = 0.60.	Weisbach's Ratio.	Co-efficient Discharge to be Expected from Weisbach's Ratio.	Actual Discharge less than per Weisbach.
									Per cent.
A, *B*, *etc.*	1.126	.....	1.00	1.00	.975	1.66	1.63		
*(*l*)	1.188	1.25	.950	.903	.866	1.443	1.475	.885	2.1
†(*h*)	1.107	1.25	.886	.785	.742	1.237	1.337	.802	7.5
†(*i*)	1.250	1.50	.833	.694	.736	1.227	1.255	.753	2.3
‡(*k*)	1.314	1.55	.848	.719	.729	1.215	1.275	.765	4.7
§(*j*)	1.250	2.50	.500	.250	.634	1.057	1.045	.627	1.1
						Ratio to Co-efficient, .50			
(*o*)	1.251	2.50	.500	.250	.581	1.162			

DISCHARGE OF SIAMESE NOZZLES.

The experiments on discharge of Siamese nozzles (see Table No. 4) are of no such general interest as the results in the preceding tables, but are merely of value as giving definite knowledge in reference to one special form of apparatus. The values found are of some general interest, however, as showing effect of various projections of shoulder upon contraction of jet. Probably had the piezometer been applied directly at base of the straight play-pipe the co-efficient for the smooth nozzle would have been not far from .975; and the less value found (by about 2 per cent.) may be regarded as due to the frictional resistance in the Siamese and elbow.

With nozzle (*s*), 1¾ inch diameter, there was less relative loss in Siamese and elbow, and the co-efficient is seen to approach the values in Tables Nos. 1 and 2 very nearly.

* Edges very sharp and square.

† This co-efficient would naturally be a little large, as shoulders are not in a perfect plane, but incline slightly in direction of current.

‡ Excellent experiment. Shoulders very sharp and square.

§ Edges very sharp and square.

TABLE No. 4.

Co-efficient of Discharge of Large Nozzles Combined with Siamese, etc.

Mark.	Date of Experiments.	Time. H.	Time. M.	Approx. Av. Temp. Air, deg. Fahr.	Temperature, Water, deg. Fahr.	Nominal Size, inches.	Kind of Nozzle.	Actual Diameter of Orifice, Inches.	Observed Pressure at entrance to Siamese, Pounds.	Actual Discharge, Gallons per Minute.	Cor. for Head due Vel. past Piez., lbs.	Static Pressure, Pounds.	Theoretical Velocity due Pressure, Feet per Second.	Theoretical Discharge, Gallons per Minute.	Co-efficient Discharge—combined Siamese and Nozzle.	Mean Value Co-efficient.	
(r)	Sept. 21, A.M.	10	44	60	64	2	Cone on Small Siamese...	2.014	40.10	707.2	1.60	41.70	78.75	782.0	.904	.908	
	"	10	39	60	64	2	" " ...	"	34.92	663.2	1.42	36.34	73.52	730.0	.908		
	"	10	30	60	64	2	" " ...	"	29.76	613.7	1.21	30.97	67.86	673.8	.911		
The play-pipe on which the above was tested is not shown in the drawings. It was a straight cylindrical pipe, 3 inches diameter for whole length. Bulb and elbow were also 3 inches diameter. Except for this smaller waterway, it was almost exactly like Figs. 43 and 44. The smaller co-efficient is due extra friction in small waterway. The following nozzles were tested on pipe shown in Fig. 44. Nozzle is shown on larger scale in Fig. 37.																	
(s)	Sept. 21, A.M.	12	00	60	64	2	Cone on Big Siamese.....	2.023	40.21	755.8	1.81	42.02	79.05	792.0	.954	.953	
	"	11	53	60	64	2	" "	"	30.00	651.9	1.37	31.37	68.30	684.3	.953		
(t)	Sept. 22, A.M.	10	05	57	64	$1\frac{3}{4}$	Cone on Big Siamese.....	1.762	39.98	575.2	1.07	41.05	78.14	593.9	.968	.969	See Fig. 42.
	"	9	57	57	64	$1\frac{3}{4}$	" "	"	35.09	538.5	.93	36.02	73.19	556.3	.968		
	"	9	50	57	64	$1\frac{3}{4}$	" "	"	30.07	500.3	.79	30.86	67.75	514.9	.972		
Co-efficient of the last nozzle is larger, probably due to relatively smaller loss in elbow, etc.																	
(u)	Sept. 21, P.M.	3	43	60	64	2	Square Ring on $2\frac{1}{4}$-in. Noz.	2.000	39.87	587.4	1.11	40.98	78.07	764.5	.768	.767	See Fig. 38.
	"	3	29	60	64	2	" " ..	"	29.95	508.2	.82	30.77	67.66	662.5	.767		
(v)	Sept. 21, P.M.	4	01	60	64	2	Square Ring on $2\frac{1}{2}$-in. Noz.	2.000	34.89	475.8	.71	35.60	72.77	712.6	.668	.668	See Fig. 40.
	"	3	53	60	64	2	" " ..	"	29.84	441.2	.62	30.46	67.31	659.1	.669		
(w)	Sept. 21, P.M.	5	38	60	64	$2\frac{1}{8}$	Square Ring on $2\frac{1}{4}$-in. Noz.	2.122	34.98	657.9	1.39	36.37	73.54	810.6	.812	.810	See Fig. 41.
	"	4	57	60	64	$2\frac{1}{8}$	" " ..	"	30.14	608.6	1.19	31.33	68.26	752.4	.809		
(x)	Sept. 21, P.M.	4	48	60	64	2	Unc. Ring on $2\frac{1}{4}$-in. Nozzle.	2.008	34.98	552.4	.99	35.97	73.14	721.9	.765	.762	See Fig. 39.
	"	4	36	60	64	2	" " ..	"	30.05	507.6	.82	30.87	67.75	668.7	.759		
The following is the same as Fig. 38, with smaller ring removed.																	
(y)	Sept. 21, P.M.	5	30	60	64	$2\frac{1}{8}$	Unc. Ring on $2\frac{1}{4}$-in. Nozzle	2.113	34.78	654.8	1.37	36.15	73.33	801.5	.817	.815	
	"	5	21	60	64	.....	.....	.....	29.82	603.3	1.16	30.98	67.88	741.9	.813		

NOTE.—These values are in proper shape for practical use, but in comparing them with co-efficients for ordinary nozzles and play-pipes, the fact that these values clude effect of loss due friction in the Siamese and the 45 degree elbow must be kept in mind. All nozzles in this table were tested on a Siamese, and pipe similar to at shown in Figs. 43 and 44. Piezometer was connected immediately back from Siamese.

Loss of Pressure by Friction in Hose.

This was determined for fourteen different kinds of $2\frac{1}{2}$-inch hose intended to represent all the typical kinds in ordinary use. No attempt was made to experiment upon all the different makes, sizes and brands in the market, these being so very numerous as to make such a work impracticable.

In each kind of hose tested, the friction loss was determined with inch, $1\frac{1}{8}$, $1\frac{1}{4}$ and $1\frac{3}{8}$-inch smooth nozzles attached, and in nearly all kinds it was also determined with $\frac{3}{4}$, $\frac{7}{8}$ and $1\frac{1}{2}$-inch nozzles.

The hydrant pressure in the various experiments varied from 15 to 125 pounds per square inch, and the delivery of the hose from about 50 to 325 gallons per minute; the corresponding mean linear velocity in some of the kinds of hose thus ranging from 3 to about 21 feet per second.

The loss of pressure by friction was found to follow the common theory of being proportional to the square of the discharge closely enough for nearly all practical purposes; but the fact that there was a slight divergence from this law was also clearly shown. (See plotted curves, Fig. 48. The full lines show mean results of the experiments. The dotted lines are mathematical curves of squares drawn through the 240-gallon point.)

The length of lines of hose experimented upon varied from 50 to 325 feet, and as was to be expected as a matter of course, the friction loss was found to be substantially proportional to the length; that is, the friction loss in a line 300 feet long was almost exactly three times that in a line 100 feet long. (See plotted curves; note, for instance, with Sample K, how closely the observations on loss per 100 feet coincide, though lines vary in length.)

The general method of making these experiments (see figures on page 315, also Fig. 50) was to lay out a line of hose of convenient length, which might be anywhere from 50 to 325 feet, but usually was 150 or 300 feet, in a straight line upon a fairly smooth plank walk or platform which sloped 1 foot in 100 down toward nozzle. Immediately alongside the plank walk or platform on which the hose was laid were three hydrants about 100 feet apart, and hose was connected to whichever came most convenient for the length under experiment. To the downstream end of this straight line of hose the play-pipe was attached and bent upward at an angle of 30 degrees. This vertical bend in the hose

was made on curve of 10 feet radius by the hose lying in a box or trough of that form. To the up-stream end of the straight line of hose a 25-foot length of hose like sample *C* was attached, and its upper part curved around in whatever way was necessary to connect it to hydrant, though care was taken that this curve should be of not less than about 4 or 5 feet radius.

Figs. 1 to 6 illustrate various features pertaining to the arrangement of the apparatus. Fig. 2 illustrates the couplings used for measuring the pressure within the hose, and already described.

There were two of these piezometer couplings used with each line of hose. The customary location of the up-stream one was in the straight line of hose 25 feet away from the hydrant, and of the down-stream one close to the base of play-pipe, though occasionally they were connected into the line of hose at other points to make sure that there was no local disturbance at the points first named.

The two mercury gauges by which pressures were measured, already described, were fastened in most cases to adjacent brick walls and connection made to the piezometers just described by from 8 to 15 feet of rubber tubing. Much care was taken to support this tubing on an even incline up toward gauge, so air bubbles should not lodge and affect results, and immediately before each set of experiments the waste cock in the gauge was opened for a few minutes so that the swift current of water through this small tube should effectually wash out any possible bubbles. The cocks at ends of this tubing were generally kept nearly wide open during an experiment, for I believe small errors are often introduced by the too close throttling of a gauge as often practiced to check oscillations.

As a check, Bourdon test gauges were, in several experiments, connected directly to the piezometer without the intervention of so much tubing. All these checks only tended to confirm the accuracy of the method generally followed.

The line of hose being stretched out in a straight line along the platform and the water let into it, then when all was in a condition of steady flow the experiment on friction loss was made. One observer, with a helper who swung the play-pipe, measured the gallons per minute discharged, as described on page 314, the two other observers meanwhile noting at half-minute intervals the readings of the pressure gauges at each end of the sample of hose under experiment.

In most cases the water was supplied from the city water-works, but in certain experiments, designed to show the effect of a very high pressure within the hose upon the friction, the water was supplied by the mill fire pumps. When water from the city reservoir was used, the fluctuations of pressure did not often exceed ½ or ¾ pound in the course of an experiment, or 0.1 to 0.2 pound between successive observations; while, when supply came from pump, the unsteadiness of pressure often made a difference of two or even three pounds at up-stream gauge or of half this amount at the down-stream gauge, but at this down-stream gauge there was seldom a difference of ½ pound between successive readings.

As each experiment on friction loss was computed from the average of five to ten independent observations taken simultaneously at the two ends of the hose experimented upon, I feel confident that the error in determination of pressure lost by friction between the two ends of any sample did not exceed ½ pound, and in general was much less than this.

Various rates of delivery were secured by attaching nozzles of different sizes to the play-pipe, the hydrant gate being generally opened to its full extent. With the exception of the 25-foot piece ordinarily used next the hydrant, all hose experimented upon was in the ordinary 50-foot commercial lengths, and all was fitted with ordinary expanded ring couplings (see Fig. 52), thus giving a waterway of the full nominal diameter of the hose. Care was taken in all cases that the hole in the elastic washer used for packing the joint should be of full size of bore of coupling. The following table (No. 5) gives results of the experiments. It is to be remarked that while temperature of water as given is probably correct within 1 or 2 degrees, the temperature of air was observed with less care, and as it varied at different parts of the line, the figure given may vary from the true mean by 5 degrees.

The results on the several sheets of Table No. 5 were plotted on cross section paper on scale of 1 centimeter = 1 pound per square inch loss per 100 feet, and 2 millimeters = 1 gallon per minute. A photo-engraving on small scale (see Fig. 48) is given of a tracing from about half these curves; the remainder having been omitted from this plotting simply to avoid confusion. Table No. 6 gives a comparison of the relative loss found in the different kinds of hose; the values there given having been taken from the plottings of Table No. 5 just mentioned.

TABLE No. 5.

EXPERIMENTS ON FRICTION LOSS IN FIRE HOSE. **Sample L: $2\frac{1}{2}$-inch Ordinary Unlined Linen Fire Hose.** (Ross, Turner & Co.'s make.) Actual average Internal Diameter of Hose, 2.60 inches; Diam. measured under Pressure in Experiment 7. Actual average Internal Diameter of Couplings, 2.50 inches.

Date.—1888.	Time. H.	Time. M.	Temperature, Fahr. Air (approx.)	Temperature, Fahr. Water.	No. of Experiment.	Pressure Gauge, Corrections, due Level, etc., Used. Up Stream.	Pressure Gauge, Corrections, due Level, etc., Used. Down Stream.	Observed Pressures at Piezometers (Corrected). (Lbs. per sq. in.) Up Stream.	Observed Pressures at Piezometers (Corrected). (Lbs. per sq. in.) Down Stream.	Length of Hose between Piezometers. (Feet.)	Loss of Pressure per 100 feet in Length of Hose.	Average Pressure in Hose.	Actual Discharge of Nozzle as Measured in Tank.	Mean Quantity Flowing through Hose, Corrected for Leaks (Gal. per min.)	Nozzle Used. Size.	Nozzle Used. Kind.	Pounds increased loss of pressure per 100 feet due sinuosity.	Per cent. increased loss of pressure due sinuosity.
Hose laid straight; water supplied from City Reservoir.																		
August 22—A.M	11	52	76	72	1	+0.35	+0.15	12.94	10.47	153.5	1.61	13	54.4	54.7	$\frac{3}{4}$	smooth (*a*)		
"	11	38	.....	72	2	"	"	29.23	23.79	154.2	3.53	28	81.5	81.8	$\frac{3}{4}$	" (*a*)		
"	11	23	.....	72	3	"	"	52.04	42.52	154.9	6.15	49	108.1	108.4	$\frac{3}{4}$	" (*a*)		
"	8	45	70	72	4	"	"	66.62	54.43	"	7.86	62	122.8	123.1	$\frac{3}{4}$	" (*a*)		
"	9	00	.....	72	5	"	"	65.72	46.68	"	12.29	58	156.6	156.9	$\frac{7}{8}$	" (*b*)		
"	9	14	.....	72	6	"	"	65.35	39.40	"	16.75	54	185.4	185.7	1	" (*c*)		
"	9	25	.....	72	7	"	"	63.46	30.88	"	21.03	49	209.4	209.7	$1\frac{1}{8}$	" (*A*)		
August 21—P.M	5	37	65	72	8	+0.30	+0.10	62.06	23.59	"	24.84	44	226.6	227.1	$1\frac{1}{4}$	" (*h*)		
August 22—A.M	9	37	75	72	9	+0.35	+0.15	61.97	23.81	"	24.63	44	228.1	228.4	$1\frac{1}{4}$	" (*h*)		
"	9	52	.....	72	10	"	"	60.93	18.34	"	27.50	41	241.8	242.1	$1\frac{3}{8}$	" (*f*)		
"	10	04	.....	72	11	+0.42	+0.22	59.60	11.64	"	30.96	37	257.8	258.1	$1\frac{55}{100}$	nozzle off		
Same hose laid straight, but supplied by rotary fire pump under higher pressures.																		
August 22—P.M	4	23	70	72	12	+0.35	+0.15	79.29	30.68	155.	31.36	57	258.8	259.1	$1\frac{1}{4}$	smooth (*h*)		
"	4	10	.....	72	13	"	"	93.71	36.58	"	36.86	67	281.2	281.5	$1\frac{1}{4}$	" (*h*)		
"	5	08	.....	72	15	+0.42	+0.23	103.98	40.31	"	41.08	74	296.2	296.5	$1\frac{55}{100}$	nozzle off		
"	4	55	.....	72	16	"	"	96.05	18.92	"	49.76	59	328.4	328.7	$1\frac{55}{100}$	"		
Same hose, with natural sinuosity due to its elongation under pressure. Hose was laid straight before admitting water, but under pressure took this sinuous form.																		
August 21—P.M	4	44	67	72	17	+0.30	+0.10	64.75	52.27	154.9	8.06	60	119.8	120.3	$\frac{3}{4}$	smooth (*a*)	0.51	6.3
"	4	58	.....	72	18	"	"	66.94	46.62	"	13.12	58	156.5	157.0	$\frac{7}{8}$	" (*b*)	0.81	6.2
"	5	11	.....	72	19	"	"	63.68	29.91	"	21.80	48	205.5	206.0	$1\frac{1}{8}$	" (*A*)	1.42	6.5
"	5	25	.....	72	20	"	"	62.16	22.95	"	25.31	44	223.2	223.7	$1\frac{1}{4}$	" (*h*)	1.51	6.0

NOTE.—In Experiments 17 to 20, position of hose was very nearly the same in each experiment. There were thirteen bends in all. Radius varied from 3 feet to 10 feet, but averaged about 5 feet. Average length of curves about 60 degrees of arc. There were no kinks or constrictions at any time.

A comparison of plotting of Experiments 11 and 12 is interesting, as showing small effect that increase of pressure within hose had in modifying friction loss or changing diameter of water-way for this kind of hose.

TABLE No. 5—(*Continued*).—EXPERIMENTS ON FRICTION LOSS IN FIRE HOSE. **Sample K:** 2½-inch Woven Cotton Rubber Lined "Mill" Hose. (Boston Woven Hose Co.'s make.) Average Internal Diameter of Hose measured under conditions of Experiment 28, 2.53 inches.

Date.—1888.	Time. H.	Time. M.	Temperature, Fahr. Air. (approx.)	Temperature, Fahr. Water.	No. of Experiment.	Pressure Gauge, Corrections, due Level, etc., Used. Up Stream.	Pressure Gauge, Corrections, due Level, etc., Used. Down Stream.	Observed Pressures at Piezometers (Corrected). Up Stream.	Observed Pressures at Piezometers (Corrected). Down Stream.	Length of Hose between Piezometers.	Loss of Pressure per 100 feet in Length of Hose.	Average Pressure in Hose.	Actual Discharge of Nozzle as Measured in Tank.	Mean Quantity Flowing through Hose, Corrected for Leaks.	Nozzle Used. Size.	Nozzle Used. Kind.	Per cent. increased loss of pressure due sinuosity.
Three 50-foot sections of hose laid straight; water supplied from City Reservoir.																	
August 28—A.M	10	14	68	71	21	+0.36	+0.16	12.43	10.56	167.3	1.12	13	54.4	54.4	¾"	smooth (*a*)	
"	9	56	.....	71	22	+0.36	+0.16	28.04	23.63	167.5	2.63	27	81.0	81.0	¾	" (*a*)	
"	9	38	.....	71	23	"	"	51.28	42.75	168.1	5.07	49	108.7	108.7	¾	" (*a*)	
"	9	10	.....	71	24	"	"	68.34	56.96	166.5	6.83	64	125.4	125.4	¾	" (*a*)	
"	11	00	.....	71	25	"	"	42.49	30.88	168.1	6.91	38	126.8	126.8	⅞	" (*b*)	
"	10	35	.....	71	26	"	"	66.73	48.15	168.5	11.03	57	159.1	159.1	⅞	" (*b*)	
"	11	12	.....	71	27	"	"	64.94	39.87	168.6	14.87	54	186.3	186.3	1	" (*c*)	
"	11	30	.....	71	28	"	"	63.19	31.29	168.5	18.93	49	211.3	211.3	1⅛	" (*A*)	
"	11	40	.....	71	29	"	"	61.66	24.14	168.3	22.29	44	229.8	229.8	1¼	" (*h*)	
"	11	48	.....	71	30	"	"	60.42	18.54	168.3	24.88	41	243.6	243.6	1⅜	" (*f*)	
"	11	55	.....	71	31	+0.43	+0.23	59.08	11.85	168.3	28.06	37	259.9	259.9	nozzle off.		
Same three sections of hose laid straight; water supplied by rotary fire pump at higher pressure.																	
August 31—P.M	2	55	85	74	32	+0.10	+0.10	106.76	77.29	171.8	17.15	94	201.5	201.5	⅞	smooth (*b*)	
"	2	07	.....	74	33	"	"	61.38	24.16	170.2	21.87	44	230.1	230.1	1¼	" (*h*)	
"	1	56	.....	74	34	"	"	72.83	28.71	170.2	25.92	52	250.2	250.2	1¼	" (*h*)	
"	1	45	.....	74	35	"	"	93.92	37.09	170.2	33.39	67	284.3	284.3	1¼	" (*h*)	
"	1	36	.....	74	36	"	"	102.73	40.62	170.3	36.47	73	297.9	297.9	1¼	" (*h*)	
Same three sections, with three additional ones, all laid straight; average diameter of the six sections = 2.54 inches; water from City Reservoir.																	
September 4—A.M	9	54	65	71	37	+0.05	+0.10	67.04	29.82	338.3	11.00	50	160.6	160.7	1	smooth (*c*)	
"	9	44	.....	71	38	"	"	66.00	21.89	"	13.04	46	175.8	175.9	1⅛	" (*A*)	
"	9	28	.....	71	39	"	"	65.29	16.19	"	14.51	42	186.0	186.1	1¼	" (*h*)	
Same hose as last experiments, but gauges connected at 50 feet from ends, thus including four sections (average diameter, 2.54). Bourdon gauges used in the two following experiments and placed close to hose. Hose all laid straight; water from City Reservoir.																	
September 3—P.M	5	35	66	71	40	00	+0.10	61.6	36.2	227.3	11.17	50	162.0	162.1	1	smooth (*c*)	
Same as Experiment No. 40, except that hose was left lying with the natural sinuosity due to its elongating when pressure was turned on; this line contained about 25 bends between gauges; average radius 5 to 8 feet; length of curves about 60 degrees of arc; no kinks or cramps.																	
September 3—P.M	5	23	66	71	41	00	+0.10	61.8	35.5	227.3	11.57	50	160.8	160.9	1	smooth (*c*)	5.0
Same kind of hose between gauges; down-str. gauge 50 ft. up from nozzle; up-str. gauge 25 ft. from hydrant as usual. Diam. prob. abt. 2.53.																	
August 30—A.M	11	38	90	72	42	+0.20	0.0	61.31	40.73	112.8	18.26	53	208.5	208.5	1⅛	smooth (*A*)	
"	1	39	.....	72	43	"	0.0	63.17	41.95	112.8	18.81	54	211.9	211.9	1⅛	" (*A*)	
"	11	54	.....	72	44	"	0.0	61.67	36.58	111.6	22.48	51	229.4	229.4	1¼	" (*h*)	

TABLE No. 5—(*Continued*).—EXPERIMENTS ON FRICTION LOSS IN FIRE HOSE.

Date.—1888.	Time. H.	Time. M.	Temperature, Fahr. Air (approx.)	Temperature, Fahr. Water.	No. of Experiment.	Pressure Gauge, Corrections, due Level, etc., Used. Up Stream.	Pressure Gauge, Corrections, due Level, etc., Used. Down Stream.	Observed Pressures at Piezometers (Corrected). Up Stream.	Observed Pressures at Piezometers (Corrected). Down Stream.	Length of Hose between Piezometers.	Loss of Pressure per 100 feet in Length of Hose.	Average Pressure in Hose.	Actual Discharge of Nozzle as Measured in Tank.	Mean Quantity Flowing through Hose, Corrected for Leaks.	Nozzle Used. Size.	Nozzle Used. Kind.
Sample D: 2½-inch Woven Cotton, Rubber Lined Hose, with very smooth interior. (Boston Woven Hose Co.'s "O. K" Brand.) (A medium weight unjacketed hose.) Av. diam. measd. in Expt. No. 46, 2.49 inches. Three 50-foot sections laid straight and supplied from City Reservoir.																
September 28	3	41	60	62	45	+0.02	−0.12	64.11	46.32	170.1	10.46	57	200.1	200.1	1	smooth (*c*)
" 28	3	51	.....	.....	46	"	"	61.34	38.07	170.3	13.66	51	233.1	233.1	1¼	" (*A*)
" 29	9	28	.....	60	47	"	"	58.63	30.36	170.5	16.58	46	257.0	257.0	1¼	" (*h*)
" 28	4	42	.....	62	48	"	"	59.03	30.55	170.3	16.72	46	258.8	258.8	1¼	" (*h*)
" 28	4	48	.....	62	49	"	"	56.98	24.22	170.5	19.21	42	277.8	277.8	1⅜	" (*f*)
" 29	9	56	.....	60	50	"	"	56.71	24.04	171.6	19.04	42	278.6	278.6	1⅜	" (*f*)
One section from the above—laid straight—average diameter 2.49 inches. Up-stream Piezometer, 50 feet from Hy. Down-stream piezometer, 50 feet from nozzle. Mer. gauges set side by side midway of hose, with long tubes leading thence to piezometers.																
September 24	2	18	60	63	51	00	00	63.76	54.10	57.20	16.89	59	259.6	259.6	1½	smooth (*A*)
"	6	14	.....	63	52	00	00	63.86	54.13	57.82	17.01	59	259.5	259.5	1½	" (*A*)
"	2	29	.....	63	53	00	00	58.47	43.85	57.17	25.57	51	324.3	324.3	1⅜	" (*f*)
"	6	36	.....	63	53½	00	00	58.63	43.86	57.82	25.54	51	325.0	325.0	1⅜	" (*f*)
A comparison of Experiments 51 and 52 with Experiments 47 and 48 is interesting as showing rate of loss of pressure to be same whether length is 50 or 150 feet.																
Sample C: 2½-inch woven cotton rubber-lined hose, with very smooth interior, Boston Woven Hose Co.'s "Boston Fire Jacket" Brand. This was intended to be the same as Sample D, except that a jacket was woven about its exterior. Mean diameter, measured in Experiment 55 = 2.47 inches. Three 50-foot sections laid straight and supplied from City Reservoir.																
August 28	2	18	68	71	54	+0.36	+0.16	64.17	46.98	162.	10.61	57	202.5	202.6	1	smooth (*c*)
"	2	30	.....	71	55	"	"	61.45	38.69	161.6	14.08	52	234.9	234.9	1¼	" (*A*)
"	2	41	.....	71	56	"	"	59.18	31.18	161.5	17.34	47	261.	261.	1¼	" (*h*)
"	2	58	.....	71	57	"	"	56.08	24.29	161.3	19.71	42	278.2	278.2	1⅜	" (*f*)
"	3	10	.....	71	58	+0.43	+0.23	54.64	16.54	161.3	23.62	37	308.	308.	nozzle off.	
Sample C': A second sample of hose, intended to be precisely like C. Same brand. Mean diameter, measured in Experiment 60 = 2.47 inches. Three 50-foot sections laid straight and supplied from Reservoir.																
September 29	11	23	55	60	59	+0.02	−0.12	64.15	46.45	154.6	11.45	57	200.8	200.8	1	smooth (*c*)
"	11	28	....	.....	60	"	"	61.49	38.18	"	15.08	55	233.4	233.4	1¼	" (*A*)
"	11	56	.....	.....	61	"	"	59.11	30.59	"	18.45	46	257.3	257.3	1¼	" (*h*)
"	12	03	.....	.....	62	"	"	57.12	24.18	"	21.31	37	279.2	279.2	1⅜	" (*f*)
(The above observation shows loss in C' to be about 5 per cent. greater than in C—no special reason for this is evident. Perhaps there was some difference in character of surface, located where it could not be observed.)																

NOTE—C and C' are omitted from plotting on Fig. 48 to avoid confusion. On original the curve for C agrees very closely with the curve for Sample D.

TABLE No. 5—(*Continued*).—Experiments on Friction Loss in Fire Hose.

Date.—1888.	Time. H. M.	Temperature, Fahr. Air (Approx.)	Temperature, Fahr. Water.	No. of Experiment.	Pressure Gauge, Corrections, due Level, etc., Used. Up Stream.	Pressure Gauge, Corrections, due Level, etc., Used. Down Stream.	Observed Pressures at Piezometers (Corrected). Up Stream.	Observed Pressures at Piezometers (Corrected). Down Stream.	Length of Hose between Piezometers.	Loss of Pressure per 100 feet in Length of Hose.	Average Pressure in Hose.	Actual Discharge of Nozzle as Measured in Tank.	Mean Quantity Flowing through Hose, Corrected for Leaks.	Nozzle Used. Size.	Nozzle Used. Kind.
Sample II: 2½-inch Knit Cotton Rubber Lined Hose. (The Cornelius Callahan Co.'s "Mill" Brand.) Three 50-foot sections laid straight and supplied by City Reservoir. Mean diameter, 2.62 inches. Measured under conditions of Experiment No. 64.															
August 28	4 33	68	71	63	+0.36	+0.16	64.10	47.51	157.1	10.56	57	203.	203.	1	smooth (c)
"	4 18	.....	71	64	"	"	61.56	39.50	157.6	14.00	52	236.7	236.7	1⅛	" (A)
"	4 42	.....	71	65	"	"	59.06	31.83	157.3	17.31	47	264.5	264.5	1¼	" (h)
"	4 51	.....	71	66	"	"	56.92	25.30	"	20.10	43	284.3	284.3	1⅜	" (f)
"	4 59	.....	71	67	"	+0.23	54.35	16.87	"	23.83	37	310.8	310.8	nozzle off.	
Same three sections laid straight, but supplied by rotary pump under higher pressure.															
August 31	3 45	85	74	68	+0.10	+0.10	100.26	83.44	158.4	10.61	93	208.8	208.8	⅞	smooth (b)
"	3 33	.....	74	69	"	"	55.86	29.92	158.6	16 36	44	256.9	256.9	1¼	" (h)
"	3 24	.....	74	70	"	"	74.54	40.44	"	21.50	59	297.2	297.2	1¼	" (h)
"	3 17	.....	74	71	"	"	90.66	49.32	"	26.07	72	328.	328.	1¼	" (h)
Six sections, including the above three—laid straight and supplied from Reservoir. Mean diameter, 264 inches. Measured under conditions of Experiment 73.															
September 4	4 25	65	71	72	+0.10	+0.10	65.57	40.03	316.6	8.07	54	186.3	186.4	1	smooth (c)
"	4 35	.....	71	73	"	"	63.64	31.21	"	10.24	48	209.7	209.8	1⅛	" (A)
"	4 45	.....	71	74	"	"	61.90	23.90	"	12.00	44	227.3	227.4	1¼	" (h)
"	4 13	.....	.....	75	"	"	65.72	39.94	"	8.14	54	185.8	185.9	1	" (c)

In Experiment 75 the same sections of hose used in Experiments 72-74 was used, but was left lying with the natural sinuosity due to its elongation of 10 feet, under pressure—it having been straightened prior to admitting the water. Fig. 50 is from a photograph taken during this Experiment. There were about twenty-five curves; average radius was probably 6 or 8 feet. Experiment 75 shows this sinuosity to have increased the friction loss only 2 per cent., an amount almost inappreciable under conditions of this experiment. Take note, however, that this brand of hose elongates less than some others.

Sample I—Hose similar to last sample, but a little heavier grade. (The Cornelius Callahan Co.'s "Volunteer" Brand.)															
August 28	5 36	60	71	76	+0.36	+0.16	64.11	48.32	157.4	10.03	58	205.5	205.5	1	smooth (c)
"	5 45	.....	71	77	"	"	61.27	40.26	"	13.35	52	238.8	238.8	1⅛	" (A)
"	5 55	.....	71	78	"	"	58.75	32.68	157.1	16.59	47	267.2	267.2	1¼	" (h)
"	6 02	.....	71	79	"	"	56.43	26.20	"	19.24	43	289.9	289.9	1⅜	" (f)
"	6 08	.....	71	80	"	"	53.46	17.57	157.0	22.86	37	318.6	318.6	nozzle off.	

TABLE No. 5—(*Continued*).—EXPERIMENTS ON FRICTION LOSS IN FIRE HOSE.

Date.—1888.	Time. H.	Time. M.	Temperature, Fahr. Air (approx.)	Temperature, Fahr. Water.	No. of Experiment.	Pressure Gauge, Corrections, due Level, etc., Used. Up Stream.	Pressure Gauge, Corrections, due Level, etc., Used. Down Stream.	Observed Pressures at Piezometers (Corrected). Up Stream.	Observed Pressures at Piezometers (Corrected). Down Stream.	Length of Hose between Piezometers.	Loss of Pressure per 100 feet in Length of Hose.	Average Pressure in Hose.	Actual Discharge of Nozzle as Measured in Tank.	Mean Quantity Flowing through Hose, Corrected for Leaks.	Nozzle Used. Size.	Nozzle Used. Kind.
						SAMPLE B.										
August 29	8	40	61	70	100	+0.36	+0.16	63.76	50.14	154.6	8.80	59	209.0	209.0	1	smooth (c)
"	9	02	.....	70	101	"	"	60.64	42.10	154.5	12.00	53	245.2	245.2	$1\frac{1}{8}$	" (A)
"	9	15	.....	70	102	"	"	56.53	33.75	"	14.74	47	271.6	271.6	$1\frac{1}{4}$	" (h)
"	9	21	.....	70	103	"	"	55.58	27.89	"	17.92	43	300.2	300.2	$1\frac{3}{8}$	" (f)
Six sections of hose used in the three following experiments—*i. e.*, the same hose used in Experiments 100–103, with as much more added.																
September 5	9	13	75	70	104	+0.05	+0.10	64.48	42.18	309.3	7.21	54	191.9	192.	1	smooth (c)
"	9	22	.....	70	105	"	"	62.69	33.69	"	9.38	49	218.7	218.8	$1\frac{1}{8}$	" (A)
"	9	40	.....	70	106	"	"	60.99	26.23	"	11.24	45	239.4	239.5	$1\frac{1}{4}$	" (h)
						SAMPLE A.										
August 29	10	50	64	70	107	+0.36	+0.16	63.29	51.32	153.1	7.81	59	211.9	211.9	1	smooth (c)
"	11	14	.....	70	108	"	"	57.04	35.96	152.9	13.79	48	280.8	280.8	$1\frac{1}{4}$	" (h)
"	11	21	.....	70	109	"	"	54.67	29.39	"	16.53	44	308.	308.	$1\frac{3}{8}$	" (f)
"	11	26	.....	70	110	"	"	51.16	19.99	"	20.39	37	339.9	339.9	nozzle off	
One section from the above (diam. av. .01 inch smaller than mean of the three above) tested with apparatus differently arranged. (Cor. made for diff. in diam.																
September 25	4	47	60	63	$110\frac{1}{2}$	0.	0.	.....	.....	51.8	16.34	56	305.4	305.4		
						SAMPLE J.										
August 30	8	38	90	72	111	+0.36	+0.16	64.00	55.19	102.2	8.62	61	199.7	202.1	1	smooth (c)
"	9	04	.....	72	112	"	"	61.55	49.87	"	11.43	57	230.1	232.3	$1\frac{1}{8}$	" (A)
"	9	33	.....	72	113	"	"	59.44	45.14	"	13.99	54	255.6	257.6	$1\frac{1}{4}$	" (h)
"	10	20	.....	72	114	"	"	56.54	40.45	"	15.74	50	272.5	274.2	$1\frac{3}{8}$	" (f)
"	10	38	.....	72	115	"	"	54.91	35.81	102.3	18.67	47	297.4	298.8	nozzle off	
						SAMPLE M.										
September 10	4	40	75	70	116	−0.20	+0.10	62.57	45.95	106.2	15.66	56	156.5	156.5	$\frac{7}{8}$	smooth (b)
"	5	10	.....	70	117	"	"	59.03	37.45	"	20.32	50	181.5	181.5	1	" (c)
"	5	27	.....	70	118	"	"	55.49	28.59	"	25.32	44	205.9	205.9	$1\frac{1}{8}$	" (A)
"	5	36	.....	70	119	"	"	50.97	20.67	"	28.53	37	219.9	219.9	$1\frac{1}{4}$	" (h)
"	5	42	.....	70	120	"	"	50.61	15.85	"	32.73	35	236.7	236.7	$1\frac{3}{8}$	" (f)
						SAMPLE N.										
August 24	.....		70	72	121	.....	.....	65.17	28.48	153.	23.98	.....	157.1	163.	1	smooth (c)
"	.....		.....	72	122	.....	.....	65.43	28.69	"	24.01	.....	158.3	164.	1	" (c)
"	.....		.....	72	123	.....	.....	63.22	13.16	"	32.72	.....	188.4	194.	1.319	noz. off
"	.....		.....	72	124	.....	.....	63.38	13.20	"	32.80	.....	188.2	194.	"	" "

TABLE No. 5—(*Continued*).—EXPERIMENTS ON FRICTION LOSS IN FIRE HOSE. Following samples were tested by water from City Reservoir. Diameter was in all cases measured during experiment with water discharging from $1\frac{1}{8}$-inch nozzle, under full reservoir pressure. Length of hose between piezometers was also measured under pressure, and includes the elongation due pressure. Hose was in each case straightened before beginning observations, except as noted for Sample E in Experiments 92, 93, 94. For fuller description of samples see Table No. 6.

Date.—1888.	Time. H.	Time. M.	Temperature, Fahr. Air. (approx.)	Temperature, Fahr. Water.	No. of Experiment.	Pressure Gauge, Corrections, due Level, etc. Used. Up Stream.	Pressure Gauge, Corrections, due Level, etc. Used. Down Stream.	Observed Pressures at Piezometers (Corrected). Up Stream.	Observed Pressures at Piezometers (Corrected). Down Stream.	Length of Hose between Piezometers.	Loss of Pressure per 100 feet in Length of Hose.	Average Pressure in Hose.	Actual Discharge of Nozzle as Measured in Tank.	Mean Quantity Flowing through Hose, Corrected for Leaks.	Nozzle Used. Size.	Nozzle Used. Kind.		Per cent. increased loss of pressure due sinuosity.
						SAMPLE G.												
September 3	11	42	70	72	81	+0.14	+0.10	64.15	48.28	155.7	10.19	58	204.9	204.9	1	smooth	(c)	
"	11	50	.....	72	82	"	"	61.46	40.08	155.6	13.74	52	238.6	238.6	$1\frac{1}{8}$	"	(A)	
"	1	28	.....	72	83	"	"	58.87	32.48	155.3	16.99	47	266.5	266.5	$1\frac{1}{8}$	"	(h)	
"	1	35	.....	72	84	"	"	56.62	25.91	"	19.77	43	288.2	288.2	$1\frac{3}{8}$	"	(f)	
"	1	45	.....	72	85	"	"	53.89	17.36	"	23.52	37	314.2	314.2	nozzle off.			
						SAMPLE E.												
October 10	10	03	43	56	86	+0.20	+0.03	68.44	63.05	153.2	3.52	67	132.6	132.6	$\frac{3}{4}$	cone	(a)	
"	10	16	.....	56	87	"	"	64.56	55.80	153.1	5.72	62	169.9	169.9	$\frac{1}{8}$	"	(b)	
"	10	24	.....	56	88	"	"	63.48	50.49	"	8.48	59	209.2	209.2	1	"	(c)	
"	10	37	.....	56	89	"	"	60.43	42.57	"	11.67	53	244.3	244.3	$1\frac{1}{8}$	"	(A)	
"	11	08	.....	56	90	"	"	57.42	35.04	153.4	14.59	48	274.6	274.6	$1\frac{1}{4}$	"	(h)	
"	11	16	.....	56	91	"	"	54.82	28.29	"	17.29	43	300.1	300.1	$1\frac{3}{8}$	"	(f)	
Same hose used in Experiments 86 to 91, but with natural sinuosity due elongation under pressure.																		
October 10	2	59	42	56	92	+0.20	+0.03	43.63	30.12	153.3	8.81	38	204.0	204.0	$1\frac{3}{16}$	ring	(l)	8.2
"	2	60	.....	56	93	"	"	57.64	40.16	"	11.40	50	236.1	236.1	$1\frac{3}{16}$	"	(l)	4.4
"	3	09	.....	56	94	"	"	58.13	39.99	"	11.83	51	238.7	238.7	$1\frac{1}{8}$	smooth	(B)	6.0
Amount of sinuosity probably not very different from Experiment 75, though somewhat greater. A photograph with plate horizontal, showing this sinuosity, was taken from roof of adjacent high mill, but was accidentally destroyed.																		
						SAMPLE F.												
October 15	3	52	50	54	95	00	00	66.13	61.19	51.7	9.56	65	177.7	177.7	$\frac{7}{8}$	cone	(b)	
"	4	06	.....	54	96	00	00	63.45	56.12	51 8	14.15	61	220.8	220.8	1	"	(c)	
"	4	13	.....	54	97	00	00	59.94	49 52	51.7	20.15	56	263.7	263.7	$1\frac{1}{8}$	"	(A)	
"	4	27	.....	54	98	00	00	56.25	42.57	51.6	26.51	51	304.1	304.1	$1\frac{1}{4}$	"	(h)	
"	4	33	.....	54	99	00	00	52.76	35.87	51.6	32.73	46	336.2	336.2	$1\frac{3}{8}$	"	(f)	

Influence of Smoothness of Surface of Waterway upon Loss of Pressure in Hose.

The most interesting and valuable point illustrated by the above table is the great effect which the smoothness of inside of hose exerts on the amount of pressure lost by friction. (See next page and also Fig. 47.)

Compare Sample *K* with Sample *D*. Each was a hose of woven cotton fabric of medium weight, lined with rubber, and both were made by the same manufacturer, and were practically alike, with the exception that *K* was rough inside, while *D* was smooth.

Sample *K* was made as most "mill hose" and much fire department hose is ordinarily made, viz., with a rather thin rubber lining, which was in calendering permanently roughened by the twill of the cloth used, and which is further roughened and corrugated by its conformation to the threads of the fabric, and is still further corrugated when in use by the rubber lining being forced by the water pressure into the vacant crevices between the threads; thus giving a surface as shown by the photograph in Fig. 47.

Sample *D* was made of same material, but with more care. Its rubber lining was calendered smoothly, and was slightly thicker than that in Sample *K*, and means were adopted for filling the crevices between the threads so that the rubber lining should be well backed up, and its inner surface not forced into a corrugated form by the action of the water pressure. This filling of the crevices may be attained either by the free use of suitable rubber cement, or by inserting the rubber lining into the cotton fabric in a semi-plastic, partially vulcanized condition, and then completing vulcanization under such conditions as to permit the rubber before hardening, to "flow" slightly, and thus fill the crevices between the threads.

Careful weaving is also essential. Sample *E* had an excellent lining, which was comparatively free from minute ridges, but a waviness of surface was produced apparently by the unequal tension in loom; and that the friction loss in this was, for equal diameters, relatively higher than *D* was mainly due to this slight irregularity of weaving, rather than to the character of the lining.

The cost of Sample *D*, in which the friction loss was only 14.2 pounds, was only five cents per foot greater than Sample *K*, in which loss was 25.5 pounds; part of which cost was due to heavier fabric and greater thickness of rubber lining. The extra cost of obtaining the greater smooth-

TABLE No. 6.—COMPARATIVE FRICTION LOSS IN VARIOUS KINDS OF FIRE HOSE. The following comparison is made on the basis of 240 gallons per minute, flowing through hose in each case, which is about the quantity discharged by a 1⅛-inch smooth nozzle under a pressure of 40 pounds per square inch at base of play pipe.

SAMPLE OF HOSE.	Diameter of couplings. Inches.	Average **internal diameter of hose.** Inches.	Mean velocity in hose. Feet per sec.	Increase in length under 50 lbs. average pressure.	Observed **loss of pressure** due friction in each 100 ft. of hose. Hose straightened and length measured under pressure. Lbs. per sq. in.	Percentage corresponding to diam. of hose to be added or subtracted to give friction which would have existed had diam. been exactly 2½ inches.	**Loss of pressure** due friction in each 100 ft. of hose, which this same hose would have shown **had diam. been exactly** 2½ inches. Lbs. per sq. in.
Sample A. **2 1-2 in. Solid Rubber Hose. Boston Woven Hose Co.'s "Extra."** A heavier hose than the next, weighing 25% more. The interior of this hose is free from ridges due threads of fabric, and was intended to be smooth as rubber could be made; being made on a highly polished steel mandrel.	2.52	**2.65**	13.96	3/4%	**10.0**	**Add 34%**	**13.4**
Sample B. **2 1-2 in. Solid Rubber Hose. B. W. H. Co.'s "Volunteer."** The interior of this hose is free from ridges due threads of fabric, and very smooth, but was made on a mandrel less highly polished than the last.	2.53	**2.60**	14.50	1½%	**11.5**	**Add 22%**	**14.0**
Sample C. **2 1-2 in. Woven Cotton, Rubber-Lined. B. W. H. Co's "Boston Fire Jacket Hose."** A Regular heavy Fire Department Hose. Rubber lining is thick, and interstices between threads next to rubber lining filled with rubber, so that inner surface remains almost free of ridges under pressure. Hose of same grade as Sample D with addition of a jacket. (See Photograph.)	2.53	**2.47**	16.07	4%	**15.0**	**Deduct 6%**	**14.1**
Sample D. **2 1-2 in. Woven Cotton, Rubber-Lined. B. W. H. Co.'s "O. K. Hose."** A medium-weight unjacketed hose. Rubber lining thicker than Sample K, and interstices between threads next to lining filled with rubber, so that inner surface remains almost free of ridges under pressure. Smoothness and character of lining apparently the same as Sample C. (See Photograph.)	2.47	**2.49**	15.81	5%	**14.5**	**Deduct 2%**	**14.2**
Sample E. **2 1-2 in. Knit Cotton, Rubber-Lined. N. Y. B. & P. Co.'s "Cable No. 2 Hose."** A medium-weight hose. Inner surface medium smooth under pressure. Interstices between threads next lining well filled wth rubber. (See Photograph.)	2.50	**2.68**	13.65	3½%	**11.3**	**Add 42%**	**16.0**
Sample F. **2 1-2 in. Knit Cotton, Rubber-Lined. N. Y. B. & P. Co.'s "Dart Hose."** A cheap, light-weight hose. Interior surface medium smooth.	2.50	**2.50**	15.69	4½%	**16.8**	**0**	**16.8**
Sample G. **2 1-2 in. Knit Cotton, Rubber-Lined. Callahan's "Jacket Hose."** A regular Fire Department Hose. Said to be about the same grade as Sample I with a jacket added.	2.51	**2.60**	14.50	1¾%	**13.9**	**Add 22%**	**17.0**

Sample H. **2 1-2 in. Knit Cotton, Rubber-Lined. Callahan's "Mill Hose."** A rather cheap, light-weight hose. Inside rather rough, due conformation of rubber lining to threads of fabric.	2.51	**2.62**	14.28	3%	**14.4**	**Add 27%**	**18.3**
Sample I. **2 1-2 in. Knit Cotton Rubber-Lined. Callahan's "Volunteer Hose."** A somewhat heavier hose than the last, otherwise apparently the same. (See Photograph.)	2.51	**2.69**	13.55	2½%	**13.5**	**Add 44%**	**19.4**
Sample J. **2 1-2 in. Leather Hose. S. Eastman & Co.'s "Improved."** A leather hose of excellent construction. Laps cut square.	2.50	**2.80**	12.51	2¼%	**12.2**	**Add 76%**	**21.5**
Sample K. **2 1-2 in. Woven Cotton, Rubber-Lined. B. W. H. Co's "Mill Hose."** A cheap hose with medium-thin rubber lining, which under pressure conformed to threads of fabric, thus making interior full of small ridges. (See Photograph.) I infer from comparative appearance of roughness that most other makes of woven "Mill Hose" of same weight would as now made give similar friction, but that they can be improved as illustrated in Sample D.	2.48	**2.53**	15.31	5%	**24.1**	**Add 6%**	**25.5***
Sample L. **2 1-2 in. Unlined Linen Hose.** This was an excellent piece of hose, made by Ross & Turner, and fairly represents friction loss in all ordinary unlined linen hose. (See Photograph.)	2.50	**2.60**	14.50	2½%	**27.2**	**Add 22%**	**33.2**
Sample M. **2 in. Woven Cotton, Rubber-Lined Hose.** Smoothness apparently same as sample D, as judged by inspection of interior at ends of sample.	2.07	**2.12**	21.81	4½%	**33.2**	**Deduct 56%**	**14.6**
Sample N. **2 1-4 in. Linen Hose with 2 in. Couplings.** Fabric same as Sample L. (Value deduced from experiments with 200 gallons per minute flowing in this hose.)	1.95	**2.30**	18.53		**49.5**	**Deduct 34%**	**32.7**
For comparison with above, it may be stated that the loss of pressure due friction in a common, clean, straight wrought-iron pipe coated with tar or asphaltum, and 2½ inches diameter, discharging 240 gallons per minute would be, computing and interpolating from experiments of Hamilton Smith, Jr., 15.7 lbs. per 100 feet; " " " " " Henry Darcy, 14.0 " " " " " } mean							**14.8**

The correction given in the sixth column is based upon the well-known hydraulic rule, that for the same quantity flowing in long pipes of different diameters, but equal smoothness, the friction loss is in inverse proportion to the fifth power of the diameter.

The truth of this rule within the limits of the above experiments is demonstrated very well by a comparison of the last two experiments on samples **M** and **N**, with experiments on samples **C** and **L**.

The figures in the seventh column give the true basis for judging of the relative excellence of a hose as regards smoothness of finish of interior.

*In justice to the Boston Woven Hose Co., the writer desires to state, that since learning the result of these experiments, they have been experimenting and modifying process of manufacture so that interior of mill hose shall retain a smooth surface even under heavy water pressure.

ness, when the proper arrangements are made at factory, will, it is believed, add but a very small percentage to the cost of the hose.

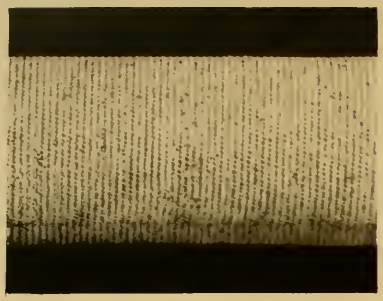
This point is well worthy the attention of all manufacturers of fire-hose. The writer is led to especially emphasize it from having many times heard hose manufacturers and hose salesmen affirm most positively that this little roughness of interior surface had no influence on the loss of pressure, that "the water next the side lay dead, etc.;" which notions, though conclusively proved to be erroneous, as every hydraulic engineer knows, by the experiments of Darcy thirty-five years ago, are still advocated as an aid to the disposal of imperfect fire-hose.

To determine the cause of the difference in friction loss in different kinds of hose, and to find out the condition of the surfaces exposed to the current of water under the practical conditions of use, plaster casts were made of the inside of pieces of hose from 1 to 3 feet in length, cut from the hose used in the experiments; to insure that these casts were fairly representative several casts from different parts of the hose were in some cases made. In making them, ordinary couplings were attached to these samples, and one end was capped. The sample was placed upright, filled with plaster of Paris, and then being instantly attached to a hydrant, a water pressure of 50 pounds per square inch was maintained on it until the plaster had set. These casts are of such interest in showing effect of roughness that photographs made from them are presented herewith. From inspecting these a reason will be seen why the friction loss in unlined linen hose is two and one-third times as great as in well made rubber lined hose, and nearly two and a half times as much as in an extra smooth solid rubber hose.*

The very great influence which smoothness of interior surface was found to exert upon the friction loss may excite question as to the accuracy of the observations. I would, therefore, call especial attention to the detailed results on Samples *L*, *K* and *D*, as given in Table No. 5.

First.—That the results are very accurate, the unusual precision of the apparatus leaves almost no room for doubt. That the quantity flowing was correctly measured is proved by a comparison which has been made for most experiments between the discharge as found by measurement

* It may be well to add, that while the great friction loss, as well as susceptibility to injury by chafing on cinders or sharp corners, makes unlined linen hose undesirable for use in long lines or for general outside use, nevertheless its extreme lightness, compactness and superior durability render it, in our opinion, better suited than any other kind for *inside* use in dry, well ventilated situations ,such as on the stairway standpipes of a mill.



SAMPLE L. **UNLINED LINEN HOSE.**

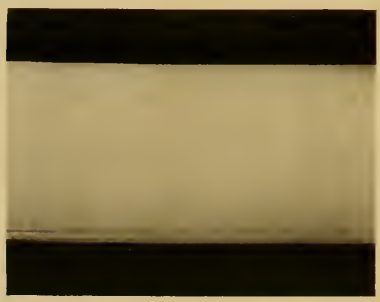
33.2 lbs. pressure lost by friction per 100 feet in length (or $2\frac{1}{3}$ times as much loss as in sample C, shown below).

SAMPLE K. **WOVEN COTTON, RUBBER-LINED "MILL HOSE."**

(Average depth of corrugations, $\frac{15}{1000}$ to $\frac{2}{100}$ inch.)

25 5 lbs. pressure by friction per 100 feet in length of hose, or 80 per cent more loss than in sample D or C, shown below.

(Rubber lining was rather thin, and crevices between threads next lining not being well filled, the water pressure caused inner surface to conform to threads of fabric.)



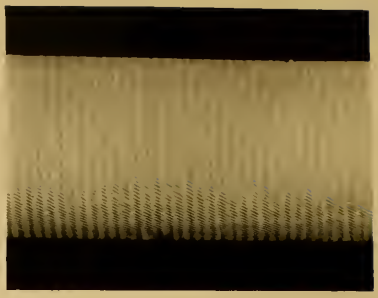
SAMPLE C. **WOVEN COTTON, RUBBER-LINED HOSE.**

(Average depth of corrugations, $\frac{1}{1000}$ to $\frac{2}{1000}$ inch.)

14.1 lbs. pressure lost by friction per 100 feet.

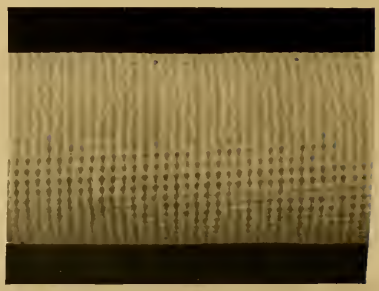
Rubber lining slightly thicker than in sample K, and interstices between threads next lining well filled with rubber, so inner surface remained smooth under pressure.

SAMPLE D had same kind of rubber lining, and gave loss of 14.2 lbs. per 100 feet.



SAMPLE X. Not tested for friction. Cast taken to illustrate unnecessary roughness often caused in process of manufacture.

Fabric and lining same as C, except that twilled cloth was used in calendering face of rubber lining, thus roughening surface.



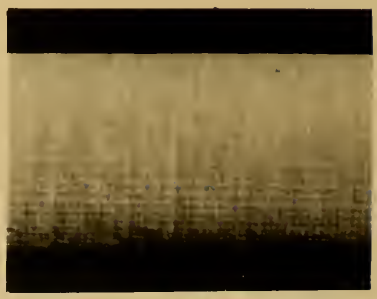
SAMPLE I. **KNIT COTTON, RUBBER-LINED HOSE.**

Interstices between threads next lining not well filled.

Average height of round-about ridges, about $\frac{1}{100}$ inch.

Average height of ridges which run lengthwise, about $\frac{15}{1000}$ inch.

19.4 lbs. pressure lost by friction per 100 feet in length (or 37 per cent more friction lost than in sample C).



SAMPLE E. **KNIT COTTON, RUBBER-LINED HOSE.**

Interstices between threads next lining well filled.

16.0 lbs. pressure lost by friction per 100 feet in length (or 13 per cent more friction loss than in Sample C).

FIG. 47. PHOTOGRAPHS FROM PLASTER CASTS OF INTERIOR OF FIRE HOSE.

Casts were all taken under a pressure of 50 lbs. per square inch. Photographs are half size of originals.

Losses of pressure given below are those found for 240 gallons per minute (or a good $1\frac{1}{8}$ inch fire stream) flowing in hose exactly $2\frac{1}{2}$ inches in diameter.

in the tank and the discharge computed for the given nozzle under the known pressure at its base.

Second.—That the piezometers were not subject to local influences is proved by the same result being obtained when positions of piezometers were changed by moving them 50 feet up-stream or down-stream in the same line of hose.

Third.—That there was no local obstruction in hose is proved by obtaining the same result with different pieces used.

Fourth.—That results are not influenced by accidental or occasional errors is shown by the regular, nearly geometrical form of the curve, and it is fair to add that throughout the whole range of these experiments no observation has been excluded simply because it failed to fall into line.

Fifth.—That gauges were all right is indicated by a similar result being obtained when Bourdon test gauges were used.

Sixth.—That diameter was correctly determined within narrow limits was proved by repeating measurements independently, and also by measurements made upon the plaster casts.

Seventh.—That the value for linen hose is fairly representative is shown by results with Sample *N* as compared with *L*, and also by equally accurate experiments on another sample, which agree closely but are omitted to save space, and also by some experiments with less perfect apparatus made a year previous. And that the comparative values for linen hose are about right was shown by laying two 300-feet lines of hose, one like Sample *L*, the other rubber lined, both fed from the same hydrant and connected to similar nozzles placed side by side as in Fig. 54, and noting the difference in height attained by the jets.

Should it be remarked that differences in friction loss due to smoothness are relatively as great or greater than Darcy's experiments showed for clean pipes *vs.* those obstructed by deposits, it may be said that Darcy gives little or no means of judging of the degree of roughness of his pipes with deposits, and probably their roughness was by no means so great as would be produced by the tubercles ordinarily known as such to American engineers.

Influence of Diameter upon Loss of Pressure in Hose.

The influence of a slight difference in diameter of hose upon the loss of pressure incident to forcing a given number of gallons per minute through it, is far greater than is generally appreciated. The magnitude

of the corrections to offset variation in diameter given in column 5 of Table No. 6 will very likely excite comment and question. That the rule by which they are computed, when applied within the limits of these experiments (on hose of 2½-inch nominal diameter), is correct within about 1 per cent., is indicated by the experiments of Darcy and of Hamilton Smith, Jr.; and that, notwithstanding the slight constriction at hose coupling, the rule is practically correct and reliable as used in the table, is proved by the comparison of the last two experiments in the table with those on Samples *D* and *L*.

It will be seen that among the dozen pieces of "2½-inch" hose experimented upon, the actual internal diameter varied from a little less than 2½ to a little more than 2¾ inches diameter. Since ⅛ of an inch variation from 2½-inch diameter may cause a variation of about 25 per cent. in friction loss in hose with same number of gallons per minute flowing, it is seen how essential it is that we know the diameter accurately; and since one manufacturer can make 2¾-inch hose as well as another, if he starts with that object in view, it would be unfair to place the 2¾-inch hose of one against the 2½-inch hose of another, just because the bore of the couplings happened to be the same in the two cases, without allowing for the difference of diameter.

So far as I can learn, previous experimenters have not measured with precision and under pressure the diameter of the hose upon which they experimented, and therefore their results are devoid of much scientific value, although they have unquestionably been of much value in the rough-and-ready, practical way.

In the present case the interior diameter of each line of hose under experiment was measured in half a dozen or more places by calipering the external diameter at these places, while the 1⅛-inch nozzle was attached and the stream flowing under full city pressure. Immediately afterward the water was shut out, the hose at the points just calipered was flattened between clamps and blocks with a pressure intended to be about 25 pounds per square inch and again calipered, and the thickness of the fabric thus determined; which, being deducted from the external, gave the internal diameter. The hose diameters thus determined were verified in several cases by independent remeasurement in the same manner, and by comparison with diameters of plaster casts from the same hose. Considering the general agreement of these measurements, and the care used in making them, I conclude that the mean diam-

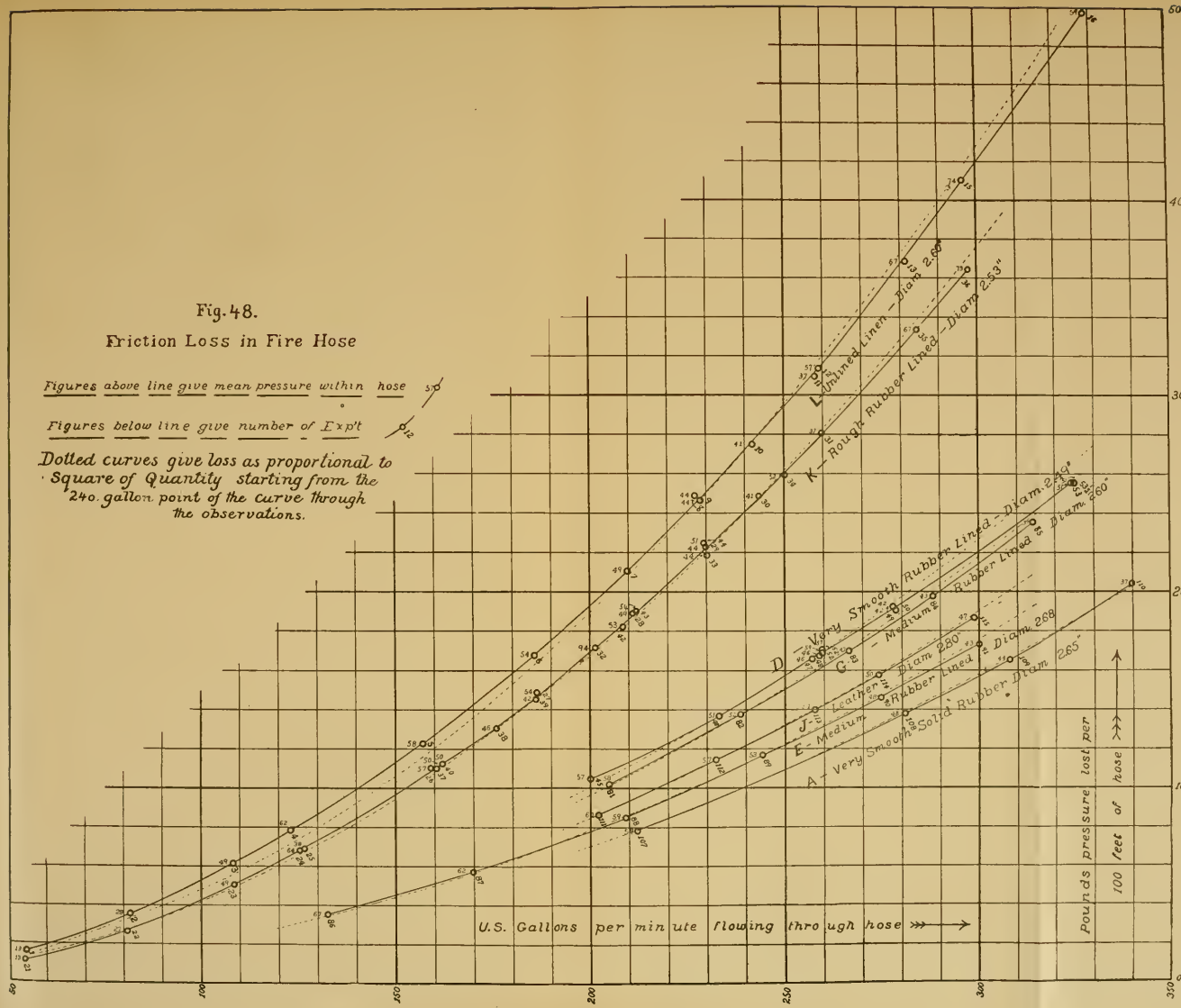


Fig. 48.

Friction Loss in Fire Hose

eters as given in the table are in almost every case probably correct within 0.01 or 0.02 inch.

(An attempt was made to determine the diameter by weighing the water contained in the hose; but this method, by reason of air bubbles and of water adhering to hose, was found less satisfactory than that just described.)

Influence of Pressure on Diameter of Hose.

It might be thought that the elasticity of the hose, acting in accordance with the varying pressures of water within it, would affect seriously the rate of loss of pressure, and thus render the results of little value in indicating laws of flow in pipes under high velocities.

To determine whether or not this was the case, the following experiments were made. The ends of a line 100 feet long of each kind of hose were capped, and then the diameter measured in half a dozen places, first with a pressure of 25, then with 50, then 75, and finally 100 pounds per square inch. The hose was meanwhile free to elongate. The average amount of expansion found is given in the following table:

Effect of Pressure in Enlarging Diameter of Hose.

Kind of Hose and Maker's Name. (All Nominally 2½ inches Diameter.)	Increase of diameter corresponding to 50 pounds increase of pressure. Inches.
Sample **C**, Boston Woven Hose Co.'s woven cotton rubber-lined "Boston Fire Jacket"	0.000
Sample **L**, Ross, Turner & Co.'s woven linen hose, unlined, "Red Line"	0.005
Sample **K**, Boston Woven Hose Co.'s woven cotton rubber-lined "Mill"	0.016
Sample **G**, Cornelius Callahan Co.'s knit cotton rubber-lined "Jacket"	0.022
Sample **E**, N. Y. Belting & Packing Co.'s knit cotton rubber-lined "Cable No. 2"	0.028
Sample **B**, B. W. H. Co., solid rubber, "Volunteer"	0.030
" C. C. Co., knit cotton rubber-lined "Mill"	0.036
" **A**, B. W. H. Co., solid rubber, "Extra"	0.036
" **I**, C. C. Co., knit cotton rubber-lined "Volunteer"	0.041
Sample **J**, Samuel Eastman & Co.'s leather hose	0.054

From the expansion of diameter of hose due to an increase of pressure within it as just determined, we may readily deduce the effect that this increase of diameter would have in decreasing frictional resistance due to flow, according to the well known law that this varies inversely as fifth power of diameter. We thus deduce that within the range of the experiments an increase of 10 pounds in mean pressure will by increasing diameter reduce the friction loss as follows :

	Per cent.		Per cent.
Sample	0.0	Sample B.....	1.2
" L.....	0.2	" H.....	1.4
" K.....	0.6	" A.....	1.4
" G.....	0.9	" I.....	1.6
" E.....	1.1	" J.....	2.2

Now let us compare these deductions with actual results of experiments upon friction loss under very different pressures, as given in Table No. 5.

Kind of Hose.	Experiments Compared.				Difference in Pressure.	Actual difference in Friction Loss by Experiment.	Loss as deduced from increase of diameter.
	No. of Experiment.	Mean Pressure.	No. of Experiment.	Mean Pressure.			
Sample L.....	11	37	12	57	20	0 per cent.	0.4 per cent.
Sample K.....	{27 28}	51	32	94	43	1½ "	2.6 "
Sample H.....	{63 64}	54	68	93	39	4½ "	5.5 "
Sample H.....	{66 67}	40	70	59	19	2 "	2.7 "

It may be remarked that a high pressure might possibly tend also to increase the friction loss by making the interior of a rubber hose slightly more corrugated by compressing or forcing the rubber more into the crevices between the threads, and thus offset to a greater or less extent the effect of the gain in diameter.

The agreement in the above table is, I think, very satisfactory, and not only gives confidence that the series of observations in Sample *K* and also *C* and *D* are reliable as pipe experiments under high velocities, but also shows that experiments on nearly all the other samples may be safely so used by applying a slight correction to reduce to constant diameter.

Data Furnished by these Experiments for Determining Laws of Flow in Pipes.

Let us insert here a word of explanation.

If there were now on record series of accurate experiments on flow in rigid pipes, fairly complete in their range of variation in diameter, velocity and smoothness of surface, and in which experiments but one variable had been changed at a time, thus making the result strictly determinate, I might cut this part of the discussion of these experiments very short; but, considering the unfilled gaps at present existing in our lines of data, may we not find it worth while to get all we can out of these experiments, even though they be on fire hose?

The writer believes a comparative study to show that the observations and methods of these experiments on hose as described above give results which, far as they go, equal in accuracy the pipe experiments of Darcy or other published standard experiments on flow in pipes.

Moreover, the results stated on the preceding page indicate that the flexibility and elasticity of some of these pipes do not affect the flow so as to make results essentially different from those which would be found with rigid pipes of same size and smoothness.

A feature of these experiments which might, at first sight, make them appear undesirable for this purpose, is the slight constriction at coupling; but it will be noted that for several samples this was very slight.

See also in this connection the results of experiments upon effect of bushings described a few pages farther on. These, bearing in mind that the bushings had sharp corners, while the reduction by couplings was somewhat gradual, indicate that the total effect of the existing reduction of area at coupling would increase the loss, even with sample *I*, which had the greatest difference in areas of any sample, by not more than 2 or 3 per cent.

In considering the bearing of these fire hose experiments upon the laws of flow of water in pipes, the point of special interest is:

Influence of Character of Surface upon Flow.

Reference has already been made to this and to photographs of plaster casts. But may we not here emphasize the necessity of a clear and definite description of the smoothness of the wetted surface, in order

that experiments on flow in pipes should have an exact value? Is it not useless to attempt to take a few isolated experiments upon one pipe here and another pipe there, and from such experiments to deduce the influence of velocity or diameter upon co-efficient of flow, when almost invisible differences of smoothness of surface may far outweigh these elements? Here is a good place to emphasize the fact that almost all recorded experiments upon flow in pipes of any considerable degree of roughness are rendered very vague and of little practical value because this all-important element of roughness has been so vaguely described.

Take Darcy's experiments; we often hear it stated that he found that in pipes covered with deposits the loss of head was double that in smooth pipes. How great was the roughness of these deposits? Were they as big as heads of steam boiler rivets like the tubercles which we often find in water pipes in New England cities, or were they of the size of grains of wheat? How can we apply such data in any but the most vague manner until we have such facts clearly stated?

Another point of interest is the

Influence of High as Compared with that of Moderate Velocities upon Co-efficient of Flow.

We will preface this with the remark that the question just now under discussion may, perhaps, be more clearly stated thus:

For a given pipe, does the loss of head vary in exact proportion to the square of the quantity flowing?

The answer may also be had in simple form by reference to Fig. 48. The full lines show the experimental result, the dotted lines are geometrical curves with head varying as the square of quantity and passing through the 240-gallon point of experimental curve. It is seen that the head does vary very nearly as the square of velocity; but that there is a divergence from the law, which, though small, is clearly defined for each of the curves. For *A*, *E*, *G* and *J* the divergence of the curves would be in more marked agreement with those for *L*, *K* and *D*, if the slight corrections for expansion of those samples of hose by pressure had been applied to the results before plotting.

In making the more complete comparisons we will use the well known Chezy formula as a basis. I think Fig. 47 illustrates sufficiently well that even slight differences in smoothness of surface, which are in most cases hidden from observation, may easily offset the refinements of

more complicated formulas, and for readily comparing the results with other experiments on flow in pipes, we will follow the line of investigation given in Hamilton Smith, Jr.'s "Hydraulics," edition 1886, page 213 *et seq.*, because of the complete and convenient collection of data included in his work; a collection admirable for the care with which questionable experiments have been excluded.

We compute the following from the data furnished by the experiments as detailed in Tables Nos. 5 and 6; taking off values for quantity and friction loss from a large scale plotting of the data just mentioned, similar to Fig. 48. For those kinds of hose in which loss was shown by experiments detailed on page 353, to be appreciably affected by expansion (viz., all samples except *C*, *D* and *L*), a slight correction (in no case greater than 2.4 per cent.) has been applied as per deductions of page 354. The results are shown graphically in Fig. 49.

TABLE No. 7.

VALUE OF CO-EFFICIENT n IN FORMULA $v = n\ (rs)^{\frac{1}{2}}$ FOUND TO EXIST IN FIRE HOSE OF VARIOUS DEGREES OF ROUGHNESS AND FOR VARIOUS HIGH VELOCITIES.

Chezy formula $v = n\ (rs)^{\frac{1}{2}}\ \therefore\ n = \frac{v}{\sqrt{rs}}$

v = Mean velocity in feet per second.
n = Co-efficient for mean velocity of flow.
r = Hydraulic mean radius = $\frac{1}{4}$ diameter in feet.
s = Natural sine of hydraulic gradient.

See Fig. 49.

Sample or kind of Hose.	Diameter of Hose. Inches.	Gallons per Minute Flowing.	Pounds Lost per 100 feet per Plotted Curve.	Correction for Expansion of Hose by Pressure.	Pounds Lost per 100 feet. Corrected for Expansion.	s.	Mean Velocity in Hose. Feet per Second.	n.
				Per cent.				
A.	2.65	214.9	8.00	+0.8	8.06	.1863	12.50	123.3
	"	240.	10.00	0	10.00	.2311	13.96	123.6
	"	292.2	14.89	—1.1	14.73	.3404	17.00	124.
	"	343.8	20.90	—2.4	20.40	.4714	20.00	124.0
C.	2.47	200.1	10.66	0	10.66	.2464	13.40	119.1
	"	240.	15.00	0	15.00	.3467	16.07	120.4
	"	298.7	22.8	0	22.80	.5269	20.00	121.5
D.	2.49	200.3	10.50	0	10.50	.2427	13.20	117.7
	"	240.	14.50	0	14.50	.3351	15.81	120.
	"	288.4	20.48	0	20.48	.4733	19.00	121.3
	"	318.7	24.70	0	24.70	.5708	21.00	122.1
E.	2.68	131.9	3.45	+1.5	3.50	.0809	7.50	111.6
	"	175.8	6.17	+1.	6.23	.1440	10.00	111.6
	"	240.	11.3	0	11.3	.2611	13.65	112.3
	"	298.9	17.20	—1.1	17.01	.3931	17.00	114.8
I.	2.69	203.7	10.10	+1.	10.20	.2357	11.50	100.1
	"	240.	13.50	0	13.50	.3120	13.55	101.7
	"	283.4	19.11	—0.8	18.96	.4382	16.00	102.2
	"	318.9	22.90	—2.4	22.35	.5165	18.00	105.8
M.	2.12	154.0	14.80	+0.8	14.92	.3448	14.00	113.4
	"	187.0	21.07	+0.6	21.19	.4897	17.00	114.8
	"	240.0	33.20	0	33.20	.7673	21.81	118.4
K.	2.53	54.8	1.11	+1.6	1.13	.0261	3.50	94.3
	"	78.4	2.50	+0.8	2.52	.0582	5.00	90.3
	"	117.5	5.99	0	5.99	.1384	7.50	87.8
	"	156.7	10.55	+0.5	10.60	.2450	10.00	88.0
	"	188.0	14.91	—0.6	14.82	.3425	12.00	89.3
	"	240.	24.10	0	24.10	.5570	15.31	89.3
	"	266.4	29.38	—0.6	29.20	.6748	17.00	90.1
	"	297.7	36.42	—1.8	35.76	.8264	19.00	91.0
L.	2.60	57.9	1.79	0	1.79	.0414	3.50	73.9
	"	82.7	3.63	0	3.63	.0839	5 00	74.1
	"	124.1	7.99	0	7.99	.1846	7.50	74.9
	"	165.5	13.50	0	13.50	.3120	10.00	76.9
	"	240.	27.20	0	27.20	.6286	14.50	78.5
	"	281.3	37.03	0	37.03	.8558	17.00	78.9
	"	331.0	50.30	0	50.30	1.1624	20.00	79.6

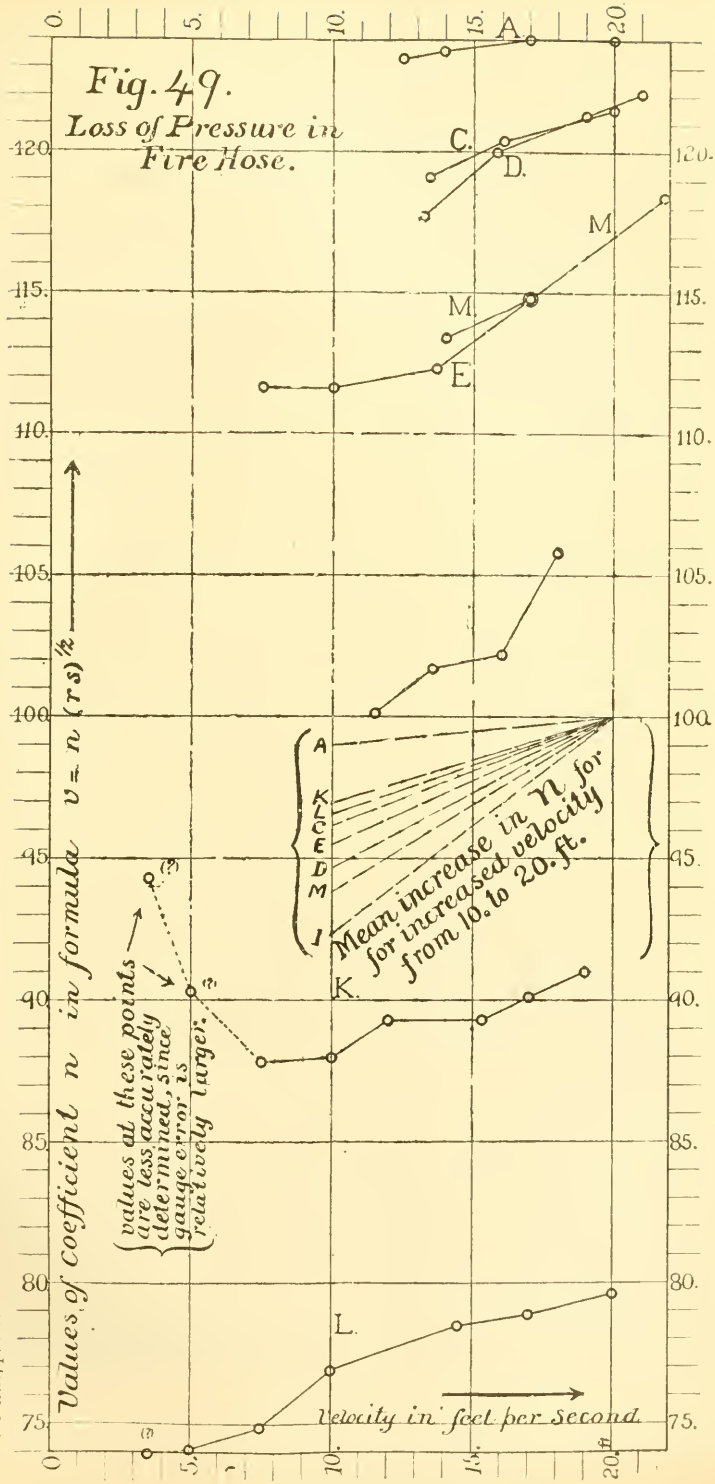


Fig. 49.
Loss of Pressure in Fire Hose.
Values of coefficient n in formula v = n (rs)^½
Velocity in feet per second
Mean increase in n for increased velocity from 10. to 20. ft.
values at these points are less accurately determined, since gauge error is relatively larger.
A
C.
D.
M.
M.
E
K.
L.
0.
5.
10.
15.
20.
120.
115.
110.
105.
100.
95.
90.
85.
80.
75.

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The question may be asked, How do results obtained by previous experimenters on friction loss in hose agree with those just described? So far as I know, the only published results on friction loss in fire-hose making pretension to scientific accuracy are those of G. A. Ellis, previously referred to, and those of E. B. Weston. There are records of a few rough experiments in Fanning's "Water Supply Engineering," page 519, but the effect of hose friction is so influenced by several other losses of pressure as to be indeterminate. The Ellis tables have already been shown (see pages 327 and 334) to estimate discharge so wrongly as to make the figures for friction loss of very doubtful value, and, moreover, no statements are given, either of the actual diameter, or as to whether the so-called "rubber hose" was cotton fabric hose with a thin rubber lining, or was "solid rubber hose." Mr. Weston's experiments were evidently much more accurate than those of Ellis. Mr. Weston informs me that he experimented upon only a single kind of hose, namely, the so-called "solid rubber" hose, and so far as I could judge from appearance of some specimens cut from it, the character of the interior was not very different from my Sample *B*. He cut several rings from some of the hose experimented upon, and found their diameter, when not stretched by pressure, to average 2.50 inches. Lack of agreement in the results which he found with different lengths of hose indicates that the different sections used differed in diameter or surface, and it is very likely that the hose was expanded to a slightly larger diameter by the water pressure when in use than existed when diameter was measured. His hose couplings were of but 2¼ inches diameter. However, the comparison stands as follows, for 240 gallons per minute flowing:

Deducing the friction loss from those experiments of Mr. Weston in which the flow was nearest to 240 gallons per minute, namely, the experiments which he numbers 11 and 22.

PRESSURE LOST BY FRICTION PER 100 FEET IN LENGTH.

	Pounds.
Per Weston's experiments on piece 96 feet long	18.3
" " " " 385 " "	14.9
Average value	16.6
Per Freeman's experiments on Sample B reduced to 2½-inch diam.	14.0
Difference	2.0

One-half pound of the above difference may be attributed to the reduction of area in the 2¼-inch coupling used by Mr. Weston.

FIG. 50.

The plate (Fig. 50) shows the site of experiments upon friction loss in hose. This view illustrates sinuosity caused by elongation of hose under pressure.

In the ordinary experiments hose was carefully straightened out after admitting pressure.

Effect of Sinuosity upon Friction Loss.

The experiments on this subject were less complete than desirable, but serve the practical purpose of showing that ordinary sinuosity does not very materially interfere with flow. Almost every kind of fire-hose elongates to a greater or less degree when subjected to water pressure. The amount of this elongation for the several kinds under experiment was determined at the same time as the expansibility (see page 353); and was also repeatedly determined in the course of the experiments on friction loss by laying the empty hose in a straight line previous to the experiment and taking its length, then, after turning on the water pressure and straightening out the sinuosity produced by the stretching, its length was again measured. The mean values for the stretch or elongation of the various kinds of hose are given in Table No. 6.

The natural result of this elongation is, that though a line of empty hose be laid perfectly straight, it will become very sinuous as soon as the water is admitted. This is illustrated in Fig. 50. To determine the effect of this natural sinuosity upon the flow, an experiment upon the friction loss was made while the hose was lying as shown in Fig. 50, and immediately afterward the hose was straightened with much care to conform to a straight line previously run out by the aid of a transit and marked on the platform by a stripe of white paint.

From data given in Tables 5 and 6 we obtain the following results:

Kind of Hose.	Per cent. of elongation of hose due to 50 pounds increase of pressure.	Mean of average pressure for whole length of hose during the experiments.	No. of experiments.	Gallons per Minute flowing in hose.	Mean Velocity in hose.	Ratio of Increased loss per hundred feet, due Sinuosity to head due Velocity.	Per cent. increased loss of head with Sinuosity over loss with hose straight.	Increased loss for flow of 240 gallons per minute. Pounds per 100 feet.
	Per cent.			From To Mean.			Per cent.	
Sample L..	2½	52	4	120—224—178	10.7	1.3	6.2	1.7
" K..	5	50	1	— — 161	10.3	0.8	5	1.2
" H..	3	54	1	— — 186	11.0	0.2	2	.3
" E..	3½	46	3	204—339—226	12.9	0.6	6.2	.7
" D*.	.....	.....	..	.....	17.0	0.9	8	.8
" A*.	... .	.....	..	.....	17.7	0.7	8	1.1

Average 1.0

* The values for these two samples are deduced from the curve experiments on curve *Z* described on the three following pages.

The above justifies us in saying, in a rough and ready way, that for fire hose in general the friction loss in hose laid in its ordinary sinuous course, but not cramped or kinked, will be somewhere about 6 per cent. greater than if laid perfectly straight. Or stated in another form, that for the standard 240 gallon stream, this sinuosity will add about 1.0 pound to the loss in the hose per 100 feet.

EXPERIMENTS ON EFFECT OF CIRCULAR CURVES OF FROM 2 TO 4 FT. RADIUS.

It was thought that, by reason of flexibility of hose, some interesting information relative to general effect under high velocities of bends of large radius could be obtained; but although the experiment was made with such precautions as would seem to prevent possibility of serious error, the results are not conclusive in determining any law of general application. They are, however, deemed worth putting on record.

A set of experiments was made on September 6th with 2½-inch hose, Sample H, but as the results were unsatisfactory, another set of experiments was tried on September 24th, this time selecting hose from Sample D, on account of its uniformity of diameter at couplings, lack of change in diameter under varying pressures, and general excellence of finish. The section of hose used measured 57.2 feet in length under pressure of 59 pounds, and the piezometer couplings (See Fig. 2) were attached directly to its two ends, there being about 50 feet of hose from Sample A, between the up-stream piezometer and the hydrant, and about 50 feet of hose (A) between the down-stream piezometer and the discharging nozzle. To enable the piezometric comparisons to be made with the minimum of error the two mercury gauges were set up side by side midway of the hose, and they were thus both observed almost simultaneously by one observer. Rubber tubes each about 29 feet long were thus necessary to reach from the gauge to the piezometer; but as the vertical rise of these tubes was very small, had air bubbles existed in the tubes they would have caused but almost inappreciable error. Much care was taken, however, to prevent the existence of air bubbles, by blowing off the tubes through cock at gauge before each experiment. To prevent so far as possible cross-currents from interfering with accuracy of piezometers, that portion of the hose within at least 5 feet of each piezometer was carefully straightened before each experiment. The discharge was in each case measured in the tank as already described; and there is no reason to think that error in determination of rate of discharge exceeded ½ of 1 per cent., and no reason to think that error in the comparative readings of the two pressure gauges could have been greater than 0.10 pound. The results are given in the following table:

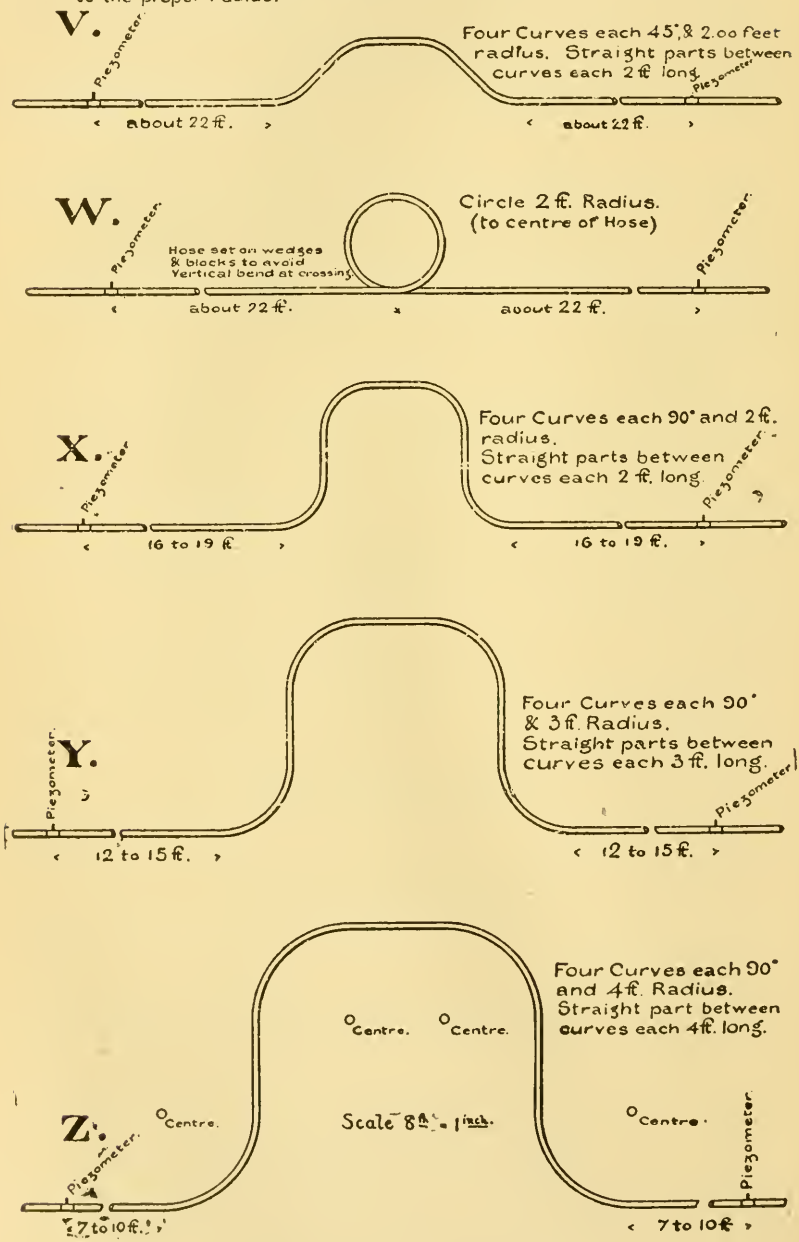
TABLE No. 8.—Experiments on Resistance due Curves.

Time. H. M.	Quantity flowing measured in Tank, gallons per minute.	Observed loss of pressure between piezometers, pounds.	Mean flow to which observations are reduced, gallons per minute.	Mean Velocity in hose, feet per second.	Loss that would have existed with mean flow, pounds.	Increased resistance due curve, pounds.	Ratio of loss due whole curve to head due velocity in hose.	
	Hose like	Sample *D*.	Diameter 2.49	inches.	Length 57.2	feet.		
September 24th.								
2 17	259.6	9.65	258.0	17.0	9.57	.....	.....	Hose straight at beginning of Expts.
6 14	259.5	9.73						" " " end " "
6 24	294.1	12.30	293.0	19.3	12.22	.....	.....	" " " end " "
6 26	324.3	14.62	323.0	21.3	14.57	.....	.....	" " " beginning " "
6 38	325.0	14.77						" " " end " "
3 16	258.1	10.05	258	.....	10.04	0.47	.24	Hose bent around four 45 degree curves of 2 feet radius. See *V*, Fig. 51.
3 24	293.2	12.68	293	.....	12.66	.44	.17	
3 33	323.5	15.22	323	.....	15.18	.61	.20	
3 01	257.3	10.08	258	.....	10.13	0.56	.29	Hose bent around one full circle of 2 0 feet radius. See *W*, Fig. 51.
2 52	293.4	12.93	293	.....	12.90	.68	.27	
2 43	323.7	15.46	323	.....	15.40	.83	.27	
4 40	257.6	10.20	258	.....	10.23	.66	.34	Hose bent around four arcs of 90 degrees of 2 feet radius. See *X*, Fig. 51.
4 25	292.7	12.94	293	.....	12.96	.74	.29	
4 11	324.3	15.43	323	.....	15.33	.76	.25	
6 05	257.9	10.32	258	.....	10.33	.76	.39	Hose bent around four arcs of 90 degrees of 3 feet radius. See *Y*, Fig 51.
5 55	293.7	13.00	293	.....	12.95	.73	.29	
5 48	322.6	15.59	323	.....	15.62	1.05	.24	
5 23	257.9	10.42	258	.....	10.43	.86	.44	Hose bent around four arcs of 90 degrees of 4 feet radius. See *Z*, Fig. 51.
5 32	292.3	13.14	293	.....	13.19	.97	.39	
5 40	322.5	15.69	323	.....	15.73	1.15	.38	
	Similar experiments with hose from Sample *A*. Diameter 2.64 inches.							
September 25th.								
2 52	305.1	8.62	304	17.7	8.55	.....	.....	Hose straight at beginning. Length, 51.8 feet.
4 47	305.4	8.63	304	.....	8.55	.....	.....	Hose straight at end.
4 24	304.3	9.06	304	.....	9.04	.49	.23	Hose bent around four 90 degree arcs of 2 feet radius. See *X*.
4 38	304.2	9.29	304	.....	9.28	.73	.35	Hose bent around four 90 degree arcs of 3 feet radius. See *D*, *Y*.
4 09	302.7	9.24	304	.....	9.32	.77	.37	Hose bent around four 90 degree arcs of 4 feet radius. See *E*, *Z*.

Resistance due Curves in Fire Hose. **Fig. 51.**

Form of curves Experimented upon

Hose laid on flat floor and held in place by form jig-sawed from 3 inch plank to the proper radius.



It will be noted that the sequence of the experiments with different radii was made irregular, in order to break up any periodic error, and that agreement of the repetitions indicates that any accidental distortion of the hose could have influenced the result but little.

The several curves experimented upon were made nearly midway between the piezometers and were of the form shown in Fig. 51.

Excepting *V*, the total curvature was in each case 360 degrees. Between each of the curves was straight tangent of length equal to radius of hose. The hose was laid on a flat floor and held accurately in place by pieces of 3-inch plank jig-sawed to the proper radius, and much care was observed in placing it in order that it might not be wrinkled on the inner side or flattened on the outer side of curve. This was attained by bending and placing it under full pressure. Careful calipering indicated that it retained its cylindrical form. Two feet radius was about as sharply as it could be bent, however, and retain this smooth cylindrical form. The difference in pressure at the two piezometers was first measured with the hose lying straight; then, without shutting off the flow or disturbing the pressure within the hose, the hose was bent around some one of the curves and the loss of pressure between the piezometers again measured. Gauges were read at intervals of fifteen seconds and each value as here given is thus the mean of about fifteen observations. The water pressure was fairly steady, the extreme range in the course of the fifteen observations seldom if ever being more than $\frac{1}{2}$ pound, and generally not more than $\frac{2}{10}$ pound.

As the results came out different from what were expected, some further experiments were made next day with another kind of hose, namely, Sample *A*, a remarkably smoothly finished piece of solid rubber hose, the interior of which was almost as smooth as lead pipe.

The above results are remarkable, and it is to be regretted that there was no means of extending the series down to shorter radii, or of investigating that loss due to the bending which may perhaps have continued within the straight pipe down stream from the end of curve.

It seems pretty clearly proved that within these limits there was more loss with the large radius than with the small. This same condition was indicated by the experiments of September 14th, under somewhat different conditions. Perhaps the reason for this is that the arc was longer and thus there was a greater length of pipe in which the current was disturbed.

Shall the Standard Diameter for Fire Hose be Increased?

To illustrate the effect of diameter of hose upon the loss of pressure by friction the following table is presented. This is computed by the well established law that loss of pressure varies inversely as the fifth power of diameter:

For any definite number of gallons per minute flowing, the loss of pressure due friction per 100 feet of hose, whose diameter is		3	inches is only		40	per cent.	as much as in hose of the same smoothness, whose diameter is exactly 2½ inches.	
do	do	2⅞	"	"	50	"	do	do
do	do	2¾	"	"	62	"	do	do
do	do	2⅝	"	"	78	"	do	do
do	do	2⅞	"	"	29	"	more than in hose of the same smoothness, whose diameter is exactly 2½ inches.	
do	do	2¼	"	"	70	"	do	do
do	do	2⅛	"	"	2.	times as much as in 2½-inch hose.		
do	do	2.	"	"	3.05	"	"	"

This table is well worthy of study by makers of hose.

By way of further illustrating the practical bearing of this question of diameter, we may say that if the Boston Woven Hose Company, for instance, should increase the diameter of hose like sample *D*, in Table No. 6, on page 348, by $\frac{3}{16}$ inch, thus making it same size as sample *E*, the observed friction loss for each hundred feet of length would be 10 pounds instead of 14.5 pounds, as found, or a saving of 31 per cent. of power now wasted.

And as a further illustration we may venture the suggestion that if some enterprising firemen, who earnestly desired the prize for distance playing at a firemen's muster, should have some hose smooth as sample *C* or *D* (see Fig. 47), manufactured with internal diameter of 2¾ inches instead of 2½, but fitted with the ordinary 2½-inch coupling, this difference in diameter, though so small as to be almost unnoticeable, especially if hose were, like sample *D*, made without a jacket, would, with the customary 250 feet of hose and a 1⅛-inch stream, make more difference in the distance reached than would any fancy nozzle; and, in comparison with hose of the same smoothness, 2½ inches in diameter, would, with the same pressure at engine, throw the extreme drops about 15 or 20 feet farther. With greater length of hose than 250 feet the difference would be very much more marked.

From strong practical reasons, this slightly increasing diameter of such brands of cotton rubber-lined hose, as are now made exactly 2½ inches diameter, is well worthy consideration, especially by the makers

of woven hose. (In knit hose, solid rubber hose and leather hose, a somewhat increased size is at present attained by the natural expansion under pressure, as shown on page 353.)

The 2½-inch standard was first adopted years ago, before pumps or steam fire engines had attained their present perfection, delivery or capacity, and before the costly experience gained in large mill fires, as well as in the large fires of Portland, Chicago and Boston, had led to a general increase in size of streams thrown at serious fires.

Costly experience has shown that in serious fires a small stream is evaporated as it falls in spray through the flames, while if a large stream is thrown enough may escape the evaporation to pass through the heated gases and reach the burning coals themselves. Streams large in volume are thus a necessity.

In a well designed and first-class, modern, mill-hydrant system, lines of hose more than 250 feet in length will not, as a general thing, be necessary; and the friction loss in hose is thus in one sense of less importance there than in much of the work of a city fire department, where lines upward of 500 feet in length are often used.

Considering the needs of city departments, the writer believes it would be of much practical advantage for ordinary fire hose to be at least of 2¾ inches diameter; this to be fitted up for the present, however, or until favorable experience fully proves the advantage of a general change, with a coupling of 2½ inches bore, so as to be perfectly interchangeable with hose at present in use. Such a hose would, with the same pressure at hydrant or steamer, give a stream at the end of 1 000 feet of hose, equally large and strong as would be thrown at the end of a line about 650 feet long of 2½ inches diameter.

The only objections to its use are, first, the increased weight to handle. This cannot be considered insurmountable, in view of the fact that much knit hose now in use is, when expanded by pressure, of nearly this size, and is handled without difficulty. Moreover, a length or two of light unjacketed 2½-inch hose could be used next nozzle for inside work or work on ladders. The second objection is the reduced bursting strength due the larger diameter. This, with the fabric remaining just the same, would amount to only 10 per cent., and any fabric now standing 500 pounds on 2½ inches diameter would stand 450 pounds on the 2¾ inches diameter. In fact, the best 2½-inch jacket fabrics of to-day are plenty strong enough to stand a 3-inch diameter, and I incline to the

opinion that it may be best not to stop content with the 2¾-inch diameter, but to go far enough while we are about it and adopt a 3-inch diameter as the standard for fire hose.* Before going too far in this recommendation I should like to see the result of an outfit of such hose in the fire department of some large city for six months, and learn if the men found it too heavy, or found practical difficulty in its manipulation. Ultimately a standard 3-inch coupling should be adopted, and somebody, preferably the National Association of Fire Engineers, should decide on an appropriate thread, which should be standard and universal from Montreal to Mexico. There are now upward of 200 different "standard" threads † in use for 2½-inch hose, and this sad confusion has many times led to serious damage and delay, and more than once rendered aid summoned from neighboring towns during a serious conflagration of little avail. While the matter is in a tentative state I would advise using this 3-inch hose on the coupling of 2½ inches bore, in order that it might fit ordinary hydrants and engines and be perfectly interchangeable with the old standard hose. The couplings should, however, be designed and made with care, avoiding a square shoulder where diameter is reduced from 3 to 2½, and making this reduction or enlargement in every case by long, easy, round cornered taper.

If the reduction of diameter is made in this manner, the loss by using the 2½-inch interchangeable coupling will be much less than would at first thought be expected, and the loss of pressure by such a coupling at each fifty feet would be less than 5 per cent. of the pressure saved by the larger size of hose.

Experiments on Effect of Reduction of Diameter at Hose Coupling.

A few experiments were made to see what extra friction loss would be caused by couplings of 2¼ inches bore on 2½-inch hose. The old-fashioned tail couplings for 2½-inch hose had a waterway of but 2¼ inches in diameter. These were the kind used in Weston's experiments, and it was desirable to know how far this constriction of waterway modified his results.

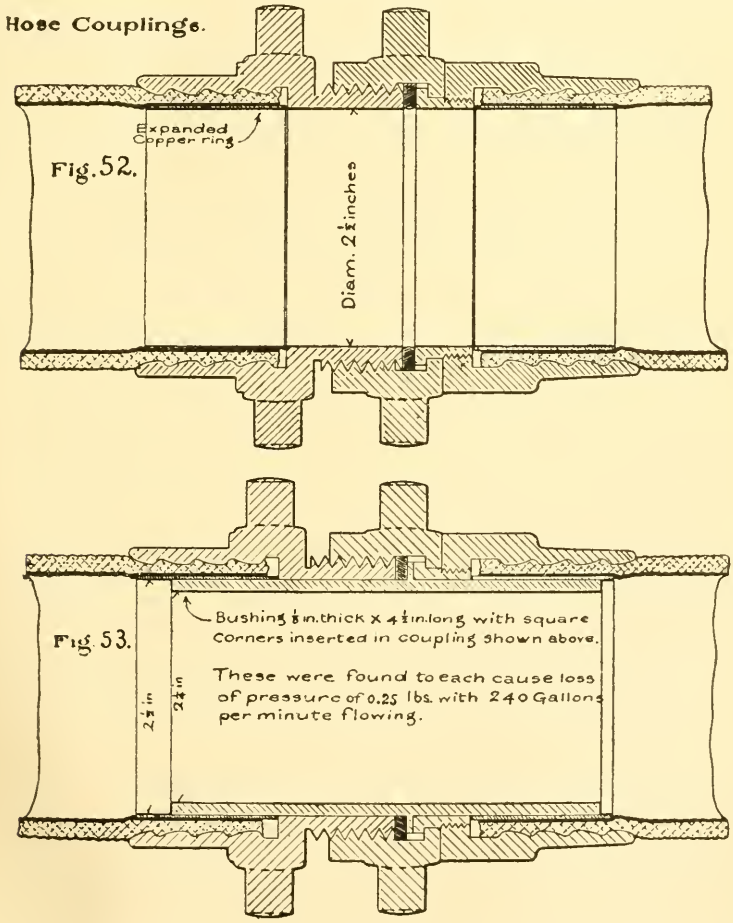
These experiments were made by taking a line of 2½-inch hose about

* The above was written about a year ago and submitted to Mr. Reed, Tressurer of the Eureka Fire Hose Company of New York, and others.

I am pleased to note that the Eureka Company have acted on this matter and have already equipped several departments with trial outfits of 3-inch hose, and from them learn that the result so far is most encouraging.

† The Chapman Valve Company have gauges for 200 different patterns of 2½-inch hose-coupling threads in regular use in various cities and towns of the United States. And A. J. Morse & Son, of Boston, make 2½-inch hose-couplings to upward of sixty different gauges.

300 feet in length, having full size $2\frac{1}{2}$-inch couplings, and after having carefully determined, by the methods already described, the loss of pressure in its length for certain rates of flow, water was shut off, and the joints between the six 50-foot sections of hose uncoupled, and bushings consisting of tubes $4\frac{6}{10}$ inches long, with $2\frac{1}{4}$ inches diameter of waterway and square ends, were inserted at each of these five joints. The joints were then coupled up again, and the loss of pressure determined for the same rates of flow as before. One of the couplings without the bushing is shown in Fig. 52, and a coupling containing a bushing is shown in Fig. 53.



Two entirely independent sets of experiments were made, one set with solid rubber hose (sample B), and the other with cotton rubber lined hose (sample H).

Date of experiments.....	September 5.	September 16.
Kind of hose.....	Solid rubber (B).	Rubber lined (H).
Average internal diameter of hose..	2.60 inches.	2.63 inches.
Average internal diameter of couplings.....	2.53 "	2.51 "
Average internal diameter of bushings.....	2.23 "	2.23 "
Number of independent experiments	4	3
Average number of gallons per minute flowing in hose.....	211.7	206.9
Average increased loss of head due inserting five bushings.....	1.01 pounds.	0.95 pounds.
Loss of head due each bushing*....	0.190 "	0.202 "

From the above experiments we deduce that with 240 gallons per minute flowing the effect of such a square shouldered reduction of waterway at hose coupling from 2½ to 2¼ inches would be to cause a loss of 0.25 pounds at each bushing, or 0.50 pounds additional loss per 100 feet of hose.

If up-stream end or entrance of bushing had been rounded and discharging end tapered out to full diameter, the loss due the reduction of area at couplings could have been made much smaller, or probably less than half the above amount.

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* These results agree very well with theory that loss by impact due a sudden reduction of velocity equals head due the difference of the velocities. (See Professor Swain's notes on Hydraulics, page 36, or Professor Merriman's Hydraulics, page 149.) Co-efficient contraction at upper end of bushing will be 0.83 (per Cox's Weisbach, page 841). Therefore, velocity in contracted portion is 20.95, and in passing to uncontracted portion of bushing velocity is there 17.39, from which sudden reduction of 3.56 feet in velocity there results a loss of 0.20 feet head; and again, in passing from bushing into hose, velocity is reduced to 12.79 feet per second, a reduction of 4.60 feet, for which the corresponding loss of head is 0.33 feet.

Thus, the total loss by this theory would be .33 + .20 = .53 feet, or 0.23 pounds, instead of 0.20, as actually found.

Loss of Head due to Reduction of Area of Hose by Washers at Coupling.

In coupling hose, use is frequently made of washers which are a little smaller than proper. As it took but little time to make, in course of the experiments already described, a few experiments on this subject, it was done. It is hardly worth while to describe the experiments in detail; but we may say that in three cases, immediately after the regular experiment in which washers of full bore of hose were used, these $2\frac{1}{2}$-inch washers were taken out and other washers with hole about $2\frac{1}{4}$ inches diameter (actually 2.28 inches) inserted in their place, and the friction loss through the hose with these small washers again determined. Then these $2\frac{1}{4}$-inch washers were in turn replaced by washers with 2.00-inch hole, and the experiment again repeated for two quite different velocities of flow. The washers were of a firm quality of rubber and about $\frac{3}{16}$-inch thick, evenly moulded, and with sharp corners, and probably retained nearly their original form during the experiment. For convenience of comparison the results are reduced to the basis of 240 gallons per minute (a first class $1\frac{1}{8}$ inch fire stream) flowing in hose, and average as follows:

Loss caused by insertion of Washers 2.28-inch Diameter in $2\frac{1}{2}$-inch Hose Coupling.

Sept.	4th.	Hose=Sample	*K.*	Average loss for each washer,			0.48	pound.
"	6th.	" "	*B.*	"	"	"	0.49	"
"	10th.	" "	*D.*	"	"	"	0.70	"
		Average.....					0.56	"

Loss caused by insertion of Washers 2 inches Diameter in $2\frac{1}{2}$-inch Hose Coupling.

Sept.	4th.	Hose=Sample	*K.*	Average loss for each washer,			2.61	pounds.
"	6th.	" "	*B.*	"	"	"	3.56	"
"	10th.	" "	*D.*	"	"	"	3.15	"
		Average.....					3.11	"

Under the ordinary theory that in constrictions of area like this the loss of pressure equals the head due the difference in velocities, and

assuming that the co-efficients of contraction found in experiments on ring nozzles apply here, we should expect for the

Washer with hole 2.28 inches diameter, a loss of..... 0.51 pounds.
" " " 2.00 " " " ..... 2.39 "

The actual loss is from 14 to 30 per cent. greater than this, but the agreement is satisfactory, in view of the yielding nature of the diaphragm. We compute, therefore (by adding 25 per cent. to theoretical loss as deduced above), that in hose with 2½-inch couplings the loss due use of washers smaller than hose will be as follows:

Inside Dimension of Washers.	For Flow of 200 Gallons per Minute.		240 Gallons per Minute.		300 Gallons per Minute.	
	Loss due each Washer.	Loss per 100 Feet of Hose.	Loss due each Washer.	Loss per 100 Feet of Hose.	Loss due each Washer.	Loss per 100 Feet of Hose.
With 2⅞-inch hole.....	0.10 lbs.	0.20 lbs.	0.15 lbs.	0.30 lbs.	0.23 lbs.	0.46 lbs.
" 2¼ " "	.52	1.04	.75	1.50	1.17	2.34
" 2⅛ " "	1.16	2.32	1.67	3.34	2.61	5.22
" 2 " "	2.08	4.16	2.99	5.98	4.67	9.34

Experiments on Jets.

The site of all the jet experiments recorded in Tables Nos. 9, 10 and 11 is shown in Fig. 54. The site of the experiments recorded in Table No. 12, for determining greatest horizontal distance that could be thrown with a given pressure, is shown in Fig. 55.

The experimental data previously on record for height reached by jets is very scanty indeed, and some of the writers apparently fail to recognize how few are the experiments on which existing tables are based. Thus it is stated in the most recently published treatise on hydraulics ("A Treatise on Hydraulics," by Mansfield Merriman, Wiley and Sons, 1889, page 132) that "the experiments of Ellis determine the height and range to which jets may be thrown," etc., and then follows a table for vertical jets, giving a synopsis of his conclusions. Now, in point of fact, Ellis says he made no experiments on vertical jets, but says these figures in his tables are from the published figures of J. Herbert Shedd, M. Am. Soc. C. E. Tracing them back a step further, we find Mr. Shedd made no such experiments; but that certain tables were prepared at his direction by E. B. Weston, M. Am. Soc. C. E., and by Charles H. Swan, M. Am. Soc. C. E. Subsequently Mr. Weston made a further study of the subject, and the data used and the conclusions reached were presented to this Society by Mr. Weston in a paper read September, 1884.

Mr. Weston, in turn, in preparing these tables made use of no experiments of his own on height reached by jets, but compiled and compared with the data given by Box a few additional experiments, viz., some experiments by J. Foster Flagg, M. Am. Soc. C. E., on an orifice which bore but little resemblance to a hose nozzle, and also some experiments by Mr. S. Van Cleve, in which the determination of the pressure was rendered uncertain by being measured at a point on a line of hose 50 feet distant from the nozzle, and which experiments show a good deal of variation among themselves. Finally Mr. Weston simply made use of the formula given in Box's Hydraulics. Thus it happens that the tables of Merriman, Ellis and Shedd for vertical jets merely repeat the table and formula in Box's Hydraulics without change (see pages 38 and 39, 6th Edition). Box, in presenting his table, says: "There are very few reliable experiments on this subject," and that "more experimental information is very desirable," and gives his formula as merely a tentative one.

High jets are a difficult subject for accurate experiment, and the result of my experiments shows that though Box's empirical formula is incorrect in some of its details, yet his general result is somewhere near correct within the range of sizes common for fire streams, and it is found to be much more accurate than would naturally be expected in view of the incompleteness of the data with which Mr. Box had to work.

This is illustrated by the following comparison :

Hydrostatic Pressure.	¾-inch Nozzle. Height of Jet by Box's Formula.	¾-inch Nozzle. Height of extreme Drops by Freeman's Experiments.	Error of Box Formula.	1¼-inch Nozzle per Box.	1¼-inch Nozzle per Freeman.	Error of Box Formula.
Feet. Pounds.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
57.7 = 25 0	50.8	49.4	+ 1.4	53 5	51.2	+ 2.3
100.0 = 43.3	79.0	82.5	— 3.5	87.5	88.0	— 0.5
115 5 = 50.0	87.7	92 3	— 4 6	98.8	101.2	— 2.4
161.6 = 70.0	107.3	113.5	— 6 2	129 0	134.8	— 5.8
200.0 = 86.6	116.7	127.0	— 10.3	150.0	150.7	— 0.7
230.9 = 100.0	120.0	133.2	— 13.2	164.4	158.9	+ 5.5

It will be seen, however, on examining the experiments presented below, and from the plotted curves of Fig. 56, that they plainly indicate that the loss of height due to resistance of air for jets of large diameter varies, according to a law, somewhat different from that implied by the Box formula.

Effect of Wind.

Since making these experiments on jets, I am disposed to regard with suspicion the high accuracy of any experiments on high jets which are not described with the fullest detail as to stillness of air. We were surprised at the extreme sensitiveness of height of jets at 40 pounds or more pressure, to even very faint wind. On three or four different days a slight breeze sprang up just as experiments on jets either vertical or at 75 or 60 degrees elevation were being commenced, and it was found that even a moderate summer afternoon zephyr would cut down the height of the extreme drops by fully 10 per cent. Thus, on September 15th, with 1¼-inch smooth nozzle set at 60 degrees elevation and under about 50 pounds water pressure, a breeze rather suddenly sprang up, blowing in direction of jet. It at once reduced height of the extreme drops from 82 to 67 feet. This was merely a "moderate to fresh" breeze, and from a station on the mill roof I very roughly estimated its velocity at from 8 to 12 miles per hour.

Again, on September 24th, when trying to experiment with a 2-inch jet from a Siamese pipe set at 60 degrees elevation and with about 50 pounds pressure on nozzle, I found that with a fresh breeze coming in occasional gusts in direction against the stream, the extreme height of the stream was 10 feet greater in the lulls than while wind was blowing.

On another occasion, when experimenting on the maximum horizontal distance that a jet could be thrown, it was found that a gentle breeze, apparently of about 6 feet velocity per second, blowing in direction opposite to jet reduced the distance attained by the extreme drops from 125 to 100 feet.

I had much difficulty in finding a day sufficiently calm for satisfactory experiments, and sometimes, when it would seem calm down on the ground, on going to the mill roof breeze enough would be found to interfere materially with accuracy.

For six weeks, while engaged on the other experiments recorded in this paper, we kept apparatus in condition so it could be quickly arranged for jet experiments, and we continually kept watch of the smoke from the neighboring tall factory chimneys, and also kept watch of a little flag which we hung near the position of summit of jet. Several times we started our jets and had to give up on account of effect of a slight wind being plainly evident. In the course of the six weeks we found only a very few brief periods when the air was calm, and that the experiments on jets now presented are in some respects less extended and complete than I desired, is due to the fact that these periods of still air were so very few and so brief.

So very marked was the effect of currents of air, as to leave no doubt that the remarkable records for distance-playing sometimes made at firemen's musters are due to some favoring flaw of wind rather than to any peculiar excellence of form of nozzle used.

The site and general arrangement of the apparatus is shown by Figs. 54 and 55 so clearly as to need but little further description. It is to be clearly understood, however, that in the final experiments upon which tables are based, only one nozzle was used at a time. The two views just mentioned both illustrate experiments for comparing the relative merits of two nozzles, and when thus using two nozzles near together, there was apparently a slight and equal increase in the distance reached by both streams, due to the air currents set in motion by the jets themselves.

VERTICAL JETS AND JETS AT ELEVATIONS OF 60 AND 75 DEGREES.

As may be judged from an inspection of Fig. 54, the facilities for accurate observation were of unusual excellence, by reason of the convenient position of the roofs of these very large and high mills.

The writer stood generally upon the Washington Mill roof, and from this position noted the height of jet; occasionally he stood on the Pacific roof, and from that point noted the character of jet as to fineness of drops, etc., which from that station could be seen with great distinctness; but as the two mills were only about 50 feet apart, the jet could also be observed with great distinctness from the Washington roof.

As there were windows at each story in the end of the Washington Mill, observations upon jets at a less height were also readily made. The joints of the brick-work in the blank end-wall of the Pacific Mill served a convenient purpose in making it resemble a large "cross-section sheet," against which the jets were seen projected, and by sighting past a horizontal or vertical bar conveniently located, it was easy to refer any point to the co-ordinates for height and distance as marked on the two masts and on the wall.

There was of course a little parallax at times due to the observer's position, but the writer was careful to bear this in mind, and to allow for it according to his best judgment. I took several instantaneous photographs of the jets; but a very wide angle lens had to be used, and in general the conditions were not favorable for good photographic work, and as the periods of still air were found to be so few, very little time for photographic records could be afforded.

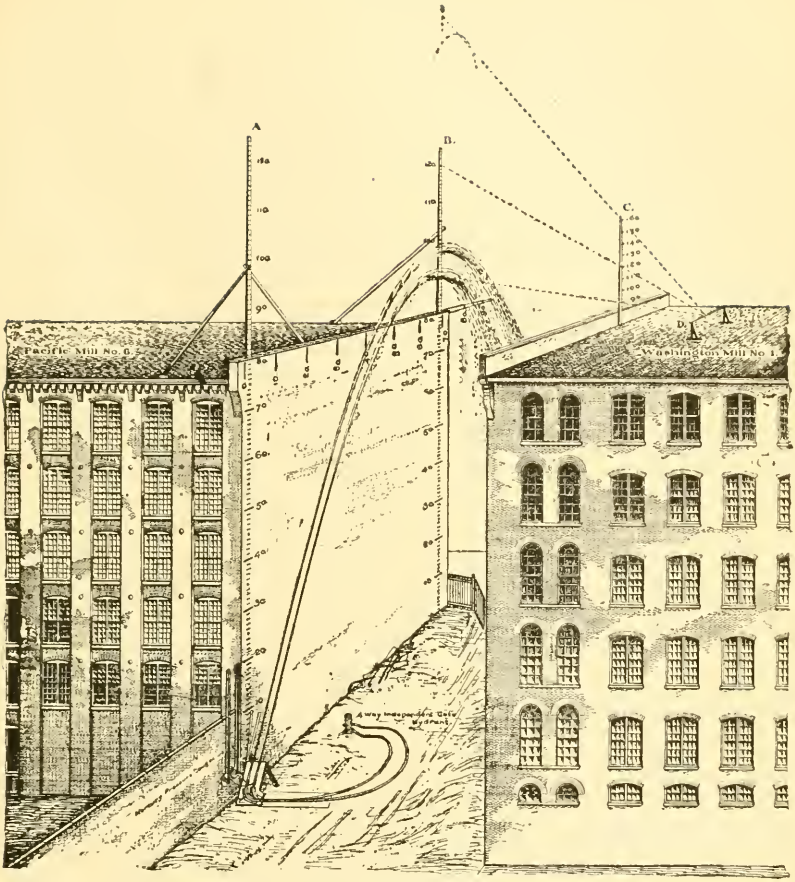
Water was supplied from the city reservoir in nearly all cases where pressure at base of play-pipe was not more than 50 or 60 pounds. For higher pressures the rotary fire-pumps were used.

Generally speaking, the pulsation of jet was not excessive, and in all cases the jet was carefully watched for some little time and the mean value recorded.

I consider that any existing errors of observation on extreme height of jet due to a given pressure are due to air currents more than to any other cause.

There is a good deal of irregularity in the observations on extreme distance as a "good fire stream," but this is due to the indefiniteness of determining just what constitutes this.

Fig. 54.



Experiments on Fire Streams at High Elevations.

The final experiments on which tables for height of jets are based were made at this site, but in these only one nozzle was used at a time.

Sketch illustrates experiments to determine which of two nozzles (ring or smooth, for instance) was the better, or would throw stream highest and with the least spraying,—pressure at base of each play-pipe being the same. For this the two 50-ft. lines of hose leading to play-pipes were of same kind; bends in hose at base of play-pipe were carefully laid flat on the inclined platform, as shown, and clamped between blocks jig-sawed from 3-in. plank, so each hose lay in flat curve of 3 feet radius. 1 to 2 feet of hose next base of play-pipe was straight.

The masts, A and B, were graduated in feet, and reached to a height of 124 feet above nozzle. Below the masts, scales, made of strips of wood strung on heavy steel wire, marked each foot in height along face of wall.

Horizontal distances from end of nozzle were marked by wooden rods 10 feet apart, nailed to top of wall, as shown.

When height of jet was more than 130 feet, the mast, C, and sighting bar, D, were used in estimating height, proper allowance being made for parallax.

The play-pipes and their supporting platform could be set at any angle of elevation required, and securely clamped.

End of nozzle was set level with zero of mercury pressure gauges and zero of scale of height.

An assistant regulated pressure to within one-tenth of a pound by valves at hydrant, and, when all was ready, one observer read the mercury gauges each half minute, while another observed and noted height and characteristics of jet, as seen from the mill roofs.

Apparatus.

The mercury gauges shown in Fig. 1 and piezometers of form shown in Fig. 2 and already described, were used exclusively in all the experiments which are recorded below. Generally speaking, the mean pressure was thus determined with certainty within ¼ pound per square inch. The height marks, at corners of Pacific Mill, were made of strips of board strung on stout steel wire. These were first adjusted on a level floor at the proper distances by aid of a steel tape. The strips of board were then conspicuously figured with black paint and hoisted into position. The masts *A* and *B* were similarly graduated by means of a steel tape. and conspicuously marked and figured before hoisting.

The angles of elevation for the jets at site shown in Fig. 54 were set off by the aid of a very large and accurate vernier protractor. This was used directly to set two batter boards or straight edges each 4 inches wide, 16 feet long, which were nailed to the wall at angles of 60 and 75 degrees respectively, with error of angle probably not exceeding one minute. As each play-pipe was clamped to the supporting frame, it was carefully adjusted by sighting the center line of jet or rather the mean of its edges out of wind with the straight edge above mentioned, and thus it is probable that jets did not vary by so much as ten minutes of arc from 60 or 75 degrees.

Height as Good Fire Stream.

The determination of this was a matter to which the writer gave much attention, because of the fact that a knowledge of height of the extreme drops is of comparatively little practical value to the fireman although of much interest to the physicist, and also because of the fact that tables of "height of jets" in all the text books, pump catalogues, etc., being really the height for the extreme drops, while height as a good practical fire stream was only about two-thirds as much, has sometimes led to serious misunderstandings.

The extreme limit at which a jet can be called a "good fire stream" is not sharply defined, but is to a considerable extent a matter of judgment. It was hard to define the exact limit, and say within 5 feet, or in some cases even within 10 feet, just exactly where the stream ceased to be good. The plotted observations show this by the way they jump

about, but it was the intention that the various estimates should be independent. To give a clearer idea of just what I had in mind, the following statement is presented:

In recording the maximum distance to which the jet attained in such shape that it could be classed as a good fire stream, I classed as such:

First.—A stream which at limit named had not lost continuity of stream by breaking into showers of spray.

Second.—A stream which up to limit named appeared to shoot nine-tenths of whole volume of water inside of a circle 15 inches diameter and three-quarters of it inside a 10-inch circle, nearly as could be judged by the eye.

Third.—Which would probably be stiff enough to attain in fair condition the height or distance named, even though a fresh breeze were blowing.

Fourth.—Which at limit named would, with no wind, enter a room through a window-opening and barely strike ceiling with force enough to spatter well.

It will be noted that plottings of these distances as "good" and "fair" streams jump about irregularly, and my mean curve for "good fire streams" should not be considered strictly definite within, say, 10 or 15 per cent.

Very likely many would class a stream as a "good fire stream" up to the point where I classed it as "fair," but I endeavored to allow a fair margin for effect of wind. The adjustment of the curves of Figs. 56*b* and 57*b* for the several sizes of nozzle for limit of good streams was guided mainly by analogy from the distance apart of the curves for extreme horizontal and vertical distance.

To sum this whole matter up, I feel some confidence that my classification as to extreme point at which stream could be called "good" or "fair" is in general agreement with these terms as they would be used by the average intelligent and experienced fireman.

The maximum horizontal distance for good streams may be exceeded considerably under favorable conditions. No correction for the very small amount of pressure absorbed by friction within the 2 to 3 feet of length of play-pipe between gauge and nozzle proper has been made (except in case of the 2-inch nozzle on Siamese) in plotting the results.

The following tables give the results of experiments in detail:

TABLE No. 9.

Experiments on Vertical Jets.

(See Figs. 56 and 56*b* for plotting of these observations.)

October 5th, 1888, Temperature of air..... 64 degrees.
Temperature of water..... 48 degrees.
Barometer (about)..... 30.0 inches.

No. of Experiment	Size and Kind of Nozzle. (See figures on pages 16 to 23.)	Pressure above Orifice shown by Gauge connected at Base of Play pipe. Pounds.	Computed Discharge of Nozzle. Gallons per Minute.	Correction for Velocity past Gauge. Pounds.	Computed Static Pressure above Orifice. Pounds per Square Inch.	Computed Static Pressure above Orifice. Feet.	Observed Elevation of Jet above End of Nozzle., Average of Highest Drops. Feet.	Mean Average Summit of Jet. Feet.	Height attained by three-fourths of Water. Feet.*
1	$\frac{3}{4}$ smooth (*a*)	60.2	130	.48	60.7	140.2	105.	102.	90.
2	" " "	49.9	117	.38	50.3	116.1	91.	90.	83.
3	" " "	40.2	105	.31	40.5	93.5	78.	76.	72.
4	$\frac{7}{8}$ " (*b*)	49.5	161	.73	50.2	115.9	94.	93.	85.
5	" " "	40.0	145	.60	40.6	93.7	80.	$78\frac{1}{2}$	75.
6	1 " (*c*)	62.4	236	1.61	64.0	147.8	124.	120.5	110.
7	" " "	50.1	209	1.26	51.4	118.7	100.	98.5	94.
8	" " "	40.0	187	1.01	41.0	94.7	82.	81.	77.
9	$1\frac{1}{8}$ " (*A*)	58.0	288	2.37	60.4	139.9	120.	115.	105.
10	" " "	50.2	267	2.05	52.3	120.8	105.	103.	88.
11	" " "	45.2	254	1.86	47.1	108.8	94.	92.	81.
12	" " "	40.1	239	1.64	41.7	96.3	84.	83.5	78.
13	" " "	35.1	223	1.44	36.5	84.3	75.	74.6	68.
14	" " "	30.0	206	1.23	31.2	72.0	64.5	64.	61.
15	" " "	25.0	188	1.01	26.0	60.0	53.	52.8	50.
16	" " "	20.0	169	.81	20.8	48.0	43.	42.8	42.
17	" " "	15.1	147	.62	15.7	36.2	32.4	32.2	31.5
18	" " "	10.0	119	.40	10.4	24.0	21.4	21.2	21.
19	$1\frac{1}{4}$ " (*e*)	53.7	343	3.40	57.1	131.8	115.5	114.5	110.
20	" " "	49.8	330	3.16	53.0	122.4	106.5	105.5	103.
21	" " "	40.2	296	2.51	42.7	98.6	86.	85.5	85.
22	$1\frac{3}{8}$ " (*f*)	48.5	393	4.45	53.0	122.4	105.	103.5	95.
23	" " "	45.1	378	4.05	49.2	113.6	100.	98.	90.
24	" " "	40.1	357	3.66	43.8	101.1	88.5	87.5	80.
25	" " "	35.2	335	3.25	38.5	88.9	77.	76.4	75.
26	2-inch Siamese	49.9	847	2.27	52.2	120.5	106.	103.	100.
27	" "	40.1	760	1.83	41.9	96.7	85.	84.	80.
28	" "	30.1	658	1.89	32.0	73.9	65.	64.	62.

Note.—In plotting experiments 26–28, 2 per cent. was added to height as here given to allow for head absorbed b friction in elbow, etc.

In all experiments in this table water was supplied from city reservoir.

Experiments on $1\frac{1}{8}$-inch and $1\frac{3}{8}$-inch nozzles made from 7.30 to 8.30 A.M.
" $1\frac{1}{4}$, 1, $\frac{7}{8}$, $\frac{3}{4}$ " " " 4.30 to 6 P.M.
" 2 " " " 10.30 to 11.15, October 11th.

* This proportion ($\frac{3}{4}$ of whole volume discharged) was estimated only by the eye, and is, therefore, a very rough approximation.

TABLE No. 9—(*Continued*).

Pulsation at Summit.	Actual Height less than Static Pressure.		Excess of Observed Height over Height Computed from Formula below.	Remarks.
	Highest Drops.	Mean Summit.		Temperature of water, 60 degrees; temperature of air, 64 degrees. In all these vertical jet experiments there was apparently much less water lost or detached from main stream on the way to the summit than when stream was inclined. Pulsations noted occur at intervals of 2 or 3 seconds; practically no wind; excellent conditions for observation; summit very well defined; stream holds together excellently; big drops and lumps of water at very top; nearly one-half of whole volume reaches summit.
Feet.	Feet.	Feet.		
3.	35.2	38.2	+ 0.1	
3.	25.1	26.1	— 0.9	Stream retains its integrity well up to 85 feet above nozzle.
2.	15.5	17.5	+ 0.2	Top is well defined, looks like an irregular solid lump of water.
2½	21.9	22.9	— 1.2	Good conditions for observing height certainly within ½ foot.
2.	13.7	15.2	— 0.2	
3.	23.8	27.3	+ 5.8	Summit of jet well defined.
2.	18.7	20.2	+ 0.3	
1.	12.7	13.7	— 0.6	
4.	19.9	24.9	+ 3.6	
2.	15.8	17.8	+ 1.7	
2.	14.8	16.8	— 0.6	Height of summit determined certainly within 1 foot.
1.5	12.3	12.8	— 1.2	
1.	9.3	9.7	— 0.8	
0.8	7.5	8.0	— 1.3	All the time, interference to jet from falling drops is very slight.
0.5	7.0	7.2	— 2.7	
0.5	5.0	5.2	— 2.2	Almost the whole of water forms a compact bunch to top.
0.4	3.8	4.0	— 2.2	
0.3	2.6	2.8	— 1.9	
2.	16.3	17.3	+ 2.5	
2.	15.9	16.9	+ 0.2	Excellent observation. Water lumpy at top.
1.	12.6	13.1	— 2.1	
2.	17.4	18.9	— 2.8	
2.	13.6	15.6	— 0.9	
1.5	12.6	13.6	— 2.6	This jet holds together to the very top.
1.	11.9	12.5	— 4.1	Effect of wind is so slight that water falls almost exactly vertically.
	14.5	17.5	— 6.7	There was a slight gusty wind, but these observations were taken in the lulls, and I don't think wind caused an error of more than 1 foot, if at all.
....	11.7	12.7	— 7.0	These were so sheltered as not to be affected by wind.
....	8.9	9.9	— 6.6	

The following formula has its constants based upon the above experiments:

$$J = h - .00135 \frac{h^2}{d}.$$

In which J = Height in feet of average extreme drops.

h = Static head in feet at base of play-pipe.

d = Diameter, in inches, of jet close to nozzle.

TABLE No. 10.—EXPERIMENTS ON JETS AT 75 DEGREES ELEVATION.

(See Figs. 56 and 56*b* for plotting of these observations.)

September 24, Temperature of Air, 58 degrees.
October 2, " " 57 "
November 9, " " 52 "

Date. 1888.	No. of Experiment.	(Size and Kind of Nozzle. See figures on pages 16 to 23.)	Actual Pressure Above Orifice, Shown by Gauge Connected at Base of Play-pipe.	Computed Discharge of Nozzle. Gallons per minute.	Correction for Velocity Past Gauge.	Computed Static Pressure Above Orifice. Pounds per Square Inch.	Computed Static Pressure Above Orifice. Feet.	Observed Elevation of Jet above End of Nozzle. Average of Highest Drops.	Summit of Center of Main Body.	Extreme Limit of Good Fire Stream.	Extreme Limit of Fair Fire Stream.
Nov. 9	29	¾-inch Smooth (*a*)..	101.0	169.	0.81	101.8	235.0	129.	122.	83.	90.
"	30	1 " (*c*)..	103.0	303.	2.65	105.7	244.1	154.	148.	90.	110.
Oct. 2	31	1 " (*c*)..	50.0	209.	1.26	51.3	118.5	95.	93.	76.	80.
"	32	1 " (*c*)..	40.1	187.	1.01	41.1	94.9	79.	78.	66.	70.
"	33	1 " (*c*)..	30.1	161.	0.73	30.8	71.1	61.	60.	49.	52.
"	34	1 " (*c*)..	20.1	134.	0.51	20.6	47.5	43.	42.	35.	38.
Nov. 9	35	1⅛ " (*B*)..	104.0	385.	4.28	108.3	250.1	155.	153.	100.	115.
Oct. 2	36	1⅛ " (*B*)..	50.3	267.	2.02	52.3	120.8	101.0	98.	81.	85.
"	37	1⅛ " (*B*)..	39.8	238.	1.63	41.4	95.6	82.	81.	70.	72.
"	38	1⅛ " (*B*)..	30.2	207.	1.24	31.4	72.5	62.	60.	51.	55.
"	39	1⅛ " (*B*)..	20.1	169.	0.81	20.9	48.3	44.	43.	36.	40.
Feb. 21	40	1⅛ " (*B*)..	9.7	.....	0.40	10.1	23.3	18.	18.	18.	.....
Nov. 9	41	1⅛-inch Sawed off (*C*)..	99.5	373.	4.00	103.5	239.0	153.	148.	105.	115.
"	42	1⅛ " (*C*)..	75.0	324.	3.04	78.0	180.1	139.	134.	85.	100.
"	43	1¼-inch Smooth (*e*)..	86.5	434.	3.88	90.4	208.7	157.	150.	100.	110.
"	44	1¼ " (*e*)..	75.7	406.	4.80	80.5	185.9	141.	135.	85.	104.
Sep. 24	45	1¾-inch Smooth Cone (*t*).	49.8	644.	1.35	51.2	118.2	98.	96.	75.	80.
"	46	1¾ " On big Siamese.	45.2	614.	1.21	46.4	107.1	90.	88.	70.	75.
"	47	1¾ " " "	40.5	581.	1.08	41.6	96.0	80.	77.	65.	72.
"	48	1¾ " " "	35.1	542.	0.94	36.0	83.1	69.2	67.5	55.	60.
"	49	1¾ " " "	30.1	503.	0.81	30.9	71.3	60.	58.5	46.	52.
Sept. 24	50	2-inch Smooth Nozzle (*s*)	50.3	820.1	2.12	52.4	121.0	100.	97.	78.	82.
"	51	2-inch On Big Siamese..	45.0	776.1	1.90	46.9	108.3	91.5	87.5	70.	76.
"	52	2-inch Rifle Barrel.. ...	40.5	736.	1.73	42.2	97.4	83.	80.	66.	70.
"	53	2-inch "	34.9	684.	1.50	36.4	84.0	71.3	69.7	57.	64.
"	54	2-inch "	30.4	640.	1.31	31.7	73.2	62.2	60.5	50.	56.
"	55	2-inch "	24.7	574.	1.05	25.7	59.3	53.8	51.3	42.	46.

NOTE.—In the plotting of experiments 50 to 55, on Fig. 56

TABLE No. 10—(*Continued*).

Observed Elevation of Jet above End of Nozzle.		Observed Horizontal Distance North from End of Nozzle.				Remarks.
Actual Height of Jet less than Static Pressure.		Of Summit of Jet.	Of Limit of Good Fire Stream.	Of Fair Fire Stream.	Of Outside of Jet where Striking Ground.	
Highest Drops.	Main Body.					
106.0	113.0	.....	.....	.....	.....	Stream tapers out to a thin fine spray at end.
90.1	96.1	.....	.....	.....	.....	More water lost by spray on way to summit than with larger nozzles.
23.5	25.5	45.	27.	30.	70.	During the eight experiments of October 2 there was very little wind, and the conditions were favorable for accurate observations.
15.9	16.9	37.	22.	27.	60.	In these eight experiments pressures observed each 15 seconds; thus pressure as here recorded is mean of five observations.
10.1	11.1	29.	15.	20.	50.	
4.5	5.5					
95.1	97.1	65.	.....	.....	.....	Practically no wind. Temperature of water 50 degrees Fahrenheit, air 52 degrees Fahrenheit. At 90 feet high three-fourths of all water shoots within a 1-foot circle; at 80 feet high I judge that eight-tenths of water shoots within a 1-foot circle.
19.8	22.8	45.	30.	31.	85.	
13.6	14.6	40.	27.	28.	70.	
10.5	12.5	30.	20.	22.	55.	
4.3	5.3	19.	13.	18.	40.	
5.3	5.3	.....	.....	.....	.....	This was a coherent solid stream to summit (Bourdon gauge used).
86.0	91.0				Values in this Column not sharply defined.	This throws a good close stream at all pressures.
41.1	46.1					
51.7	58.7	67.				
44.9	50.9					
20.2	22.2	51.5	26.			
17.1	19.1	48.	.....	28.		
16.0	19.0	42.	23.	.....		
13.9	15.6	37.	20.	23.5		
11.3	12.8	32.	18.	21.		
21.0	24.0	47.	28.	31.	73.	Very calm. Excellent conditions for observing.
16.8	20.8	46.	25.	29.	70.	
14.4	17.4	42.	24.	27.	73.	
12.7	14.3	36.	21.	25.	66.	
11.0	12.7	33.	18.	22.	60.	
5.5	8.0	23.5	16.5	19.	50.	

2 per cent. is added to observed height to allow for loss of head in elbow and Siamese.

TABLE No. 11.

Experiments on Jets at 60 Degrees Elevation.

September 17th, 1888. Temperature of Air, 75 degrees. Temperature of Water, 67 degrees. Barometer (about) 30.0.

No. of Experiment.	Size and Kind of Nozzle.	Actual Pressure above Orifice shown by Gauge Connected at Base of Play-pipe.	Computed Discharge of Nozzle. Gallons per Minute.	Correction for Velocity past Gauge.	Computed Static Pressure above Orifice. Pounds per Square Inch.	Computed Static Pressure above Orifice. Feet.	Observed	
							Average of Highest Drops.	Summit of Center of Main Body.
56	⅞-inch smooth (*a*)	61.2	179	.91	62.1	143.4	93.5	91.
57	" " "	50.3	162	.74	51.0	117.8	76.	74.
58	" " "	40.2	146	.60	40.8	94.2	61.5	59.
59	" " "	25.4	115	.37	25.8	59.6	43.	41.5
60	1-inch smooth (*c*).....	61.3	234	1.59	62.9	145.2	91.5	88.
61	" " "	55.2	219	1.39	56.6	130.7	84.	82.5
62	" " "	50.4	210	1.27	51.7	119.4	76.5	74.25
63	" " "	40.1	187	1.01	41.1	94.9	65.5	63.5
64	" " "	30.1	161	.74	30.8	71.1	50.	48.5
65	1⅛-inches smooth (*B*)..	50.1	267	2.03	52.1	120.3	81.	79.
66	" " " ..	55.8	281	2.26	58.1	134.2	90.	87.5
67	" " " ..	40.2	239	1.64	41.8	96.5	67.	65.5
68	" " " ..	30.0	206	1.23	31.2	72.0	51.	50.
69	" " " ..	25.0	187	1.01	26.0	60.0	41.5	40.
70	1¼-inches smooth (*e*)..	50.3	331	3.20	53.5	123.5	84.	82.
71	" " " ..	40.0	296	2.51	42.5	98.1	67.5	66.
72	1⅜-inches smooth (*f*)..	46.6	384	4.26	50.9	117.5	81.	80.
73	" " " ..	40.0	357	3.66	43.7	100.9	69.	67.
74	" " " ..	35.0	334	3.24	38.2	88.2	61.5	60.3
75	" " " ..	30.1	311	2.80	32.9	75.9	52.4	51.
76	" " " ..	25.0	283	2.26	27.3	63.0	43.5	42.5

In all experiments in this table water was supplied from city reservoir.

TABLE No. 11—(*Continued*).

Elevation of Jet above End of Nozzle.		Observed Horizontal Distance North from End of Nozzle.		Remarks.
Extreme Limit of Good Fire Stream.	Extreme Limit of Fair Fire Stream.	Of Summit of Jet.	Of Middle of Jet where striking ground.	In all these experiments noted below, the regular 30-inch Morse play-pipe was used and was fed by 50 feet of solid rubber hose. The hose near play-pipe was laid on a flat, inclined table which kept it in a plane curve and was confined between circular blocks which held it in a curve of 3 feet radius and of 90 degrees arc. Up stream from this point hose was straight for 10 feet and thence led to hydrant in a loop curved nowhere with less than 4 or 5 feet radius.
77	82	88	*	
63	66	71		
45	55	62		
36	39	45		
75	80	85	120	
72	77	78		
60	64	75		
55	58	70		
42	48	55		
69	75	92	126	Excellent experiment. No wind. Mean of several independent trials.
73	79	89	120	Occasional faint puffs of wind, but not enough, I think, to affect jets. Very little difference in character of stream under the different pressures. Stream if anything of better character under the high than the low pressures.
50	58	70	107	
45	48	57	90	
37	40	43	78	
75	80	88	.....	Both these are excellent experiments. Wind very faint. Drops at summit, ½-inch diameter.
57	63	70	.....	
63	72	87	Center, 128 Extreme, 135	No wind.
58	65	74	Center, 118 Extreme, 123	
50	55	65	105	
46	50	56	100	
35	40	46	86	

* Values in this column not sharply defined.

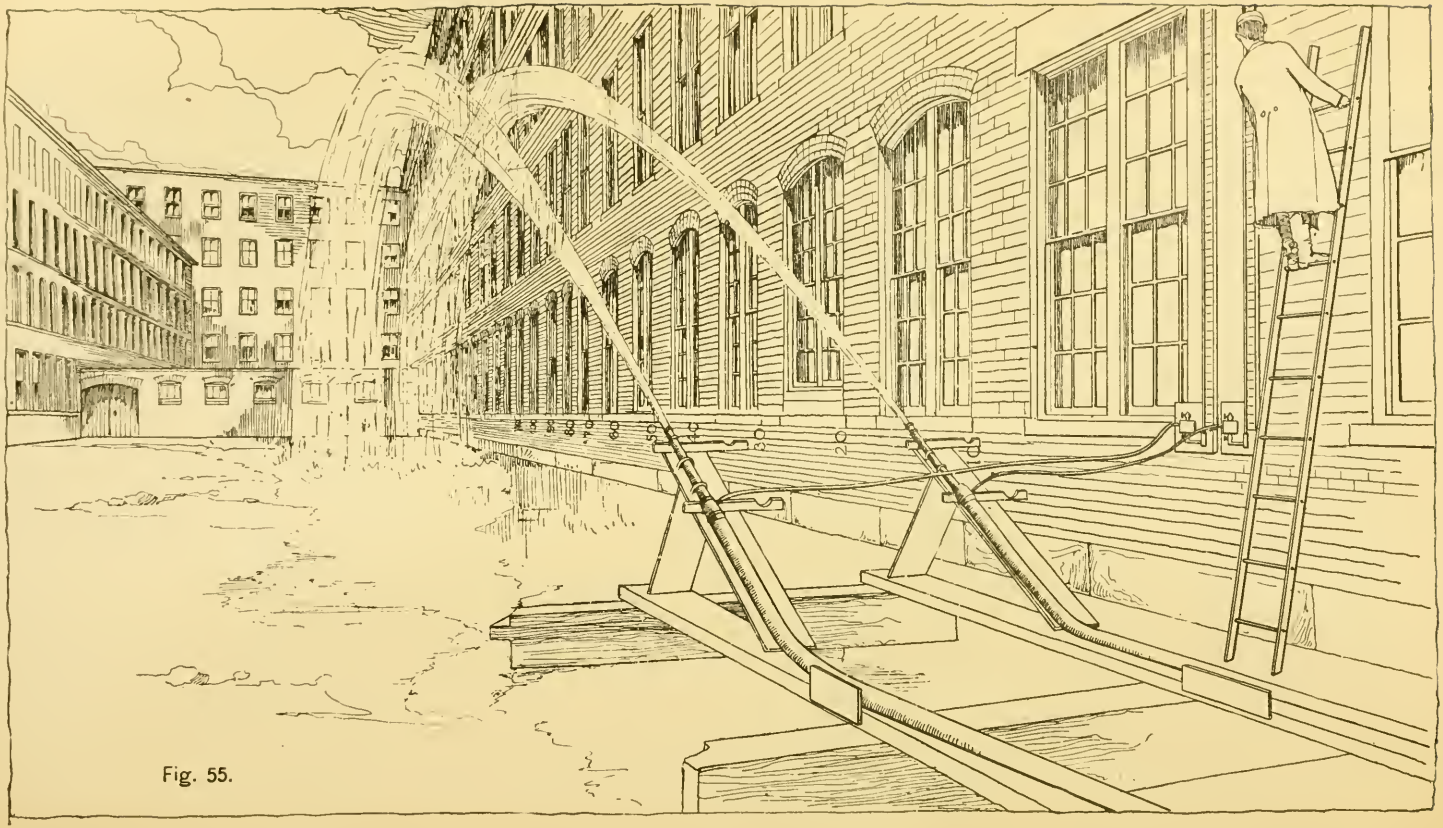


Fig. 55.

Jets at 32 Degrees Elevation.

These experiments were intended to show the greatest horizontal distance that jet could attain. Their site is shown in Fig. 55. It will be noted that the site was well sheltered from currents of air, but care was taken to record no experiments here when any breeze was perceptible other than the slight air current caused by the jet itself.

The range of pressure was carried up beyond limits of ordinary practical use, and I consider that this set of experiments is very satisfactory.

Distances east from end of nozzle were measured off by a steel tape and conspicuously marked on the adjacent wall, and as the nozzles were set just at level of the window sills and as windows happened to be built with 10-foot bays and stories just 15 feet high, the mill itself furnished a convenient measuring scale, and its windows furnished very convenient points for observation. The heights were, however, determined with much less exactness, proportionally, than for experiments at Fig. 54, the main attention being here given to the distance. It will be seen that the nozzle supports were adjustable to various angles of elevation. Several preliminary experiments were made with 50 pounds pressure, to determine angle of elevation which would give the maximum horizontal distance. Thirty-two degrees was the elevation which appeared to do this, and was adopted in the experiments as being a fair value within the range of pressures adapted for ordinary fire streams; but as a degree or two made but little difference in this distance, it cannot be stated certainly whether 30 or 34 degrees might not have served equally well.

As there was a slight deflection of support for nozzle under the recoil from the jet, this angle of elevation was determined with less exactness than for jets shown in Fig. 54, and may be 1 degree in error. Care was taken, however, that when a comparison between two jets was made the angles of elevation should be precisely the same.

Certain other experiments at 15 pounds pressure showed that this low pressure, 35 to 40 degrees elevation, enabled stream to reach about 5 per cent. further than 32 degrees elevation. And at from 5 to 10 pounds pressure the maximum horizontal distance was found to be attained when elevation approached 45 degrees.

The extreme end of jet was made up of a mere rain of scattering drops and was less sharply defined than in the vertical jets; but as I was able to stand close beside it, a fair average could be taken, and even at the highest pressures I think the recorded maximum distance could seldom, if ever, have been more than 5 feet in error. The main body

TABLE No. 12.

Experiments on "Horizontal" Jets. Elevation, 32 Degrees.

November 9th, 1888. Temperature of air, 55 degrees. Temperature of water, 49 degrees. Water supplied by rotary fire pump.

No. of Experiment.	Size and Kind of Nozzle.	Actual Pressure above Orifice, Shown by Gauge Connected at Base of Play-pipe.	Computed Discharge of Nozzle. Gallons per Minute.	Correction for Velocity Past Gauge.	Computed Static Pressure above Orifice. Pounds per Square Inch.	Computed Static Pressure above Orifice. Feet.	Observed Horizontal Distance from end of Nozzle reached by Jet.				
							At Level of Orifice.				
							Average of Extreme Drops.	Average of Main Body.	Distance Reached by three-fourths of Water.	At Summit.	Extreme Limit of Good Fire Stream.*
77	$\frac{3}{8}$-inch Smooth Cone (*a*)	99.7	168	0.81	100.6	232.3	167.	155	140	130	70
78	$\frac{3}{8}$ " " "	75.4	146	0.61	76.1	175.7	150.	140	120	100	57
79	$\frac{3}{8}$ " " "	51.8	119	0.39	52.2	120.5	127.	120	100	80	50
80	$\frac{3}{8}$ " " "	31.1	92	.24	31.3	72.3	98.	95	87	57	38
81	1-inch Smooth Cone (*c*)	106.1	308	2.75	108.9	251.4	205.	195	165	150	88
82	1 " " "	79.3	265	2.00	81.3	187.7	188.	175	150	125	78
83	1 " " "	47.3	203	1.08	48.4	111.8	147.	140	120	87	57
84	1 " " "	30.4	162	.75	31.2	72.0	108.	105	.....	59	47
85	$1\frac{1}{8}$-inch Smooth.....(*B*)	101.0	380	4.18	105.2	242.9	230.	215	175	150	90
86	$1\frac{1}{8}$ " ..... "	88.0	354	3.61	91.6	211.5	210.	197	.....	150	80
87	$1\frac{1}{8}$ " ..... "	75.6	328	3.13	78.7	181.7	197.	190	150	127	73
88	$1\frac{1}{8}$ " ..... "	50.4	268	2.03	52.4	121.0	165.	155	.....	95	65
89	$1\frac{1}{8}$ " ..... "	50.0	267	2.02	52.	120.0	164.	155	.....	95	65
90	$1\frac{1}{8}$-inch Smooth Cone (*A*)	20.0	169	.81	20.8	48.	.....	.....	.....	.....	43
91	$1\frac{1}{8}$ " " "	15.3	99	.61	15.9	36.7	60.	59	.....	32	40
92	$1\frac{1}{8}$ " " "	9.7	40	.40	10.1	23.3	36.5	36	.....	18	20
93	$1\frac{1}{4}$-inch Smooth Cone (*e*)	94.3	454	5.95	100.3	231.6	230.	210	180	155	95
94	$1\frac{1}{4}$ " " "	75.8	406	4.80	80.6	186.1	208.	200	160	137	80
95	$1\frac{1}{4}$ " " "	49.7	330	3.16	52.9	122.1	169.	157	138	98	75
96	$1\frac{1}{4}$ " " "	35.9	281	2.25	38.2	88.2	137.	130	120	70	65

In all these experiments jets were sheltered by mill wall and there was no wind whatever blowing, except some motion of air caused by jet itself.

In all experiments of November 9th water was supplied by the mill fire pump. Experiments 90, 91, 92 were made upon February 21, 1889, measuring pressure by Bourdon gauge.

* Since constructing the jet diagram, I have been led to think that perhaps on this day I was uncommonly critical, and thus rated distance to which stream could be classed as a "good fire stream" a few per cent. too low.

TABLE No. 12—(*Continued*).

Observed Elevation of Jet above End of Nozzle.				
Extreme Limit of Fair Fire Stream.	At Summit.	At Limit of Good Fire Stream.	At Limit of Fair Fire Stream.	REMARKS.
77	52			
70	40	.....	.....	All rain beyond 100 feet.
60	31	.....	.....	All rain beyond 85 feet.
45	20	.....	.....	Too small and feeble for a first-class stream.
98	65	.....	.....	All is a rain beyond 170 feet.
90	48	.....	.....	Only rain beyond 130 feet.
75	45			
55	20	19.5		
105	63	50.	.....	Beyond distance 180 feet this is only a heavy rain. At 70 feet distance stream all [goes within circle of 1 foot diameter.
100	55	41.	.....	
95	48	40.		
80	34	29.5		
80	33	30.5	.....	Beyond 155 feet distance jet became only a heavy rain.
.....	.....	.....	.....	At 36 and 40 degrees elevation stream goes 3 feet farther.
45	.....	.....	.....	Stream is tolerably compact to end—would do fair execution in second story of a cottage house.
.....	.....	.....	.....	This at 45 degrees elevation, since that was found to throw farthest.
108	65	48.	.....	Stream all rain beyond 180 feet.
100	50	.....	.....	Stream mostly rain beyond 155 feet.
85	33	32.	.....	" " " 130 "
70	24	23.		

was also not sharply defined. As considerable variations appear on all the plotted sheets of jet experiments, it is but fair to state that an earnest effort was made to have the successive observations independent of one another, and with this in view the writer frequently called an assistant to make an independent record of his own opinion of the proper values for distance, etc. In other words, it was intended that the record should fairly represent the indefiniteness existing in the observation.

In all these jet experiments, at whatever angle, one assistant manipulated the hydrant-gate, another observed the mercury gauge continuously, and another generally assisted by observing jet from a different point of view from the station of the writer. Care was taken that water, after opening the hydrant-gate, should get into a condition of steady flow before the observations were begun.

The observations of Table No. 9, on extreme height of vertical jets, were plotted on cross-section paper to the scale of one centimeter, = 2 feet and = 2 pounds. A small scale photographic reproduction of this plotting is shown in Fig. 56, and on the same sheet in Fig. 56*b* are shown the heights attained by jets at 75 degrees elevation.

It will be seen by table that no experiments on jets exactly vertical were obtained with higher pressure than 60.7 pounds. Experiments on vertical jet from each size of nozzle would have been made at pressures ranging up to 100 pounds, but for failure to find an opportunity when the air was still enough to make observations of any real value. The curves for mean values of height attained by extreme drops under pressures from 60 to 110 pounds were therefore projected by the aid of experiments upon the 75-degree jet plus a percentage derived from a comparison of the vertical with the 75-degree jets at from 40 to 50 pounds pressure.

Accuracy of Mean Curves for Height and Distance of Jets.

I consider that we can prudently rely on the mean curves of Figs. 56 and 57 being not more than two or possibly three per cent. in error in their definition of the extreme height and distance to which a stream will rise or reach in still air under a given pressure. It will be noted that the completeness and general agreement of observations is better for horizontal distance than for vertical height.

The proper location for these curves was studied with much care, taking the four sets with the different angles of elevation together, and making use of various little auxiliary plottings and computations.

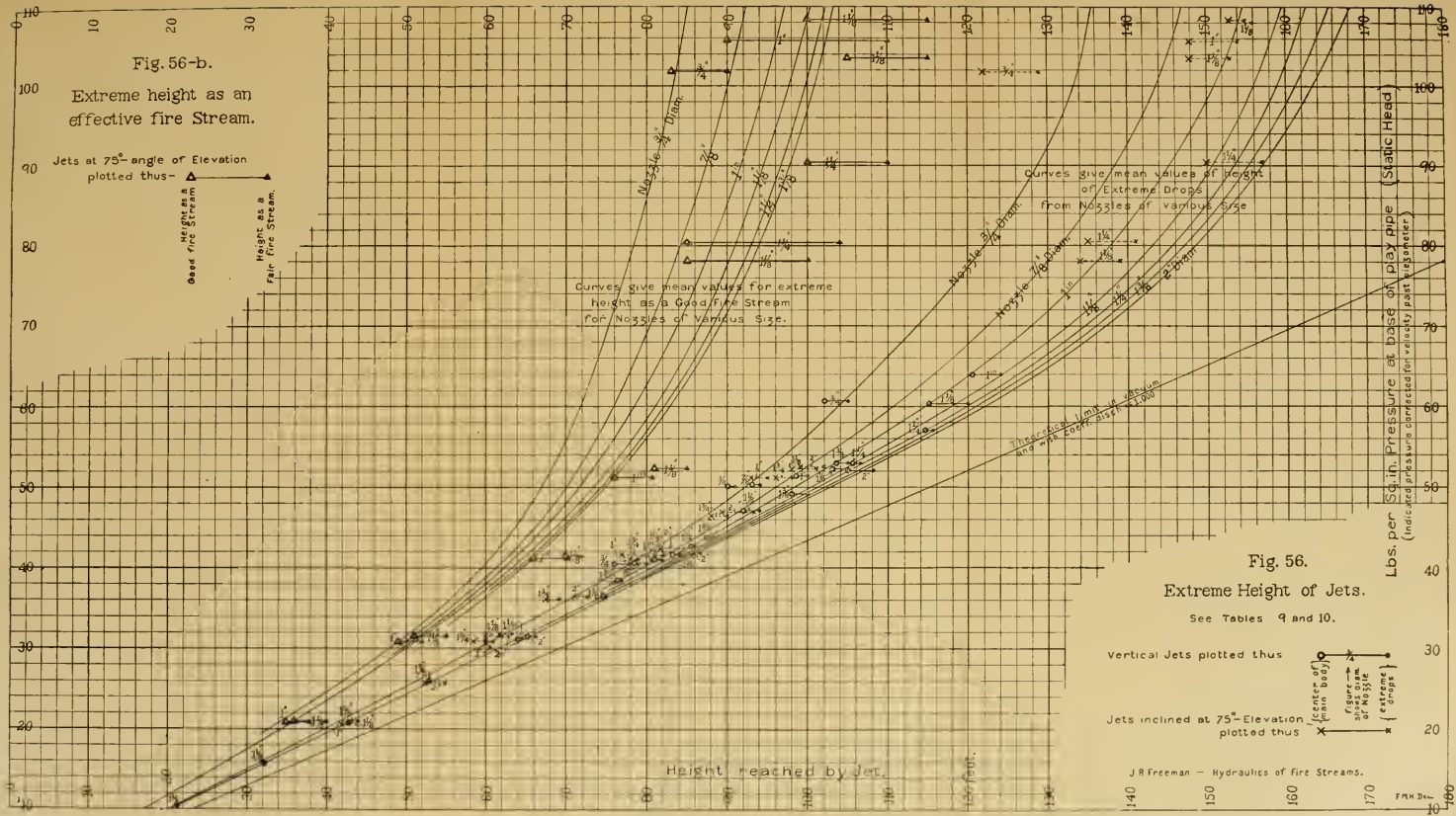


Fig. 56-b.
Extreme height as an effective fire Stream.
Jets at 75°- angle of Elevation plotted thus-
Height as a Good fire Stream.
Height as a Fair fire Stream.
Curves give mean values for extreme height as a Good Fire Stream for Nozzles of various Size.
Curves give mean values of height of Extreme Drops from Nozzles of various Size
Nozzle 3/4" Diam.
Nozzle 7/8" Diam.
Nozzle 1" Diam.
Nozzle 1 1/8" Diam.
Nozzle 1 1/4" Diam.
Nozzle 1 1/2" Diam.
Nozzle 2" Diam.
Theoretical limit in vacuum and with coeff. discH = 1.000
Lbs. per Sq. in. Pressure at base of play pipe (Static Head)
(Indicated pressure corrected for velocity part of gauge)
Height reached by Jet.
Fig. 56.
Extreme Height of Jets.
See Tables 9 and 10.
Vertical Jets plotted thus
Jets inclined at 75°-Elevation plotted thus
(Centre of Fire body)
(Figure of 3/4" Nozzle Extreme drops)
J R Freeman — Hydraulics of Fire Streams.
F.M.H. Dr.

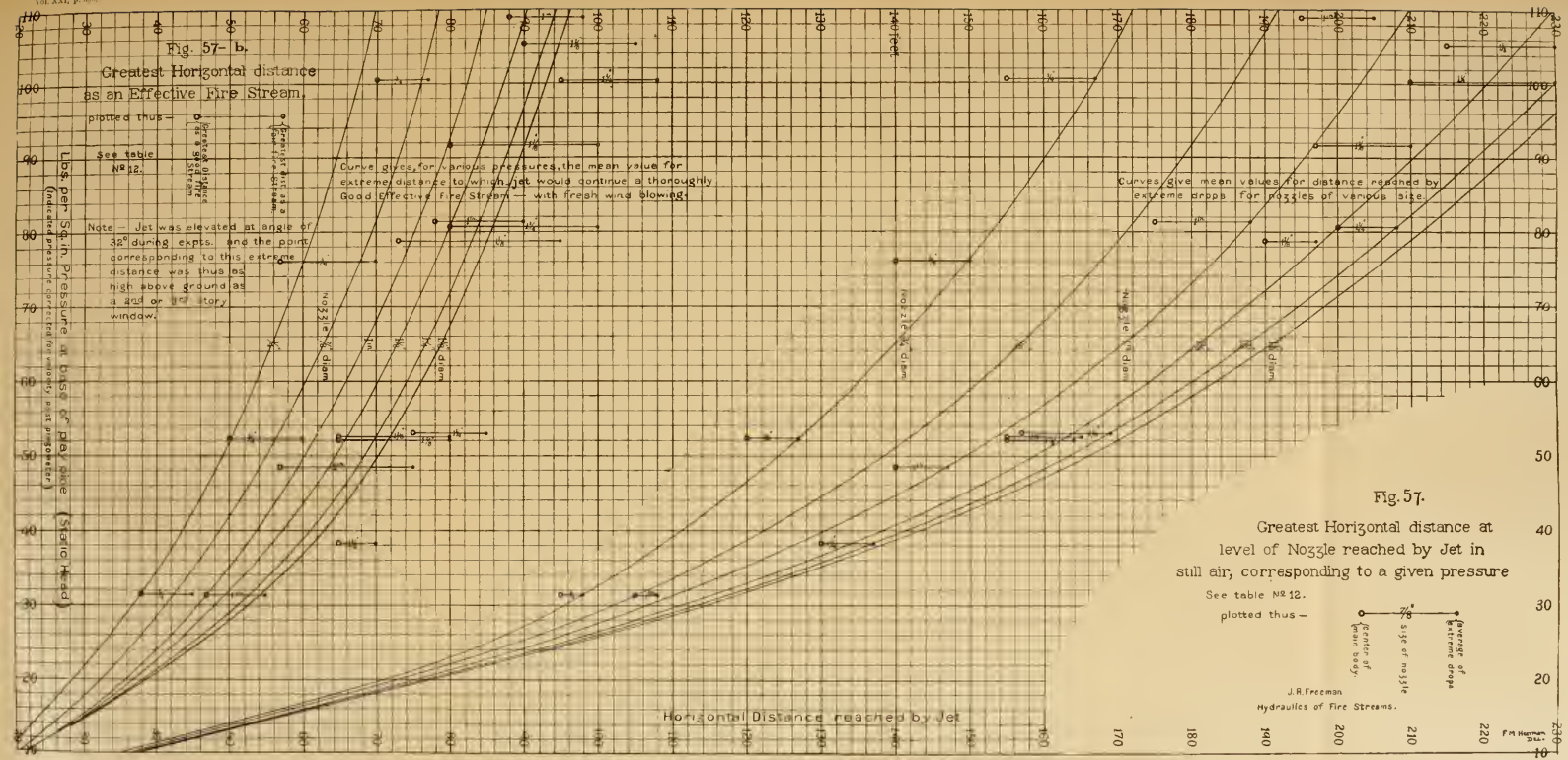


Fig. 57- b.
Greatest Horizontal distance as an Effective Fire Stream.
plotted thus —
See table Nº 12.
Curve gives, for various pressures, the mean value for extreme distance to which jet would continue a thoroughly Good Effective Fire Stream — with fresh wind blowing.
Note — Jet was elevated at angle of 32° during expts. and the point corresponding to this extreme distance was thus as high above ground as a 2nd or 3rd story window.
Curves give mean values for distance reached by extreme drops for nozzles of various size.
No. 2 1/2" diam.
No. 2"
No. 1 3/4" diam.
No. 1 1/2" diam.
No. 1 1/4" diam.
No. 1" diam.
Lbs. per Sq. in. Pressure at base of play pipe (Static Head)
(Indicated pressure corrected for velocity at nozzle-orifice)
Horizontal Distance reached by Jet
Fig. 57.
Greatest Horizontal distance at level of Nozzle reached by Jet in still air, corresponding to a given pressure
See table Nº 12.
plotted thus —
Number of gum bags.
Size of nozzle
Average of extreme drops
J. R. Freeman
Hydraulics of Fire Streams.

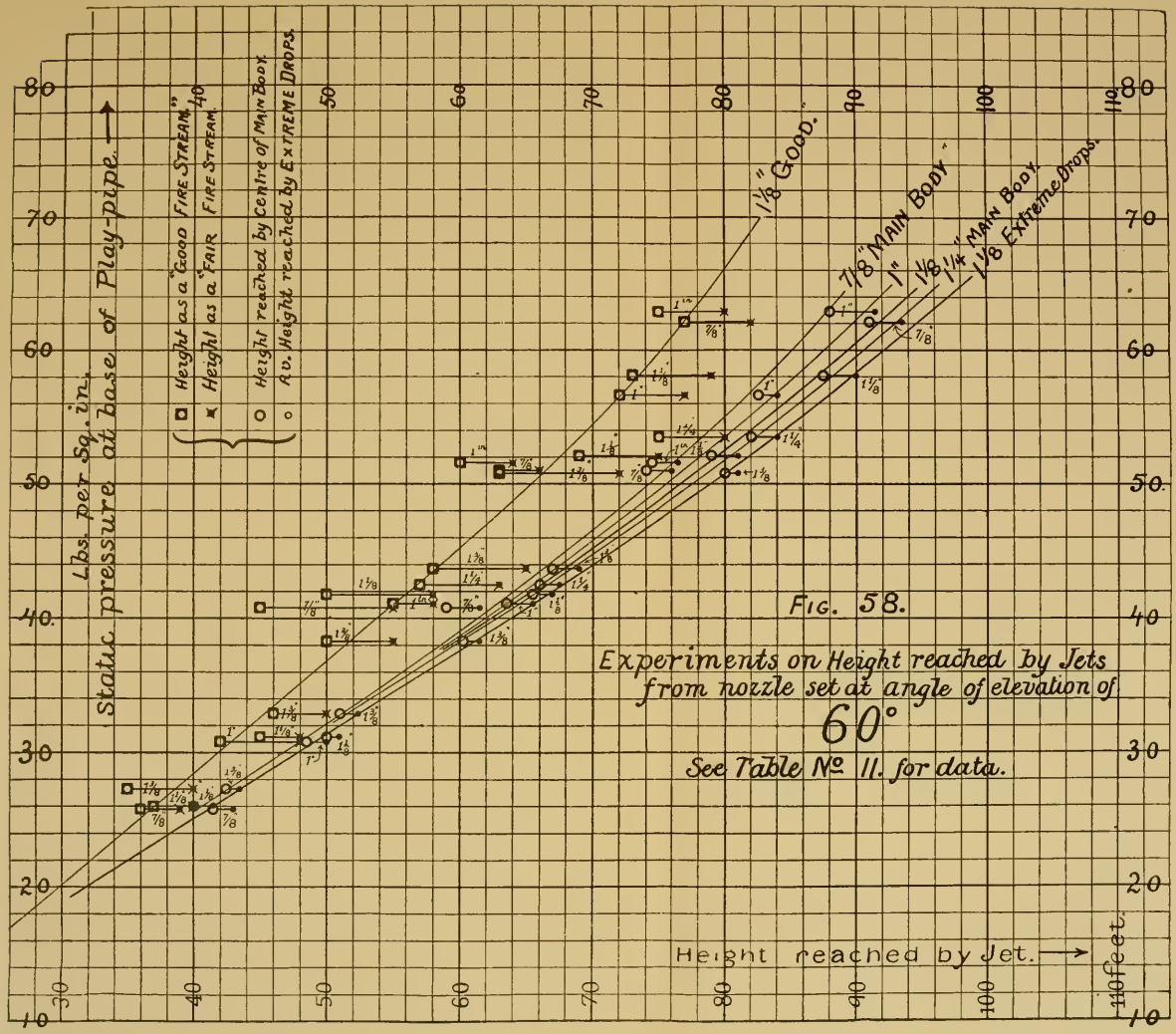


FIG. 58.

Experiments on Height reached by Jets from nozzle set at angle of elevation of 60°

See Table No. 11. for data.

In some cases one observation was given slightly less weight than another, but only when justified by recollection of some circumstance unfavorable to the highest accuracy.

An earnest effort was made to keep the curves close to the plotted observations, rather than to follow any particular algebraic law.

The full records of the observations have been given, to enable any investigator to judge of the liability to error of these curves, to draw new curves or to arrange an algebraic expression for the law.

It may not be amiss to say that for problems of this nature I believe a large scale plotting and the method of average judgment to be preferable to the "method of least squares." It so happened that I did not attempt to state the result in algebraic form until after the practical tables were computed and Figs. 56, 57 and 58 engraved. Any formula for this must be almost purely empirical and an empirical curve was deemed as good as an empirical formula for the preparation of the practical fire stream tables which was the main purpose in view. In cases like this a curve is more convenient than a formula for most practical uses, and has the advantage of being less likely to be used far beyond the limits of the experiments from which it was derived.

In connection with limiting the application of an empirical formula to the range of the experiments from which it was derived we may again refer to the Box formula for jets. Mr. Box presents a formula whose curve coincides very well with the few experiments at hand, and then guided but by a single experiment on a 1-inch jet proceeds far beyond most of the experiments into a generalization from the mathematical form of this curve, that it follows for each size of nozzle there is a pressure producing a maximum height of jet, and that to increase the pressure beyond this point will actually diminish the height of jet. Now this may very likely be true, or possibly for a jet of given size the height may remain nearly stationary above a certain pressure, but as yet no really decisive experiments are on record. If, as is not unreasonable to suppose, there is a slight decrease of height for extremely high pressures, it probably follows an entirely different law from the ascending curve.

It will be seen that the range of the experiments includes all sizes commonly used for fire streams, and that the pressures at base of play-pipe were carried up considerably beyond those with which firemen can work with any ordinary appliances.

The curves of Figs. 56 and 57 were used in the construction of the practical tables at the end of this paper.

To express algebraically with great accuracy the extreme height to which a jet of given diameter will rise under a given head requires a formula which, so far as I have been able to devise it, is inconveniently complicated for ordinary use.

As an approximate formula, the following will serve very well, and within the range of fire streams, namely, for diameters of from $\frac{3}{4}$ inch to $1\frac{3}{8}$ inches and pressures of not more than 80 pounds, its use will not lead to an error of more than 3 or 4 per cent.* Practically this is near enough for almost any purpose, inasmuch as even a faint wind will lessen the height to an extent greater than the error in this formula:

Let J = extreme height in feet to which the average extreme drops of a vertical jet will rise in still air.

h = static head of water in feet acting at base of play-pipe.

d_1 = diameter of jet close to nozzle in inches. (For smooth nozzles this is same as diameter of orifice. For ring nozzles take d_1 as 86 per cent. of diameter of orifice.)

Then $J = h - .00135 \frac{h^2}{d_1}$.

This is substantially the same as the formula used by Box, but expressed in more convenient form and with the constant modified to fit my experimental results.

For values of d_1, larger than 2 inches, use 2, until further experiments, since the plotted curves indicate that for sizes larger than this the effect of diameter of jet upon its height is small.

---

* For pressures of from 40 to 70 pounds, and diameters from $\frac{3}{4}$ up to $1\frac{3}{8}$ inches, this formula agrees remarkably well with the experiments. For diameters larger than $1\frac{3}{8}$ inches the diameter has less influence on height to which the water will rise than the formula indicates; and, indeed, it would appear that after a diameter of 2 inches was reached, a further increase of size would have but little influence on height, while pressure remained the same.

When pressures become greater than 70 pounds, or 150 feet, the loss in height of jet by resistance of air increases apparently at a more rapid rate than the formula implies, or varies as a power of h somewhat higher than the square. Until we have more data on jets of 2 or more inches diameter and at pressures of 100 pounds and upward, it is, perhaps, best not to attempt to devise an extremely accurate general formula. Meanwhile the above formula, the plotted curves of Fig. 56, or the table in the appendix, will satisfy almost any practical purpose.

Experiments to Determine Best Form of Nozzle.

A leading object of these experiments was to determine the best form of nozzle and play-pipe for practical use, or to find out what form would, for a given pressure, project a given number of gallons of water per minute to the greatest height or distance, and with the least loss by the detaching of drops or spray on the way, thus delivering the water in as solid a body as possible, at the greatest possible distance.

At the beginning of the experiments there was no thought of trying half so many different styles of nozzle; but as the experiments went on, various lines of inquiry were prompted by the facts developed, and new nozzles were constructed or additional styles found in practical use were secured for experiment; for in matters like this one seems never to get to the end. Had time afforded, still other styles would have been constructed and experimented upon, and the experiment would have been made of directly measuring the number of gallons thrown into an opening of certain size, located at a given distance from nozzle near end of jet, thus obtaining a definite measure of the amount of water detached from the main stream and lost by spray with the different styles of nozzle. However, the range of the experiments actually made covers the subject well enough for all present practical purposes. It is believed that the conditions for accurate comparison were much superior to those generally available, and therefore the results are given with considerable detail, in order that future seekers for the "perfect nozzle" may save time by avoiding paths already pursued.

In one way the results of these comparisons were disappointing to the writer, for at the beginning he did not expect to find the plain cone superior in projecting power to all other forms tried. It was hoped that some form or modification would be found from which the jet would issue in a clear, transparent, polished stream, with a clean, glassy surface, and with little spraying for the first quarter of its course.

Out of the about forty different nozzles tested, there was not one which threw such a stream, and though the writer has had occasion to critically examine the streams from many other nozzles, he has not yet seen one which, under pressures of 30 pounds or upward, gave this transparent and undisturbed appearance, similar to that found, for instance, in the stream over a well constructed weir.

For pressures of 5 or perhaps even 10 pounds a smooth, transparent stream can be obtained; but with a good working pressure like 40

pounds there was always found even only an inch away from the orifice a "ground glass appearance" on surface of jet.

And a careful and minute examination showed that even at an inch distance from orifice of the best nozzles or within about one-thousandth part of a second of time after issuing into free air, minute drops were beginning to tear themselves away from the main body, this being induced, no doubt, by the retardation due to walls of orifice, and to the consequent great difference in velocities between particles in the different layers near outside of jet.

The conclusion derived from all these comparisons, made, many of them, under conditions where 6 inches difference in heights of jet for the same pressure could be readily distinguished, and with accelerations varying from a uniform acceleration nozzle (E) to a sharply contracted ring nozzle (k), was:

First.—That form of convergence of nozzle makes much less difference in jet than had been previously supposed.

That if shape be such that eddies or tendencies toward eddies or unsymmetrical currents are not induced, then it makes but very little, if any, difference in character of jet, whether the longitudinal section of nozzle be a straight taper, or a curve giving uniform acceleration in a given time, as proposed by Mr. Nagle and some others, or a curve giving uniform increase of velocity in a given distance, as proposed by Captain Shaw, or any smooth nondescript curve.

Second.—The plain conical nozzle B, shown in Fig. 16, in connection with the play-pipe shown in Fig. 13, gave a jet which was, on the whole, slightly superior to any other form tried. And the nozzle shown in Fig. 7, designed since the experiments as a standard form easily manufactured, is presented with the belief that it is as good a form for practical use as can be constructed.

There are many fancy "nozzles" to be found, the possessors of which claim for them wonderful merit, and nozzles are frequently brought forth at firemen's musters of which the form is a most jealously guarded secret; but I am disposed to regard these special merits as fanciful rather than actual, and to think that by giving great care to accurate form and high polish of a nozzle like that shown in Fig. 7 equally as good results can be attained.

Third.—Ring nozzles were found to possess no advantage whatever over smooth nozzles, but proved very slightly inferior in projecting

power and gave a greater amount of spraying; and though this difference was so small as to be only barely perceptible and of almost no practical moment, yet the use of ring nozzles should be discouraged as tending to deceive, since their only advantage is to make a show of playing a larger stream than is the fact.

COMPARISON OF EFFICIENCY OF NOZZLES AND PLAY-PIPES.—ROTARY MOTION, OR TWISTING OF CURRENT AT NOZZLE.—(See drawings of nozzles on pages 318 to 325.)

For a fair and very accurate comparison of two play-pipes it is necessary that the hose for at least 5 to 8 feet back from play-pipe lie in a plane. There may be a curve close to play-pipe; but it must be a plane curve, not warped or twisted or cork-screw-like in the slightest degree. For it seems that this warped, cork-screw-like or spiral approach tends to produce a rotary motion within the play-pipe; and as the water progresses through the play-pipe and nozzle, it being confined within a channel of diminishing diameter, the angular velocity of rotation of the water becomes greater as it passes along (like the water in the vortex of a whirlpool), and on issuing into free air this rotary motion, by the centrifugal force generated, acts to cause more scattering of the jet.

Comparison No. 1.—That this is the case was indicated by simultaneous observations as shown in Fig. 54, with two play-pipes of the same length fitted with nozzles of same size (1¼ inch smooth) placed side by side, elevated at 60 degrees, each fed by 50-foot pieces of hose of same kind and size (2½-inch solid rubber), with pressure at base of nozzle identical within $\frac{1}{10}$ pound. The only difference in the conditions affecting the jets was that while hose to one lay in plane curve of 90 degrees arc and 3 feet radius, the similar curve in hose to the other nozzle was about 2 inches "winding," or out of plane at center of arc, and thus was a slightly warped or helical curve. It was found, and confirmed by reversing position of nozzles while position of hose remained unchanged, that height and distance reached by the latter jet, in which the conditions favored a slight twisting of currents, was 3 to 5 per cent. less than that attained by the first mentioned jet, which had that part of the hose close to its nozzle lying in a plane curve.

This twisting of jet has been noted by many practical men, and its effect sometimes, I think, overestimated. Several patents have been taken out for devices for preventing the twisting of current. I con-

structed, especially for these experiments, the play-pipe shown in Fig. 18, with the expectation that these blades, which projected in far enough so that more than half the whole body of water lay exterior to a cylinder containing their inner edges, would practically control the twisting motion of jet, and that it would present the advantage over nozzle shown in Fig. 24 of less surface for internal friction and less liability to obstruction by a stone or other foreign substance passing through pipe.

It may be of interest to mention that it was learned subsequently (see Bowie's "Hydraulic Mining," page 50, also page 182) that this device of straight internal blades parallel to axis, and called a "Rifle," was patented twenty-five years ago and much used in the monitor nozzles for the hydraulic mines of California, and proved of very material advantage in removing the twist induced by the swivel joint at base of play-pipe. The term rifle blades is to some extent a misnomer, since the blades are put in with an entire absence of that twist, on which the advantage of rifling for a gun depends.

I was disappointed in the results attained, for it was found that these rifle blades, shown in Fig. 18, were not fully effective for preventing this rotary motion, even where the curve of hose was so slightly winding, as noted above, and moreover, it was difficult and expensive to insert blades with proper straightness in pipe of size of fireman's play-pipe.

Comparison No. 3.—When both lines of hose were laid in plane curves, and all arranged as shown in Fig. 54, no difference whatever was perceptible between stream from the "rifle barrel" and stream from the ordinary pipe of same size without the rifle blades; both being tried simultaneously side by side at same elevation, with pressure identical, and under conditions where a difference of one per cent. would have been readily noticeable.

The Silsby Manufacturing Company, well known as enterprising builders of steam fire engines, have introduced a special play-pipe of the form shown in Fig. 24, containing internal diaphragms for straightening the current. One of these pipes, with the two nozzles as shown, was kindly loaned me by the Messrs. Silsby. I tested it with especial care in the manner shown in Fig. 54 and again on another day in the manner shown in Fig. 55, and repeated these tests at a demonstration given in presence of our corps of inspectors and others.

Comparison No. 4.—In all cases when the hose near base of play-pipe lay in a plain curve, as shown in the figures, these experiments established with certainty the fact that the jet from this play-pipe with the internal partitions was in no way superior to that from the ordinary play-pipe shown in Fig. 13, but averaged not reaching so far by from 1 to 3 per cent. for the same pressure.

Comparison No. 5.—The Silsby pipes proved superior, however, when the lines of hose near the bases of the two play-pipes were lifted out from the blocks which held hose in a plane curve, and the hose was thus brought into a cork-screw-like form. The jet from the ordinary pipe without partitions was made less perfect by the rotary or twisting motion thus induced in the hose, while the jet from the Silsby pipe held its original character. For cases where this irregularity of curve of hose was made as great as would probably occur in ordinary practice, it was found that the spraying of jet within 25 feet of the Silsby nozzle was very noticeably less than from the other, and that the distance reached by main body of jet from Silsby nozzle was, say, 2 to 4 per cent. greater than distance reached by main body of jet from the plain nozzle. My conclusions regarding the use of "Rifle blades" or internal partitions on play-pipes of size adapted for 2½-inch hose is, that they are not advisable.

Rotary Motion and Disturbed Flow in Siamese Nozzles.

With the ordinary Siamese, by which two, three or even four hose streams are united into one at base of play-pipe, or united at a point within 25 feet of it, some arrangement similar to the rifle blades already mentioned seems absolutely necessary to the obtaining of a good clean jet. This is especially true of an arrangement like that shown in Figs. 43 and 44, where the union of the several lines of hose occurs close to base of play-pipe. The play-pipe for this was so made that the rifle blades shown in Fig. 45 could be removed.

Comparison No. 6.—The removal of these blades and partitions was tried repeatedly, and the effect on jet was very marked. When blades were removed, the twist of the jet as it issued was very plainly noticeable, and judging as closely as possible by the eye alone, it appeared that for a pressure of 50 pounds at Siamese, the spraying was fully 25 or perhaps 33 per cent. more when partitions were removed than when they were in.

Comparison No. 7.—A careful comparison between the merits of the partitions reaching to the center, and the blades reaching half way to center, both shown in Fig. 45, showed certainly that the partitions gave the better stream. That is, the stream was more solid, with fewer drops detached in the first half or two-thirds of its course. It was found, moreover, that it was much more difficult to construct the pipe with blades of proper straightness than to construct the partitions and keep their surfaces flat and out of twist. Even with the partitions shown in Fig. 45, the stream was less solid than desirable and less solid than I believe it possible to attain. It did not seem to lie within the power of these partitions to remove all the twisting and disturbed condition of flow induced by the Siamese and the elbow.

It happened that at about the time of my experiments a convention of the New England Firemen's Association was held at Haverhill, Mass., and an exhibition was made of 2-inch Siamese streams from pipes of three different kinds, made by different parties. I noticed that with each of these streams there was proportionally very much more spraying than from a $1\frac{3}{8}$ inch nozzle from a single line of hose, and am inclined to attribute it to the disturbed condition in which the water reaches the nozzle.

As soon as there is opportunity I hope to construct another Siamese in which the three portions of water from the three lines of hose shall be kept separated by thin partitions until after passing the elbow and until reaching the play-pipe proper. The water passages will, moreover, be so designed as to avoid sudden changes of section, and care will be taken that axis of each of the three streams shall make the bend by a curve lying in a plane. With such a pipe it is expected to avoid twist of current and get a more solid stream.

Advantage of Siamese Nozzles and Large Streams.

We may add a word here as to the reason for the especial merit of these large streams. They are, of course, available only in cases where the water supply is especially ample. At many large factories the fire pumps are of capacity sufficient to supply one of these in addition to a convenient number of single streams, and on fire boats they can ordinarily be provided for. If supplied by city steamers it will ordinarily require the united efforts of three steam fire-engines of the usual size to supply one 2-inch stream; yet in a serious conflagration it may often be

the case that one 2-inch stream will do far more execution in stopping the progress of the fire than would the three single streams. It is a fact well known to firemen, and one which the writer has had good opportunity to observe personally, that a small stream is often almost wholly evaporated in the flames of a very hot fire; whereas with these larger streams, even though the same quantity be evaporated as before, enough water may be left to pass through the flames and burning gases to reach and chill the coals themselves.

EXPERIMENTS ON COMPARATIVE EFFICIENCY OF DIFFERENT KINDS OF NOZZLES.—(See drawings of nozzles on pages 318 to 325.)

The most of these experiments were made with the apparatus arranged as shown in Fig. 54, and in a number of the more important cases further experiments on the same nozzles were made with apparatus arranged as shown in Fig. 55. In a number of cases, after making the experiments with nozzles in one position, their positions were reversed, to make sure that the jet next wall was not shielded from air currents or affected differently from the outer one in any way. In no case was proximity of wall found to affect jet. Care was also taken in each experiment to sight the two streams out of wind with each other for the first part of their course, and thus insure their having precisely the same angle of elevation.

Comparison No. 8.—1⅛-inch conical nozzle *B* (Fig. 16), compared with 1⅛-inch conical nozzle *A* (Fig. 13). Both these nozzles were tested on the same kind of play-pipe, and the nozzles themselves were exactly alike, so far as any measurement or appearance could indicate, excepting only that in nozzle *B* the slight corner at end of cone was carefully rounded off. Experiment was made at 50 pounds pressure at base of play-pipes, and with nozzles at 60 degrees elevation. There was no wind, and conditions were favorable for accurate observations. By four successive trials another observer and myself arrived independently at the conclusion that *B* threw its stream just a little closer—that is, with a little less spraying than *A*—but that difference was very small. We also concluded that main body of stream and also the extreme jet averaged reaching a height of about 6 inches greater than *A* for pressure of 50 pounds, or was superior by about three-fourths of 1 per cent. It took very close watching to see any difference between these two nozzles.

Comparison No. 9.—1⅛-inch nozzle *P* (Fig. 23) (screw nozzle, so-called), compared with nozzle *A* (Fig. 13). Nozzle *P* differed from *A* in that diameter at end of play-pipe was 1¼ inches instead of 1½ inches. The comparison was made at 50 pounds pressure, 60 degrees elevation, with still air and conditions favorable for accurate observation. Trying the two nozzles simultaneously side by side, as in Fig. 54, unquestionably the jet from the screw

nozzle was at its summit 4 feet below summit of jet from nozzle *A*, and extreme point at which jet from screw nozzle could be called a good fire stream was 5 to 7 feet less than the corresponding point on jet from nozzle *B*. The screw nozzle thus appeared on the whole to be from 5 to 8 per cent. inferior to *A* in projecting power. This difference was undoubtedly due to the smaller water-way at end of play-pipe. Nozzle *P* is sold about 15 per cent. cheaper than *A*, but having been proved to be less effective, its manufacture is to be discouraged.

This and the next experiment illustrated the necessity of having throat of play-pipe of ample size, and indicated that the old-fashioned sporting play-pipe, so-called, which was about 5 feet long and converged throughout its length on a uniform taper, was not adapted to give such good results as are to be attained by a play-pipe in which stream is continued of approximately full size up nearly to the point of delivery, and it was by reason of these experiments that diameter of throat or end of standard play-pipe shown in Fig. 7 was made 1¾ inches in diameter.

Comparison No. 10.—1¼-inch nozzle *J* (Fig. 23) (screw nozzle, so-called), compared with 1¼-inch nozzle *e* (Fig. 11). These experiments were also made with two pipes tried simultaneously side by side, arranged as shown in Fig. 54, pressure at base of play-pipe 50.0 pounds, angle of elevation of jet 60 degrees. From position on Washington roof I concluded that unquestionably nozzle *J* was inferior to nozzle *e*, and that its jet was not projected so high as for nozzle *P* by about 6 per cent. on the average. The extreme drops from nozzle attached to the larger play-pipe went 3 or 4 feet higher than for nozzle *J*, and the limit as a fire stream appeared to be 6 or 7 feet higher. Examining both jets from below, unquestionably the stream from nozzle attached to the larger play-pipe sprayed the least; thus, for 20 feet distance from nozzle nearly all of the water from nozzle *e* goes through a circle 2½ inches in diameter, while for nozzle *J* it scattered over a circle of about 3½ inches in diameter. Examining both jets from Pacific roof, the nozzle on the larger play-pipe was seen to give decidedly the closer stream. Repeating experiment with 40 pounds pressure, the maximum height of jet from nozzle *J* was, on an average, about 4 per cent. less than from nozzle *e*.

Comparison No. 11.—1⅛ inch truncated cone nozzle *C* (Fig. 17), compared with nozzle *A*. This nozzle *C* was constructed as a result of experiments on distribution of velocity (see Fig. 61). Reasoning by analogy from curves of distribution for conical nozzle *B* and curve for pipe of uniform diameter, it was evident that within certain limits the addition of a straight cylinder to end of cone increased the retardation of the filaments of water next the side. It was thought, therefore, that possibly there would be less retardation of the outer portion of jet if the straight part were almost wholly removed from the ordinary nozzle. Then, too, it was desirable to learn whether this straight part was of any real advantage, even if of no disadvantage. Nozzle *C* was compared with nozzle *B*, in manner shown in Fig. 54, with pressures of about

50, 75 and 105 pounds at base of play-pipe, and angle of elevation of 75 degrees. These two nozzles were also compared under the conditions shown in Fig. 55, with pressure of 50 pounds at base of play-pipe. From all these comparisons it was concluded that the difference between the jets was practically inappreciable, and though perhaps the height of jet from *C* was 1 per cent. less, certainly the difference in distance reached by main body of the two streams could not have averaged greater than 2 per cent. without being plainly noticed. I am inclined to think that if corner at end of taper had been smoothly rounded off as in *B*, Fig. 16, or as in the standard nozzle, Fig. 7, the comparison would have been slightly more favorable to *C*.

Comparison No. 12.—Nozzle *D* (Fig. 15) compared with nozzle *A*. These two nozzles were compared playing simultaneously, in the manner shown in Fig. 55, under a pressure of 50 pounds. It was not possible to detect any difference in the character of the two jets. This experiment was not made under such favorable conditions as some of the others, and a difference of 2 per cent. in distance reached may have escaped detection. The comparison is of interest, as showing that the enlargement of diameter of nozzle next end of play-pipe, customary with most makers, causes little if any interference with flow.

Comparison No. 13.—Uniform acceleration nozzle *E* compared with nozzle *B*.

In preparing apparatus for these experiments, the writer was led to design two nozzles of this form, from having noticed in several cases the smooth, transparent, undisturbed appearance of a stream falling vertically from the contracted end of a pipe, and falling by direct influence of gravity on the particles forming the jet, rather than by the propulsion from a high pressure within the pipe. A very little study showed the form to be that due to uniform acceleration of velocity, and though the analogy was of course not strong enough to amount to a real reason why the form of this smooth and undisturbed jet should serve as a good form for a nozzle, yet the experiment was thought worth trying, and two nozzles were constructed with great care. One of these is shown in Fig. 14 and the other in Fig. 18. They were bored and polished to fit templets which were laid out with micrometer calipers and cut with great care. The templets probably nowhere varied more than, say, two or three thousandths of an inch from the mathematical curve, and the nozzle as finally finished apparently nowhere diverged more than $\frac{5}{1000}$ inch from the mathematical curve, and both were smoothly polished, and in the one shown in Fig. 14 the joints were so finely fitted that they could hardly be seen from the inside. The elements for the curves were, for Fig. 14, diameter at beginning, 2½ inches; diameter at end, 1.133 inches.

Distance of beginning from plane of zero velocity, 2.99 inches. Length along axis 7 inches, followed by 1 inch in length of uniform diameter, with the slight corner between the curve and the straight part rounded off.

For Fig. 18, diameter at beginning, 1.70 inches; diameter at end, 1.122 inches; distance of up-stream end from plane of zero velocity, 0.712 inches; length of curve along axis, 3.00 inches; followed by 1 inch in length of uniform diameter; these tapering and cylindrical surfaces merging into each other almost imperceptibly.

Subsequent to the making of the nozzles my attention was called to a paper by Mr. A. F. Nagle, in Transactions Am. Soc. M. E. for 1888, page 505, on the Best Form of Nozzles, etc., in which Mr. Nagle expressed the opinion that this form of nozzle must be the best. I therefore experimented with it with much interest.

A comparison of nozzle *E*, Fig. 14, with nozzle *B* on the pipe shown in Fig. 13, was made in the manner shown in Fig. 55 with 50 pounds pressure on two different occasions, one time being at the exhibition made in the presence of the corps of inspectors. The closest inspection failed to show any material difference in the solidity of the streams or in the distance reached. If anything, the jet from the uniform acceleration nozzle sprayed the most within the first 40 feet of the course and was a little the less solid at the further end. These conditions were favorable for a very accurate comparison. Examining the two jets near orifice, there was no appreciable difference noticed in smoothness at surface of jet.

A comparison was also made at the site shown in Fig. 54 at very high pressure, trying one nozzle at a time, first *B* and then *E*. In this comparison the general result was that at about 105 pounds static pressure at base of play-pipes the height of jet as a good fire stream was fully 5 per cent. less for the uniform acceleration nozzle than for the conical nozzle *B*, but the height to which the extreme drops were thrown appeared much the same with each.

Comparisons Nos. 13 and 14 conclusively show that this "uniform acceleration" curve possesses no special merit as a form for a nozzle.

On looking over Mr. Nagle's paper, it does not appear that his argument in favor of this form for nozzles is a logical one at all, any more than the mere notion mentioned on page 88, which led me to have two nozzles of the form made.

In Professor Merriman's recent valuable treatise on hydraulics, too, the mathematical derivation of the curve for a uniform acceleration nozzle is presented as though there were good reason to expect this form of nozzle to be the best; but in both these demonstrations must not the starting point, that this uniform acceleration of velocity will give the best results, be classed as resting on a notional rather than a rational basis? It may be mentioned that in any case the derivations of the curve, as given, are merely convenient approximations and are not mathematically exact, for velocities of all particles in each plane of section are assumed to be uniform, which we well know not to be the case; and even if they were uniform at end, the successive surfaces in which all the particles issuing in a given plane would be found as they approached orifice would not be planes, but surfaces of revolution subtending equal distances on the various paths of the fluid fillets from center to circumference. Why should uniform acceleration, which involves supposing the force in its full magnitude applied instantly, continued uniform for a brief interval then all instantly removed, be expected to give better results than a force gradually applied and gradually increased in magnitude, as is the case in a plain conical nozzle? Is not the argument as good in one case as in the other, and of no certain value in either? And is not Mr. Nagle's argument as to proper form for diverging tubes, in the paper above referred to, equally without substantial basis? Since extreme divergence near end would probably lead to formation of eddies.

I fancy that for the best efficiency of nozzles as for turbines it is not a question of having the curves follow any given equation, but almost wholly a question of making curves of such form that eddies or tendencies toward eddies or unsymmetrical distribution of velocity may be avoided.

One other criticism may be made on Mr. Nagle's nozzle (shown in Fig. 128, page 506, Transactions Am. Soc. Mech. Eng., 1888), viz.: After increasing the velocity to almost its extreme limit, the water is carried within the walls of nozzle for a comparatively long distance, thus tending to incur more loss by friction against walls and consequent greater retardation at side of jet than if the velocity were kept low up nearly to end of nozzle.

It has been thought well to discuss this form of nozzle at greater length by reason of the prominence given it by the publications of Mr. Nagle and Professor Merriman, already referred to.

Comparison No. 14.—Comparison of small uniform acceleration nozzle *F* (Fig. 18), on conical pipe shown in Fig. 13, with nozzle *B* (Fig. 16) placed on a similar pipe. These were compared in manner shown in Fig. 54 at 60 degrees elevation with 50 pounds pressure. Careful examination failed to show any appreciable difference in the character of the jets from these two nozzles; certainly the difference in height to which jets were projected was not over one per cent.

Comparison No. 15.—The bottle-shaped nozzle *L* (Fig. 20) compared with conical nozzle *B*. Both were attached to play-pipes of the kind shown in Fig. 13, and were tested side by side in the manner shown in Fig. 55, at a pressure of fifty pounds. Hardly any difference could be detected in distance reached by the two jets, though, on the whole, that from *L* appeared to be slightly inferior. The difference, if any, was probably not greater than two per cent. This comparison was rather quickly made, however, and although no reason appears for doubting its accuracy, it should be understood that the comparison was less complete in its range than most of the others.

Comparison of Choke-Bore Nozzles.

These nozzles termed "choke-bore" nozzles were constructed with a slight contraction of water-way close to the orifice, with the idea that possibly this slight contraction of water-way might force those particles of water next to the outside of jet a little ahead of their normal position, and thus tend to prevent the retardation at side of jet (shown in Figs. 61 and 62). The reason for thinking that possibly this might be the case was that the writer, in measuring, a number of years since, the distribution of velocities in an open river channel in the vicinity of a bridge pier, found the obstruction by the pier to force the curve of velocities slightly ahead at points a few feet from the pier.

In making flume measurements, too, of the quantity of water passing a mill-race, by the Francis method, with floating tubes, the writer had, in the course of gaugings at certain race-ways where there was a contraction of width of flume immediately above the site of the gauging, found a velocity at a short distance from side even greater than that in the center. Various other somewhat similar phenomena noted in the writer's experience indicated as probable that when an obstruction is placed in a water-course where there is a certain rate of flow, the inertia of the water moving immediately adjacent to this obstruction tends to produce the increase of velocity necessary to make up for lost area of section, mainly in the immediate vicinity of the obstruction; rather than to let this necessary increase of velocity be uniformly distributed throughout the cross-section.

It looked reasonable to suppose that some such action as this might obtain in nozzles, and prior to the experiments it was thought by the writer that perhaps this might prove a sufficient cause for the superiority of a ring nozzle having some certain projection of shoulder over the smooth form.

Various nozzles as mentioned next in order were therefore constructed one after another, in the effort to increase the proportional velocity at side of jet. We may say at once that our hopes were disappointed and none of these choke-bores proved to have any merit over the smooth cone.

Comparison No. 16.—The long choke-bore, nozzle *Q*, Fig. 21, was therefore constructed, and was compared with the conical nozzle *B*, under the conditions shown in Fig. 54, with 50 pounds pressure at base of play-pipe, and an angle of 60 degrees elevation. It appeared that nozzle *Q* was in nowise superior to nozzle *B*, and that in general the extreme height of its jet was not so great by about 1 foot. Judging of the two as good fire streams at 40 pounds pressure, the highest point at which they could be classed as such was 3 or 4 feet less with *Q* than with *B*. On the whole, I conclude nozzle *Q* was from 2 to 4 per cent. inferior to nozzle *B* in projecting power.

Comparison No. 17.—The short $1\frac{1}{4}$-inch choke-bore nozzle *H*, Fig. 19, compared with conical nozzle *B*. Nozzle *H* was constructed at the same time and with the same object in view as for the experiment last described. Experiment was made with 50 pounds pressure and also with 40 pounds pressure at base of play-pipes, and with apparatus arranged as shown in Fig. 54. I concluded nozzle *H* to be in nowise superior to nozzle *B*, and that the summit of its jet did not average so high by about 3 feet.

Nozzle *H* was then tested side by side with nozzle *J*, and the streams from the two appeared almost identical. Thus, on the whole, it was concluded that jet from nozzle *H* reached a distance 3 per cent. less than that from *B*.

Comparison No. 18.—Short $1\frac{1}{8}$-inch choke-bore nozzle *q* compared with conical nozzle *B*. Test made under conditions shown in Fig. 54, 60 degrees elevation, 50 pounds pressure, but with only one stream playing at a time. From trying first one, then the other, and then repeating, my assistant and myself independently reached the conclusion that *q* was probably slightly inferior to *B* in projecting power. It was difficult to perceive any difference, and it appeared certain that difference was not more than 1 per cent. Apparently the stream from *q* wavered more and broke into drops a little sooner near the top.

Comparison No. 19.—$1\frac{1}{4}$-inch taper ring *p* (Fig. 35) compared with $1\frac{1}{8}$-inch smooth nozzle *B*. The taper-ring nozzle gave a jet which at time of measurement of co-efficient of discharge was seen to be so plainly inferior by reason of greater spraying and apparently disturbed condition of current that the more complete comparison as per Fig. 54 was not made.

Comparison No. 20.—Nozzle *K* (Fig. 22) compared with nozzle *B*. These two nozzles were compared playing simultaneously, under the conditions shown in Fig. 55, at 50 pounds pressure at base of pipe. An increased amount of spraying due to the obstruction in water-way was very evident, and distance reached by jet was very appreciably less than from nozzle *B*, say 10 per cent. The difference in distance reached by jet was not determined with great exactness by reason of a slight wind which existed at the time.

Comparison No. 21.—Comparing the Silsby long cone with the Silsby short cone (Fig. 24), by trying first one and then the other and then repeating, at an elevation of 60 degrees with 50 pounds pressure, it appeared that there was no material difference in the maximum elevation attained by the two jets, but on the whole it seemed as though the long nozzle gave a slightly more solid stream.

Comparison No. 22.—Nozzle *A*, on flexible play-pipe of uniform diameter shown in Fig. 26, compared with stream played simultaneously from nozzle *B*, attached to the ordinary tapering play-pipe shown in Fig. 13. Experiments

were made with apparatus arranged as in Fig. 54, with pressure at 50 pounds and an elevation of 75 degrees. The conditions for observation were excellent in every way, and I examined jets from both the Pacific and the Washington roof. Jet from the nozzle on flexible pipe, shown in Fig. 26, was on an average fully 1 foot to 1½ feet inferior in height reached, to jet from nozzle *B*. (Experiment No. 1 would indicate that probably half of this difference was due to difference in the nozzles, leaving the other half to be accounted for by difference in the forms of play-pipes.) On the closest examination I could detect no difference in the spraying quality of the two jets. Drops of water at summit of both streams were about ⅜ inch in diameter. This is of special interest as showing that the special form of play-pipe has little influence on character of jet so long as diameter at end of play-pipe and beginning of nozzle proper is reasonably large. And it is of interest also as showing that this flexible play-pipe, which is quite popular among firemen, is, when kept straight, but very little inferior to the best in projecting power.

Comparison No. 23.—Play-pipe shown in Fig. 25 with rough interior surface and 1-inch outlet, compared with nozzle of same size (*c*) on play pipe with smooth interior shown in Fig. 13. The comparison was made as shown in Fig. 54 at 75 degrees elevation and with 50 pounds pressure, and all conditions were very favorable for accurate observation.

The play-pipe shown in Fig. 25 was of a kind often found in the market. It was made from a brass casting, and its outside was nicely polished, while its inside was left rough as it came from the foundry sand. It was found that the jet from this reached a height 12 feet less (about 13 per cent.) than jet from the smooth play-pipe.

There was one very remarkable fact, however, noticed, which is worthy of mention. The drops from this rough pipe were at the summit of double the size of those from the higher jet. They averaged about ½ inch in diameter. This was unquestionably evident, as from the station on the Pacific roof I stood close to summit of jet.

In this connection it may be remarked that in comparing the jet from small Siamese, which by reason of its too-small play-pipe reached a less height than jet from the larger barrel, the drops composing the summit of the inferior jet were much larger and closer together than those from the jet attaining the greater height, being, in fact, irregular lumps of water rather than drops.

Ring Nozzles *vs.* Smooth Nozzles.

Considerable interest attaches to the comparison between ring nozzles and smooth nozzles, for although at the present time the best authorities agree in the opinion that a smooth nozzle will throw equally as

good a stream as a ring nozzle, yet there are many experienced firemen who consider that the ring nozzle throws a cleaner and more solid stream, and who maintain their opinions very positively. It was intended, if possible, to settle this question by the following experiments, and the result of the following comparison shows that the ring nozzle possesses no advantage whatever over the smooth; but the difference in the character of the streams from the two nozzles was almost inappreciable, both as to distance attained by the extreme drops and also by the main body of the jet. As a result of all the comparisons it was concluded that the extreme height attained by jet from smooth nozzle was about 1 per cent. greater than height of jet from ring nozzle discharging the same quantity of water under the same pressure. The spraying from the ring nozzle was certainly slightly greater than for the stream from the smooth nozzle. On the whole, it was concluded that the distance to which the stream could be classed as a "good fire stream" was 3 per cent. less with the ring nozzle than with the smooth nozzle, and since the ring nozzle discharges only about three-fourths as much water as does the smooth nozzle with the same size of orifice, there is, in addition to difference noted above, a still further superiority in distance reached by a stream from a smooth nozzle over a stream from a ring nozzle of the same diameter. Although the difference in the reach and solidity of jets from ring nozzles or from smooth nozzles is so small as to be almost of no practical importance, yet the use of the ring nozzle is to be discouraged, since it makes a show of playing a larger stream than is the fact. Details regarding the comparisons are given below:

Comparison No. 24.—$1\frac{1}{4}$-inch square ring nozzle (*i*) of the ordinary form, compared with $1\frac{1}{8}$-inch smooth nozzle *B* under conditions shown in Fig. 54, at 50 pounds pressure and 60 degrees elevation of jet. The height of jet from ring nozzle was from 1 to $2\frac{1}{2}$ per cent. less than from smooth nozzle, and unquestionably there was more spraying from the ring nozzle jet.

Comparison was then made with both jets vertical, first with 50 pounds pressure and afterwards with 40 pounds pressure at base of play-pipe. As the mean of these two experiments, ultimate height appeared the same within less than 1 per cent., but water at top of jet was certainly in less solid and compact form in jet from ring nozzle than from smooth nozzle.

Comparison No. 25.—In order to make the comparison still more conclusive, a ring nozzle was constrnced (see nozzle *k*, Fig. 33) of such diameter as to discharge almost exactly the same quantity of water as did the $1\frac{1}{8}$-inch smooth nozzle *B;* but as finally made the diameter proved a few thousandths of an inch larger than intended, and thus the discharge was nearly 2 per cent. greater than the discharge from *B*. This difference was of course too small to affect

the following comparison. This nozzle *k* was constructed with especial care to have the face of the ring as nearly in a plane surface as possible, and the edge was finished as sharply as brass could be worked and was not dulled by polishing with emery cloth. The several comparisons made between jet from this nozzle *k* and from nozzle *B* are considered very satisfactory.

The comparison was made first under the conditions shown in Fig. 55, the two jets playing simultaneously in each case, and the pressures being adjusted so as to be equal within $\frac{1}{10}$ pound or less. The conditions were such that a difference of less than 1 per cent. in distance reached could be easily detected. The two jets agreed with surprising closeness. It was almost impossible to detect any difference in their path until very near the end; but it was evident that the extreme drops from the ring nozzle attained in general a distance about 1 per cent. less than those from the smooth nozzle, and as a "good fire stream" it was judged that the jet from smooth nozzle attained a distance from 2 to 4 per cent. greater. Unquestionably there was a little more spraying on the ring nozzle jet than on the smooth. This experiment was afterward repeated, both as shown in Figs. 54 and 55, in the presence of the corps of inspectors, and after making one experiment the nozzles were reversed in position, each being placed on the other's play-pipe, all else remaining the same. The general result was again found as already stated.

Ring nozzle *k* was afterward compared with smooth nozzle *B* at the unusually high pressure of about 103 pounds at base of play-pipe and 75 degrees elevation, the comparison being made at site of Fig. 54, but with only one stream playing at a time. So far as could be seen the extreme drops of the two jets reached the same height, and main body of jet at summit was about 2 per cent. less for the ring nozzle, and the height at which the two were classed as "fair fire streams" was considered to be 5 per cent. less for the ring nozzle.

Comparison No. 26.—Ordinary undercut ring nozzle *n* (see Fig. 30) compared with smooth conical nozzle *B*. This undercut nozzle, so-called, is of a style which is considered by many good firemen to give a superior stream; but the reason usually given for this opinion is rather more amusing than scientific, namely, that it holds back the slow, disturbed currents of water near side of play-pipe and gives out only the central swift and least disturbed portion for the jet. Comparisons between these two nozzles, made at 60 degrees elevation, as in Fig. 54, and also at a lower elevation as in Fig. 55, failed to show any appreciable difference in jet from this form of nozzle and the more common square ring nozzle last described.

Comparison No. 27.—Narrow ring nozzle *l*, Fig. 29, was constructed subsequent to a portion of the above experiments, partly from the same motives that led to the construction of the choke-bore nozzles already mentioned. It will be noted that its discharge was almost identical in quantity with that from smooth conical nozzle *B*, and several comparisons were made therefore between these two playing side by side. The general result of these comparisons was to indicate that this narrow ring gave a stream slightly superior to that from the wider rings, and it could not be said that it was any inferior to the jet from the smooth nozzle. It appeared certain, however, that it was not superior. In this comparison the pressure, though generally 50 pounds, was carried up to 100 pounds in one experiment made at an angle of 75 degrees.

Comparison No. 28.—Undercut cap *o*, Fig. 32. By reason of failure to find suitable opportunity when there was no disturbance from wind, this comparison was not carried out with such detail as the most of those already described. It was found by the preliminary experiments at 50 pounds pressure that the jet from this nozzle was certainly very much poorer than from the smooth conical nozzle, and apparently poorer than from any other form of nozzle tried. The form of jet appeared slightly unstable, as though due to an internal disturbed condition. There were also a few lines or nodes occasionally apparent on exterior of jet at a distance of a foot or so from nozzle, but they were not sharply defined and their position was unstable.

Comparison No. 29.—Siamese nozzle with 3-inch straight barrel compared with Siamese nozzle with play-pipe or barrel 4 inches in diameter at base (see Fig. 44). Comparison made at 75 degrees elevation at site shown in Fig. 54, trying first one and then the other. In both these comparisons it was found that the height of extreme drops and of the main body of stream at its summit was from 6 to 8 per cent. less than for the Siamese with the smaller play pipe. It appeared that the jet from the larger play-pipe could be classed as a "good fire stream" for a height from 8 to 12 per cent. greater than the jet from the smaller, but the remarkable fact was noted that the size of drops and general solidity of jet was better with this smaller play-pipe.

Comparison No. 30.—Several comparisons were made between the smooth 2-inch Siamese nozzle, and also the 1¾-inch nozzle and the ring nozzle shown in Figs. 39 and 41. So far as could be detected these ring nozzles gave jets equally as close as those obtained from the smooth nozzle, but, as mentioned previously, I do not consider that any of these Siamese jets were so solid and free from spraying as it is possible to obtain.

In closing this subject, the writer would suggest two rather fine points upon which further experiment would be of practical value; the first is, to determine by careful experiment the conditions as to form or smoothness of play-pipe necessary to insure that the drops into which the jet or stream breaks toward the end be of the largest possible size; for such coarse drops will be less likely to evaporate in passing through the flames.

It so happened that in two cases (comparisons Nos. 23 and 29), where the nozzle as a whole was inferior in projecting power, the drops at end of the lower jet were certainly of double the size of those belonging to the other stream playing at its side.

It is surmised that this may have been due to the action of the greater friction loss within the play-pipe, etc., in removing or absorbing the influence of irregular currents originating within the hose.

The second point suggested for further experiment is, that when it is

found expedient to make further tests on jets, it would be well to get some quantitative measurement of the proportion of the whole volume leaving the nozzle, which attains some given position near end of jet.

Thus, for instance, with a jet under 50 pounds pressure, arrange a tank or weir to measure the water received into a vertical opening say 2 feet square, placed, say, 80 or 100 feet distant from nozzle and at an elevation of 30 feet above it.

This would settle conclusively whether there was a very considerable difference in the thinness to which jets whose extreme drops reached the same distance tapered out toward the end, as stated by Captain Eyre M. Shaw in his book on fire protection, and thought to be dependent on the form of nozzle, but of which the writer found no evidence.

The writer did not find any very marked difference between different nozzles in this respect, although it was looked for; and although such eye estimates are, of course, less positive than actual measurements, it is to be said in this connection that by reason of having such a near point of view for examining the jets under comparison, and also by reason of their being seen against the excellent background furnished by the adjacent brick wall, and also especially from the fact that in most cases the comparisons were made with the two jets playing side by side, it is not thought that the comparisons can have been very much in error.

Distribution of Velocity in Jets.

This is a matter upon which the writer was unable to find that previous experiments had been made. The following investigation was undertaken, therefore, in the hope that it might lead to improvement in form of nozzle.

The apparatus is shown in Fig. 60 and the results in Figs. 61 and 62.

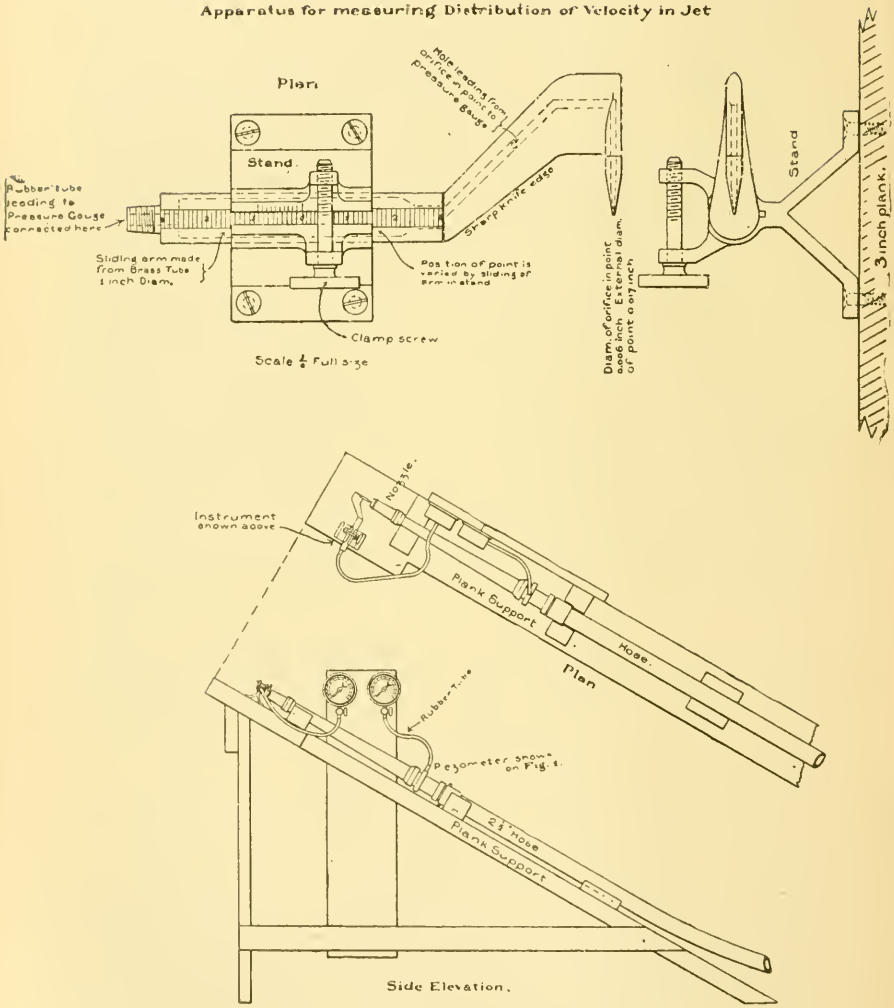
The instrument shown in Fig. 60 was designed by the writer specially for these experiments and proved all that could be desired. The stiff form and secure clamp prevented all vibration, and the sharp knife-edge of bar opposed so little resistance to current that it was found to cause no disturbance whatever in the relative distribution of velocities. In other words, the curves of distribution were in almost every case symmetrical on the two sides of center (see Fig. 62), although the velocities at side of jet furthest from stand were measured with the bar of instrument passing almost entirely through jet and cutting it in halves. The main instrument was of brass, but the small point was composed of an alloy of gold tipped with iridium, and was in fact the tip from a stylographic pen having an orifice of unusually small size.

This point was carefully re-finished by a watchmaker in order to get its minute end faced up true and square. The diameter of orifice was about six-thousandths of an inch and external diameter of the point at its end 0.017 inch. By means of the vernier and scale engraved on the stand and body of instrument it was possible to determine position of point within a few thousandths of an inch. The greatest care was taken that even the most minute leakage from the gauge connections should not exist, as it was realized that by reason of the small size of orifice such leakage would have proved fatal to accuracy.

The two gauges used were new and excellent standard test gauges of the Bourdon style, graduated to single pounds, but read by estimation to tenths of pounds. At the close of the experiments these gauges were both compared with one of the mercury columns and proper corrections determined. These gauges were selected rather than the mercury columns by reason of their greater portability ; but the writer now regrets that he did not make use of the mercury gauges, for since the rest of the apparatus proved accurate and the measurements definite to an unexpected degree, it would have been of some interest to have had means for measuring the pressures with certainty to within a few hundredths of a pound.

Fig. 60.

Apparatus for measuring Distribution of Velocity in Jet



Complete Apparatus.
Scale 1/16 of full size.

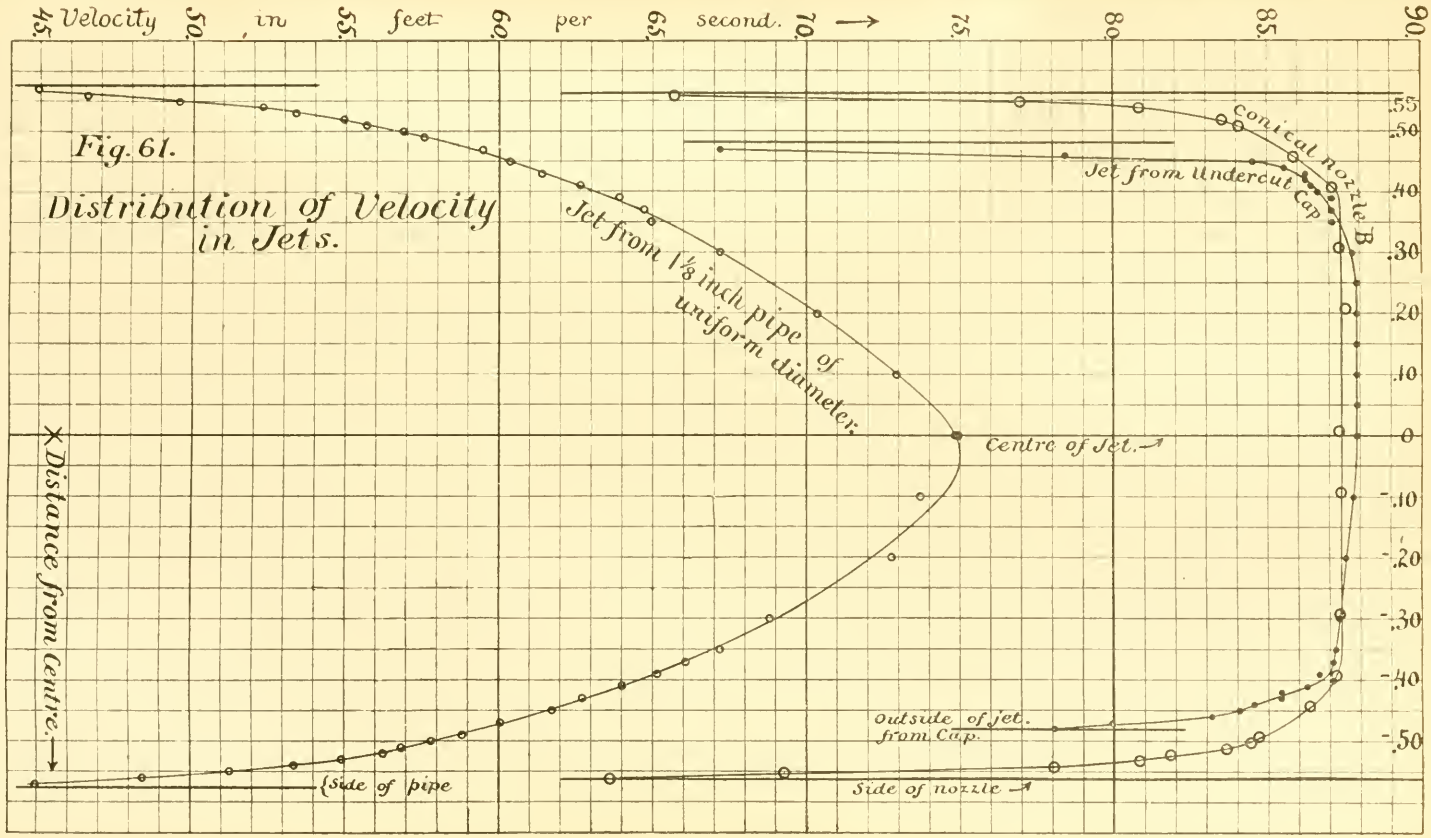


Fig. 61.

Distribution of Velocity in Jets.

These Bourdon gauges, however, worked unusually well, and there is no reason to think their indication more than, say, two-tenths of a pound in error.

Water was supplied from the city reservoir through the hydrants and the 2½-inch hose, and the pressure was regulated by the hydrant gate.

There were jets from about a dozen different nozzles experimented upon in this manner.

A single sheet of the reduced notes is reproduced here to illustrate the method followed (see Table No. 13).

As it will be remarked that the time occupied by each observation was rather short, we may remark that care was taken to give the gauge ample time to establish its equilibrium and come to rest after each change in position. Under these very high velocities it took but a very short time for the gauge to settle. This particular sheet happens to be one taken after considerable facility in the quick manipulation of the instrument had been acquired.

In all the plottings the co-efficient of the instrument has been assumed to be unity and the velocity is thus taken as that corresponding to the pressure shown by the gauge attached to the instrument. The velocity was generally measured at from $\frac{1}{8}$ to $\frac{1}{2}$ inch away from the orifice, and in case of the contracted streams the velocity instrument was set with its point in the plane of greatest contraction.

The diameters of the jets were calipered with much care, but it was found difficult to determine these diameters nearer than $\frac{1}{100}$ inch, by reason of the incipient spraying already referred to.

Fig. 61 gives three representative curves plotted on a magnified scale. The plotting is made with actual distances from center as ordinates and actual velocities in feet per second as abscissæ.

A point which at once strikes one as of special interest is the difference between the jet from a conical nozzle or an orifice with sharp contraction, and a jet from a straight pipe of uniform width. The velocity in the jet from the nozzle or sharp-edged orifice is seen to be almost absolutely uniform for about four-fifths of the whole distance from center outward to circumference, while the jet from the pipe of uniform diameter is retarded all the way from the center outward.

This pipe of uniform diameter it should be said was a good, straight, average, smooth piece of brass tubing 5 feet long and 1⅛ inches internal diameter. It was not "seamless drawn" as would have been preferable,

but was "brazed tube," and there was thus a slight difference in character of service along this seam, which is perhaps the cause of the curve shown in Fig. 61 not being quite symmetrical. This 5-foot piece of $1\frac{1}{8}$ tubing was connected to the end of one of the $1\frac{1}{8}$-inch conical nozzles by appropriate screw threads and couplings, and this nozzle was in turn attached to play-pipe in the same way as shown in the lower sketch of Fig. 60, except that of course it carried the play-pipe, etc., farther up stream along its plank support.

The character of the jet from the straight pipe was very much inferior to that from a conical nozzle; it sprayed and spread out much more quickly.

It thus appears that excessive retardation of velocity in outer layers tends to increase the spraying and to decrease the distance to which the jet will retain its solidity. This might naturally be expected.

These experiments on distribution of velocity are also of great interest in illustrating the reason for the value of the co-efficient of discharge for a tapering nozzle. They indicate that the reduction of its value below unity is due only to the retardation of velocity of the outermost fluid fillets caused by friction against walls of orifice.

That the velocity within the central portion of the jet is very nearly, if not quite, that theoretically due to the pressure, is indicated by the following comparison:

Nozzle experimented upon.	Pressure indicated by Gauge at base of Play-pipe, corrected for gauge error and reduced to mean value.	Static Pressure corresponding corrected for Velocity past Piezometer.	Theoretical Velocity due Static Pressure at base of Play-pipe.	Velocity indicated by Instrument in central portion of jet (corrected).	Difference.
	Lbs. sq in.	Lbs. sq in.	Ft. p. sec.	Ft. p. sec	Ft. per sec.
A (conical)	50.0	52.0	87.89	87.45	+0.44
B (conical)	50.0	52.0	87.89	87.33	+0.56
c (truncated cone)	50.0	52.0	87.89	88.54	—0.65
E (unit. accel.)	50.0	52.1	87.97	87.20	+0.77
i ($1\frac{1}{4}$ sq ring)	50.0	51.8	87.71	87.45	+0.26
n ($1\frac{1}{4}$ u. c. ring)	50.0	51.6	87.54	87.12	+0.42
k (1.314 sq. ring)	50.0	52.1	87.97	87.73	+0.24
l ($1\frac{3}{8}$ sq. ring)	50.0	52.0	87.89	87.40	+0.49
o ($1\frac{1}{4}$ cap)	50.0	51 1	87.12	87.86	—0.74
Average difference					+0.20 ft. per second.

Average proportional difference between theoretical velocity and actual velocity in central portion of jet = 0.00227.

We should naturally expect that the friction within the play-pipe proper would absorb some force, and thus that the instrument velocity might be very slightly less than the theoretical velocity due to pressure measured at base of play-pipe.

This difference actually found is so very slight that the small gauge errors might account for it, therefore it is a matter of regret that the more accurate mercury gauges were not used.

We can, however, I think, safely say that velocity at central portion of jet equals the theoretical velocity due the head at base of play-pipe, providing play-pipe be short, of good diameter and smooth.

Upon Fig. 62 as many representative curves have been traced as was possible without so confusing the lines as to make the diagram of little or no value. The curves obtained with the other nozzles are all similar; even that with the smooth 2-inch nozzle for Siamese was similar near the side. We see revealed by these curves the interesting fact that about two-thirds of the whole distance from center to circumference the velocity remains the same as at center, and that at $\frac{1}{16}$ inch from the wall of this orifice the velocity was only 5 per cent. less than at center of jet.

The difference between the relative arrangement or distribution of velocities for the contracted nozzles or ring nozzles and the smooth nozzles is less than I had expected to find, especially so in the case of jet issuing from the re-entrant sharp tube (*o*).

It will be seen, however, that with *o* there was less retardation of the fluid fillets next outside of jet than with *B*, and the reason the form of jet was no better is probably that there was a disturbed condition or unstable motion within the jet as it issued, due to some unstable equilibrium in the currents approaching the orifice.

It is of interest to note that with the uniform acceleration nozzle *E*, Fig. 14, the influence of the length which current travels within nozzle after raised to a high velocity is plainly shown in the curve of distribution of velocities by a greater retardation near side.

On the other hand, the velocity close to outside of jet from the truncated cone nozzle is less retarded than the jet from any other form of nozzle tested, except the ring nozzles.

That the observations are probably accurate is indicated by the closeness with which the observations on the two sides of center agree.

It is of interest to compute the total retardation exhibited by one of

TABLE No. 13.

Distribution of Velocity in Jets. $1\frac{1}{8}$ Inch Uniform Acceleration Nozzle (E).

(Sample Sheet of Observations.)

Time—October 6th, 1888.	Scale Reading.	Distance from Center.		Gauge at Base of Play-pipe.					
		Actual.	Proportional.	Observed Reading. (Ashcroft Gauge).	Reading Corrected for Gauge Error.	Mean Value for Comparison.	Variation from Mean Value.	Correction to Mean Value for Velocity past Piezometer.	Static Pressure.
P.M.							Per Cent.		
3 37	— .00	+ .011	.019	50.0	49.5	50.0	1.0	2.1	52.1
3 38	— .10	— .089	.157	50.0	"	"	"	"	"
3 38	— .30	— .289	.510	50.0	"	"	"	"	"
3 39	— .35	— .339	.598	50.0	"	"	"	"	"
3 40	— .37	— .359	.633	50.0	"	"	"	"	"
3 42	— .39	— .379	.668	50.0	"	"	"	"	"
3 43	— .41	— .399	.704	50.0	"	"	"	"	"
3 43	— .43	— .419	.739	50.0	"	"	"	"	"
3 44	— .45	— .439	.774	50.0	"	"	"	"	"
3 45	— .47	— .459	.810	50.0	"	"	"	"	"
3 46	— .49	— .479	.845	50.0	"	"	"	"	"
3 47	— .50	— .489	.862	50.0	"	"	"	"	"
3 48	— .51	— .499	.880	50.0	"	"	"	"	"
3 49	— .52	— .509	.898	50.0	"	"	"	"	"
3 50	— .53	— .519	.915	50.0	"	"	"	"	"
3 51	— .54	— .529	.933	50.0	"	"	"	"	"
3 52	— .55	— .539	.951	50.0	"	"	"	"	"
3 53	— .56	— .549	.968	50.0	"	"	"	"	"
3 54	— .57	— .559	.986	50.0	"	"	"	"	"
3 54	— .58	— .569	1.004	50.0	"	"	"	"	"
3 55	— .59	— .579	1.021	50.0	"	"	"	"	"
3 57	+ .00	+ .011	.019	50.0	"	"	"	"	"
3 58	+ .10	+ .111	.196	50.0	"	"	"	"	"
3 59	+ .20	+ .211	.372	50.1	49.6	"	0.8	"	"
4 00	+ .30	+ .311	.549	50.0	49.5	"	1.0	"	"
4 01	+ .35	+ .361	.637	50.0	"	"	"	"	"
4 01	+ .37	+ .381	.672	50.0	"	"	"	"	"
4 02	+ .39	+ .401	.707	50.0	"	"	"	"	"
4 02	+ .41	+ .421	.743	50.0	"	"	"	"	"
4 03	+ .43	+ .441	.778	50.0	"	"	"	"	"
4 03	+ .45	+ .461	.813	50.0	"	"	"	"	"
4 04	+ .47	+ .481	.849	50.0	"	"	"	"	"
4 05	+ .49	+ .501	.884	50.0	"	"	"	"	"
4 06	+ .50	+ .511	.901	50.0	"	"	"	"	"
4 06	+ .51	+ .521	.919	50.0	"	"	"	"	"
4 07	+ .52	+ .531	.937	50.0	"	"	"	"	"
4 08	+ .53	+ .541	.954	50.0	"	"	"	"	"
4 08	+ .54	+ .551	.972	50.0	"	"	"	"	"
4 09	+ .55	+ .561	.990	50.0	"	"	"	"	"
4 10	+ .56	+ .571	1.007	50.0	"	"	"	"	"
4 11	+ .57	+ .581	1.025	50.0	"	"	"	"	"
4 12	+ .58	+ .591	1.043	50.0	"	"	"	"	"
4 14	+ .59	+ .601	1.060	50.0	"	"	"	"	"

Diameter of orifice, 1.133 inches. ∴ radius, 0.567 inch.

TABLE No. 13—(*Continued*).

Gauge at Base of Play-pipe.	Gauge Connected to Velocity Instrument.						Ratio to Velocity at Center, 87.28.
Velocity Corresponding to Static Pressure.	Observed Reading. (Crosby Gauge No. 2.)	Correction for Gauge Error.	Correction for Variation of Pressure from Mean.	Corrected Pressure on Instrument. Pounds.	Corrected Pressure on Instrument. Feet.	Corresponding Velocity.	
87.97	50.1	+ 0.6	+ 0.5	51.2	118.2	87.20	.9991
"	50.1	"	+ 0.5	51.2	118.2	87.20	.9991
"	50.0	"	+ 0.5	51.1	118.0	87.12	.9982
"	50.0	"	+ 0.5	51.1	118.0	87.12	.9982
"	50.0	"	+ 0.5	51.1	118.0	87.12	.9982
"	50.0	"	+ 0.5	51.1	118.0	87.12	.9982
"	50.0	"	+ 0.5	51.1	118.0	87.12	.9982
"	50.0	"	+ 0.5	51.1	118.0	87.12	.9982
"	49.9	"	+ 0.5	51.0	117.8	87.05	.9974
"	49.6	"	+ 0.5	50.7	117.1	86.78	.9943
"	49.0	"	+ 0.5	50.1	115.7	86.27	.9884
"	48.5	"	+ 0.5	49.6	114.5	85.82	.9833
"	47.0	"	+ 0.5	48.1	111.1	84.54	.9686
"	46.0	"	+ 0.5	47.1	108.8	83.62	.9581
"	43.9	"	+ 0.4	44.9	103.7	81.63	.9353
"	42.0	"	+ 0.4	43.0	99.3	79.92	.9157
"	39.0	+ 0.3	+ 0.4	39.7	91.7	76.80	.8799
"	33.5	+ 0.1	+ 0.3	33.9	78.3	71.97	.8246
"	30.0	+ 0.1	+ 0.3	30.4	70.2	67.20	.7699
"	23.0	+ 0.1	+ 0.2	23.3	53.8	(58.83)	*
"	18.0	0	+ 0.2	18.2	42.0	(51.98)	*
"	50.5	+ 0.6	+ 0.5	51.6	119.1	87.53	1.0029
"	50.1	"	+ 0.5	51.2	118.2	87.20	.9991
"	50.5	"	+ 0.5	51.6	119.1	87.53	1.0029
"	50.5	"	+ 0.5	51.6	119.1	87.53	1.0029
"	50.0	"	+ 0.5	51.1	118.0	87.12	.9982
"	50.0	"	+ 0.5	51.1	118.0	87.12	.9982
"	50.0	"	+ 0.5	51.1	118.0	87.12	.9982
"	50.0	"	+ 0.5	51.1	118.0	87.12	.9982
"	50.0	"	+ 0.5	51.1	118.0	87.12	.9982
"	49.5	"	+ 0.5	50.6	116.8	86.68	.9931
"	48.9	"	+ 0.5	50.0	115.6	86.19	.9875
"	47.0	"	+ 0.5	48.1	111.1	84.54	.9686
"	45.5	"	+ 0.5	46.6	107.6	83.19	.9531
"	43.5	"	+ 0.4	44.5	102.7	81.28	.9313
"	41.0	"	+ 0.4	42.0	97.0	78.99	.9050
"	38.0	+ 0.3	+ 0.4	38.7	89.4	75.83	.8688
"	33.9	+ 0.1	+ 0.3	34.3	79.2	71.38	.8178
"	28.9	+ 0.1	+ 0.3	29.3	67.7	65.99	.7561
"	25.3	+ 0.1	+ 0.3	25.7	59.3	(61.76)	*
"	19.0	0	+ 0.2	19.2	44.3	(53.38)	*
"	13.0	0	+ 0.1	13.1	30.3	(44.15)	*
"	5.0	0	+ 0.1	5.1	11.8	(27.55)	*

Add .011 to scale reading to get distance from center.

* Possibly point was not fully immersed in jet all the time.

the curves of Fig. 62, and thus by this method obtain a value for the co-efficient of discharge of that nozzle.

Take the curve for nozzle *E*, as that happens to be the one for which the full notes are given.

Integrating the total retardation by dividing the whole cross-section into concentric rings, we deduce as follows:

Outer Radius of Ring. (Proportional.)	Width of Ring.	Proportional Area of Ring to Area of Whole Circle.	Per Cent. which Velocity in Ring is Less than Velocity at Center.	Per Cent. by which Retardation Reduces Whole Discharge.
.55	.3025	.3025	0	0
.65	.4225	.1200	.002	.0002
.75	.5625	.1400	.002	.0003
.80	.6400	.0775	.003	.0002
.85	.7225	.0825	.008	.0007
.90	.8100	.0875	.025	.0022
.925	.8556	.0456	.062	.0028
.95	.9025	.0469	.095	.0045
.975	.9506	.0481	.159	.0077
1.00	1.0000	.0494	.232	.0115
Total.....	.....	.....	.....	.0301

This is proportional retardation of whole stream.

Therefore whole volume of water discharged averages flowing with 0.970 of the velocity at center, or if velocity at center is equal to the theoretical velocity due to the head, as supposed on page 415, then we have co-efficient of discharge = .970, while co-efficient actually found by measurement in tank was .971.

The velocity at center was found to be $\frac{0.77}{87.97} = .009$ less for this particular nozzle, however, than velocity due to head measured at base of play-pipe, which difference may have been in part due to slight error in gauge at base of play-pipe.

Thus we see it established that co-efficient of our instrument for measuring velocity is 1.00 within less than 1 per cent. and very likely within less than ¼ of 1 per cent.

A more delicate instrument than this modification of Pitot's tube for measuring very high velocities in water can hardly be desired.

In closing the account of these experiments it is but proper that the writer should record his appreciation of the painstaking care and great interest shown in all this work by his assistants, Mr. H. P. Barr and Mr. Benjamin G. Buttolph.

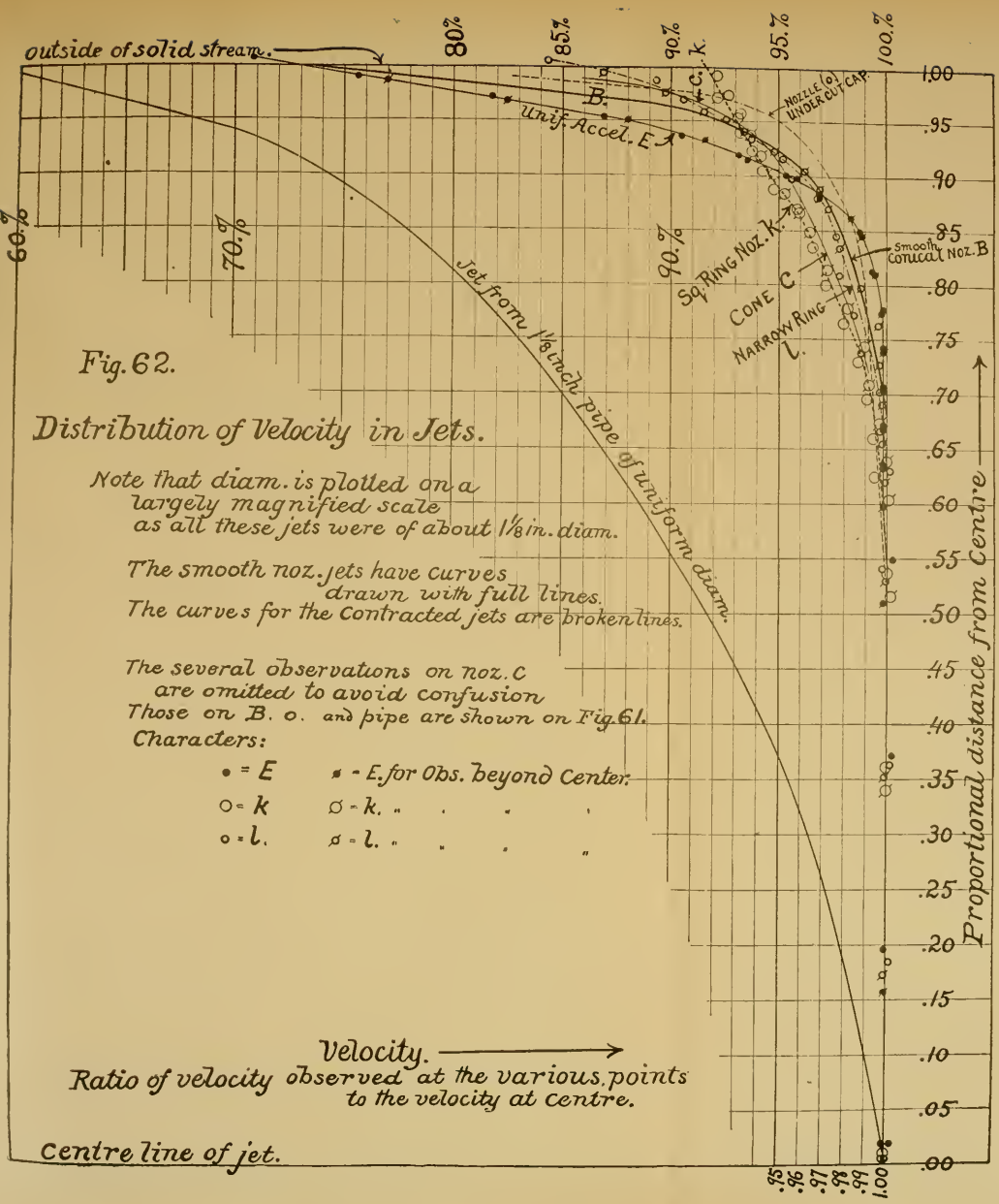


Fig. 62.

Distribution of Velocity in Jets.

Note that diam. is plotted on a largely magnified scale as all these jets were of about 1⅛ in. diam.

The smooth noz. jets have curves drawn with full lines.
The curves for the contracted jets are broken lines.

The several observations on noz. C are omitted to avoid confusion
Those on B. o. and pipe are shown on Fig. 61.

Characters:

• = E ⁎ = E. for Obs. beyond Center.
○ = k ⊘ = k. " " " "
o = l. ⊘ = l. " " " "

Practical Tables.

That experiments like the foregoing must be reduced to form convenient for practical use before they can be of much, if any, value to the profession at large, is illustrated by the popularity of the handy Ellis tables, already referred to, and their frequent repetition in the note-books published by pump makers, etc.; while the more accurate though far less convenient formulas for discharge of nozzles presented by Mr. Weston, in his paper before this Society, have been much less used. The writer, therefore, in the effort to make his results as useful as possible, has prepared the following tables giving the practical results of his experiments.

Notes on the Tables for Practical Use.

Table A, pages 422 to 441.—This is the most complete of the tables given relative to fire streams; but it is not in quite so convenient form, however, for the regular use of an insurance inspector, mill superintendent or fire engineer in testing the capacity of a fire pump or the delivery from water works as Table B, pages 444 to 447, which has been prepared for that special purpose.

It will be noted that in the first column the pressure is called "indicated pressure," meaning by this that it was the pressure which gauge attached to a proper piezometer on a hose or pipe 2½ inches in diameter would indicate while the stream was flowing. Nearly, if not all, other authors have based their tables upon the hydrostatic pressure, which, of course, equals the indicated pressure plus an allowance for mean velocity of current past piezometer. Thus, for instance, the indicated pressure of 50 pounds, given in the bold type of the first column of following table for 1⅛-inch smooth nozzle, corresponds to an effective pressure of 52.1 pounds as given by Ellis, Weston or Box. I adopted the indicated pressure as the main basis of the table because of its greater convenience for use by the practical fire engineer or mechanic. The hydrostatic pressure is, of course, the more scientific unit, and therefore this also is given in finer type in the second column of table.

The third and fifth columns of Table A give the maximum limit of height and distance as a "good effective fire stream." For very favorable conditions with still air the stream would be in condition to do very fair execution at a considerably greater distance, as implied in

notes in margin of column ; but in fixing this limit as a good effective stream it was intended that the figure given should be low enough so that it could be safely relied upon as a good stiff stream out to this point under almost any practical conditions—such, for instance, as when a fresh breeze was blowing.

As already stated (page 378) I classed as "good" a stream which, at the limit named, would enter through a window and barely strike ceiling of room with force enough to spatter well, and which at this limit named had not lost its continuity and solidity of stream by dividing into a mere shower of spray. In fixing this extreme horizontal distance reached by a jet as a good effective fire stream, the jet itself is not supposed to be horizontal, but inclined upward an ordinary working angle of about 30 to 45 degrees. In other words, with, for instance, a $1\frac{1}{8}$-inch nozzle and with pressure at nozzle of 40 pounds, the stream would reach, in excellent condition, a third-story window of a building 59 feet distant from end of nozzle.

The Jet Diagram, Fig. 63, gives this information as to reach of $1\frac{1}{8}$-inch jet in very convenient form.

As to number of gallons per minute discharged, this quantity as given in the table is probably exactly correct within less than a single gallon for the exact size and pressure stated. Reference to the previous description in detail may be had as showing that with any ordinary varieties of smooth form of taper or finish of nozzle or play-pipe the variation in form or finish will lead to no error of practical importance; but it must be borne in mind that a difference of $\frac{1}{100}$ of an inch in diameter of orifice of nozzle makes a little more than 2 per cent. difference in its discharge. In computing this part of table the value assumed for co-efficient discharge was 0.974.

There is a great difference in the conducting quality of different kinds of hose, even of the same size (see Fig. 47), and therefore there are given in each table values for pressure required at hydrant or steam fire engine corresponding to each of three different kinds of hose, these values covering about the whole range ordinarily met in practice. The two varieties of rubber hose referred to in the tables are respectively the roughest and the smoothest, or the best and the poorest in conducting power which are usually met in practice, and are the same as shown in *D* and *K*, Fig. 47. Intermediate values can be readily interpolated for other grades of hose between those given in the table. The values

for hydrant pressure given in the table include an allowance for the ordinary crookedness of lines of hose, and an allowance also for the effect upon friction loss in the hose of the ordinary slight excess of the commercial over the nominal diameter. As a basis for computing in these tables the pressure lost in going through hose, the following values were adopted for pounds pressure lost per 100 feet of hose with 240 gallons per minute flowing.

For linen hose,	2½ inch. nom. diam.,		30.0	pounds per	100 feet.
Inferior,	2½ "	rubber lined hose,	26.0	"	"
Best,	2½ "	" " "	13.0	"	"

These values include allowance for ordinary curves and for ordinary slight excess of actual over nominal diameter.

The loss of pressure was assumed to vary directly as the square of the quantity; a hypothesis shown admissible by a comparison of the dotted lines with the full lines in Fig. 48.

There is also included an allowance for the small loss of pressure in head of hydrant where water enters the hose. This, in computing table, was assumed to be double the theoretical head necessary to produce this volocity in a 2½-inch coupling; this being the ratio actually found by the writer in a series of experiments upon a Chapman Valve Co.'s independent gate hydrant.

Thus the hydrant pressures in the twenty-seven columns at the right-hand side of each sheet of Table A are those which would be found by a gauge attached as shown in sketch on Table B.

¾-INCH SMOOTH

This Table may also be

Hydrant Pressure Required—Discharge

INDICATED PRESSURE by Gauge attached at Base of Play-Pipe, and set level with End of Nozzle		Effective or Static Pressure at Base of Play-Pipe.	Extreme Height of Jet. Average of Highest Drops, Vertical Jet, Still Air.	Extreme Height of Jet. Maximum Limit of Height as Good Effective Fire Stream with Moderate Wind.	Extreme Horizontal Distance reached by Jet. Average Extreme Drops at Level of Nozzle, with Still Air.	Extreme Horizontal Distance reached by Jet. Maximum Limit of Distance as Good Effective Fire Stream with Moderate Wind.	Gallons per Minute Discharged.	Pounds Pressure Required at Hydrant (or first column, through): Length 50 ft. Unlined Linen Hose.	Length 50 ft. Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 50 ft. Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Length 100 ft. Unlined Linen Hose.	Length 100 ft. Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 100 ft. Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Length 200 ft. Unlined Linen Hose.	Length 200 ft. Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 200 ft. Ordinary best quality Rubber-lined Hose.—Inside Smooth.
	lbs.	lbs.	ft.	ft.	ft.	ft.		lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
Feeble Streams.	5.	5.0	10	.....	18	...	37	5	5	5	6	6	5	6	6	6
	10.	10.1	20	17	36	19	52	11	11	10	12	11	11	13	13	11
	15.	15.1	30	25	54	24	64	16	16	16	17	17	16	19	19	17
	20.	20.2	40	33	72	29	73	22	22	21	23	23	22	26	25	23
	25.	25.2	50	41	86	33	82	27	27	26	29	28	27	32	31	28
Ordinary Fire Pressures.	30.	30.2	59	48	96	37	90	33	32	31	35	34	32	39	38	34
	35.	35.3	69	55	105	41	97	38	38	37	40	40	38	45	44	40
	40.	40.3	78	60	112	44	104	43	43	42	46	46	43	52	50	46
	45.	45.4	86	64	119	47	110	49	48	47	52	51	48	58	57	51
	50.	50.4	93	67	125	50	116	54	54	52	58	57	54	65	63	57
	55.	55.5	99	70	131	52	122	60	59	58	64	63	59	71	69	63
	60.	60.5	104	72	136	54	127	65	65	63	69	68	65	78	76	68
Unusually High Pressures.	65.	65.5	109	74	141	56	132	71	70	68	75	74	70	84	82	74
	70.	70.6	114	76	145	58	137	76	75	73	81	80	75	91	88	80
	75.	75.6	119	78	149	60	142	81	81	79	87	85	81	97	94	85
	80.	80.7	123	79	153	62	147	87	86	84	93	91	86	104	101	91
	85.	85.7	126	80	157	64	151	92	92	89	98	97	92	110	107	97
	90.	90.7	129	81	161	65	156	98	97	94	104	102	97	117	113	102
	95.	95.8	132	82	164	66	160	103	102	99	110	108	102	123	120	108
	100.	100.8	134	83	167	68	164	109	108	105	116	114	108	130	126	114

Extreme Height of Jet: "is about 10 per cent. higher" / "is about 12 per cent. farther" / "fire stream" / "fair limit as a" / "Maximum"

80 pounds per square inch is now considered best hydrant pressure for general use; 100 lbs. per sq. in. should not be exceeded, except occasionally for very high buildings, or lengths of Hose exceeding 300 feet.

If nozzle is much higher or lower than hydrant, allowance for difference of level must be made on hydrant pressure (10 feet in height corresponds to 4.33 lbs. water pressure).

NOZZLE. (This size of Nozzle gives too small a body of water for extinguishing a fire of much size.) used for ⅞-inch Ring Nozzle.

—Height and Distance of Jet.

TABLE A.—No. 1 (From experiments of J. R. FREEMAN, 1888.)

Steamer) while stream is flowing, to maintain pressure at base of play-pipe, as per various lengths and kinds of 2½-inch Hose, as below.

Length 300 ft.			Length 400 ft.			Length 500 ft.			Length 600 ft.			Length 800 ft.			L'gth. 1000 ft.		
Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
lbs.	lbs.	lbs.	lbs	lbs	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
7	7	6	8	8	6	9	8	7	9	9	7	11	10	8	12	11	8
14	14	12	16	15	13	17	16	13	19	17	14	21	20	15	24	22	16
22	21	18	24	23	19	26	24	20	28	26	21	32	30	23	36	34	24
29	28	24	32	30	25	34	32	26	37	35	28	43	40	30	48	45	32
36	35	30	39	38	31	43	41	33	46	44	35	53	50	38	61	56	41
43	41	36	47	45	38	52	49	40	56	52	41	64	60	45	73	67	49
50	48	42	55	53	44	60	57	46	65	61	48	75	70	53	85	78	57
58	55	48	63	60	50	69	65	53	74	70	55	86	80	60	97	89	65
65	62	54	71	68	57	77	73	59	84	79	62	96	90	68	109	101	73
72	69	60	79	75	63	86	81	66	93	87	69	107	100	75	121	112	81
79	76	66	87	83	69	95	89	73	102	96	76	118	110	83	133	123	89
86	83	72	95	90	76	103	97	79	112	105	83	128	119	90	145	134	97
93	90	78	103	98	82	112	106	86	121	114	90	139	129	98	157	145	106
101	97	84	110	105	88	120	114	92	130	122	97	150	139	105	169	156	114
108	104	90	118	113	94	129	122	99	139	131	104	160	149	113	182	168	122
115	111	96	126	120	101	138	130	106	149	140	111	171	159	120	194	179	130
122	117	102	134	128	107	146	138	112	158	148	117	182	169	128	206	190	138
129	124	108	142	135	113	155	146	119	167	157	124	193	179	135	218	201	146
137	131	114	150	143	120	163	154	125	177	166	131	203	189	143	230	212	154
144	138	120	158	150	126	172	163	132	186	175	138	214	199	150	242	223	163

TAKE NOTICE.—The above is pressure at hydrant head *while stream is flowing.* The corresponding Static Reservoir Pressure, or Fire-pump Pressure, must be greater than hydrant pressure by an amount equal to friction loss between hydrant-head and pump or reservoir.

⅞-INCH SMOOTH

This Table may also be

Hydrant Pressure Required—Discharge

	INDICATED PRESSURE by Gauge attached at Base of Play-Pipe, and set level with End of Nozzle	Effective or Static Pressure at Base of Play-Pipe.	Extreme Height of Jet. Average of Highest Drops, Vertical Jet, Still Air.	Extreme Height of Jet. Maximum Limit of Height as Good Effective Fire Stream with Moderate Wind.	Extreme Horizontal Distance reached by Jet. Average Extreme Drops at level of Nozzle, with Still Air.	Extreme Horizontal Distance reached by Jet. Maximum Limit of Distance as Good Effective Fire Stream with Moderate Wind.	Gallons per Minute Discharged.	Pounds Pressure Required at Hydrant (or first column, through): Length 50 ft. Unlined Linen Hose.	Length 50 ft. Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 50 ft. Ordinary best quality Rubber lined Hose.—Inside Smooth.	Length 100 ft. Unlined Linen Hose.	Length 100 ft. Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 100 ft. Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Length 200 ft. Unlined Linen Hose.	Length 200 ft. Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 200 ft. Ordinary best quality Rubber-lined Hose.—Inside Smooth.
	lbs.	lbs.	ft.	ft.	ft.	ft.		lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
Feeble Streams.	5.	5.1	11	....	18	....	50	6	6	5	6	6	6	8	7	6
	10.	10.2	21	18	37	21	71	12	11	11	13	13	11	16	15	13
	15.	15.2	31	26	56	27	87	17	17	16	19	19	17	23	22	19
	20.	20.3	41	34	74	33	100	23	23	22	26	25	23	31	30	25
Ordinary Fire Pressures.	25.	25.4	51	42	90	38	112	29	29	27	32	31	29	39	37	31
	30.	30.5	61	49	103	42	123	35	34	33	39	38	34	47	45	38
	35.	35.5	71	56	114	46	133	41	40	38	45	44	40	54	52	44
	40.	40.6	81	62	124	49	142	46	46	43	52	50	46	62	59	50
	45.	45.7	89	67	132	52	150	52	51	49	58	57	51	70	67	57
	50.	50.8	97	71	140	55	159	58	57	54	65	63	57	78	74	63
	55.	55.8	105	74	147	58	166	64	63	60	71	69	63	85	82	69
	60.	60.9	112	77	153	61	174	70	69	65	78	75	69	93	89	75
Unusually High Pressures.	65.	66.0	118	79	158	64	181	75	74	71	84	82	74	101	96	82
	70.	71.0	123	81	163	66	188	81	80	76	90	88	80	109	104	88
	75.	76.1	128	83	168	68	194	87	86	82	97	94	86	117	111	94
	80.	81.2	132	85	172	70	201	93	91	87	103	101	91	124	119	101
	85.	86.3	136	87	176	72	207	99	97	92	110	107	97	132	126	107
	90.	91.3	139	88	180	74	213	104	103	98	116	113	103	140	134	113
	95.	96.4	142	89	183	75	219	110	109	103	123	119	109	148	141	119
	100.	101.5	144	90	186	76	224	116	114	109	129	126	114	155	148	126

Feeble Streams: 10 per cent. higher; 15 per cent. farther. Ordinary Fire Pressures: 20 per cent. higher; 25 per cent. farther. Unusually High Pressures: Maximum limit as a "fair fire stream."

80 pounds per square inch is now considered best hydrant pressure for general use; 100 lbs. per sq. in. should not be exceeded, except occasionally for very high buildings, or lengths of Hose exceeding 300 feet.

If nozzle is much higher or lower than hydrant, allowance for difference of level must be made on hydrant pressure (10 feet in height corresponds to 4.33 lbs. water pressure).

NOZZLE.

used for 1-inch Ring Nozzle.

—Height and Distance of Jet.

TABLE A.—No. 2.
(From experiments of J. R. FREEMAN, 1888.)

Steamer) while stream is flowing, to maintain pressure at base of play-pipe, as per various lengths and kinds of 2½-inch Hose, as below.

Length 300 ft.			Length 400 ft.			Length 500 ft.			Length 600 ft.			Length 800 ft.			L'gth. 1000 ft.		
Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
9	9	7	10	10	7	12	11	8	13	12	9	16	14	10	18	17	11
18	17	14	21	19	15	23	22	16	26	24	17	31	28	19	37	33	22
27	26	21	31	29	22	35	33	24	39	36	26	47	43	29	55	50	33
36	34	27	42	39	30	47	43	32	52	48	34	63	57	39	73	66	43
45	43	34	52	48	37	59	54	40	65	60	43	78	71	48	91	83	54
54	51	41	62	58	45	70	65	48	78	72	51	94	85	58	110	99	65
64	60	48	73	68	52	82	76	56	91	84	60	109	100	68	128	116	76
73	68	55	83	78	59	94	87	64	104	96	68	125	114	78	146	132	87
82	77	62	94	87	67	105	97	72	117	108	77	141	128	87	164	149	97
91	86	69	104	97	74	117	108	80	130	120	86	156	142	97	183	165	108
100	94	75	114	107	82	129	119	88	143	132	94	172	157	107	201	182	119
109	103	82	125	116	89	140	130	96	156	144	103	188	171	116	219	198	130
118	111	89	135	126	96	152	141	104	169	156	111	203	185	126	237	215	141
127	120	96	145	136	104	164	152	112	182	167	120	219	199	136	255	231	152
136	128	103	156	145	111	175	162	120	195	179	128	234	213	145	....	248	162
145	137	110	166	155	119	187	173	128	208	191	137	250	228	155	....	264	173
154	145	116	177	165	126	199	184	136	221	203	145	....	242	165	....	....	184
163	154	123	187	174	134	211	195	144	234	215	154	....	256	174	....	....	195
173	163	130	197	184	141	222	206	152	247	227	163	....	....	184	....	....	206
182	171	137	208	194	148	234	216	160	260	239	171	....	....	194	....	....	216

TAKE NOTICE.—The above is pressure at hydrant head *while stream is flowing*. The corresponding Static Reservoir Pressure, or Fire-pump Pressure, must be greater than hydrant pressure by an amount equal to friction loss between hydrant-head and pump or reservoir.

1-INCH SMOOTH

Hydrant Pressure Required—Discharge

INDICATED PRESSURE by Gauge attached at Base of Play-Pipe, and set level with End of Nozzle.		Effective or Static Pressure at Base of Play-Pipe.	Extreme Height of Jet. Average of Highest Drops, Vertical Jet, Still Air.	Extreme Height of Jet. Maximum Limit of Height as Good Effective Fire Stream with Moderate Wind.	Extreme Horizontal Distance reached by Jet. Average Extreme Drops at level of Nozzle, with Still Air.	Extreme Horizontal Distance reached by Jet. Maximum Limit of Distance as Good Effective Fire Stream with Moderate Wind.	Gallons per Minute Discharged.	Pounds Pressure Required at Hydrant (or first column, through								
								Length 50 ft.			Length 100 ft.			Length 200 ft.		
								Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
	lbs.	lbs.	ft.	ft.	ft.	ft.		lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
Feeble Streams.	5.	5.1	10	....	17	....	66	6	6	6	8	7	6	10	9	7
	10.	10.3	21	18	38	21	93	13	12	12	15	14	12	20	18	14
	15.	15.4	32	26	58	30	114	19	19	17	23	22	19	29	27	22
	20.	20.5	43	35	77	37	132	26	25	23	30	29	25	39	37	29
Ordinary Fire Streams. Fair.	25.	25.6	53	43	94	42	147	32	31	29	38	36	31	49	46	36
	30.	30.8	63	51	109	47	161	38	37	34	45	43	37	59	55	43
Good.	35.	35.9	73	58	122	51	174	45	44	40	53	51	44	68	64	51
	40.	41.0	83	64	133	55	186	51	50	46	60	58	50	78	73	58
Excellent.	45.	46.2	92	69	143	58	198	57	56	52	68	65	56	88	83	65
	50.	51.3	101	73	152	61	208	64	62	57	75	72	62	98	92	72
	55.	56.4	109	76	160	64	218	70	69	63	83	79	69	108	101	79
	60.	61.5	117	79	167	67	228	77	75	69	90	87	75	117	110	87
Unusually Strong Streams. (Difficult to hold Nozzle without special appliances.)	65.	66.7	124	82	173	70	237	83	81	75	98	94	81	127	119	94
	70.	71.8	130	85	179	72	246	89	87	80	105	101	87	137	128	101
	75.	76.9	135	87	184	74	255	96	94	86	113	108	94	147	138	108
	80.	82.1	140	89	189	76	263	102	100	92	120	115	100	156	147	115
	85.	87.2	144	91	193	78	274	109	106	98	128	123	106	166	156	123
	90.	92.3	147	92	197	80	279	115	112	103	135	130	112	176	165	130
	95.	97.4	150	94	201	82	287	121	118	109	143	137	118	186	174	137
	100.	102.6	152	96	205	83	295	128	125	115	150	144	125	195	183	144

Ordinary Fire Streams: Fair (25.–30.); Good (35.–40.); Excellent (45.–60.). Unusually Strong Streams: Maximum limit as a "fair fire stream" (65.–80.); Maximum limit as a "good effective fire stream" (85.–100.).

80 pounds per square inch is now considered best hydrant pressure for general use; 100 lbs. per sq. in. should not be exceeded, except occasionally for very high buildings, or lengths of Hose exceeding 300 feet.

If nozzle is much higher or lower than hydrant, allowance for difference of level must be made on hydrant pressure (10 feet in height corresponds to 4.33 lbs. water pressure).

NOZZLE.
—Height and Distance of Jet.

TABLE A.—No. 3.
(From experiments of J. R. FREEMAN, 1888.)

Steamer) while stream is flowing, to maintain pressure at base of play-pipe, as per various lengths and kinds of 2½-inch Hose, as below.

Length 300 ft.			Length 400 ft.			Length 500 ft.			Length 600 ft.			Length 800 ft.			L'gth. 1000 ft.		
Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
12	11	8	14	13	9	17	15	10	19	17	11	23	21	13	28	25	15
24	22	16	29	26	18	33	30	20	38	34	22	47	42	26	56	50	30
36	33	25	43	39	28	50	45	30	57	51	33	70	63	39	84	75	45
48	45	33	57	52	37	66	60	41	75	68	45	93	84	52	111	99	60
60	56	41	71	65	46	83	75	51	94	85	56	117	105	65	139	124	75
72	67	49	86	79	55	99	90	61	113	102	67	140	125	79	167	149	90
84	78	57	100	92	64	116	105	71	132	119	78	163	146	92	195	174	105
96	89	66	114	105	73	132	120	81	151	136	89	187	167	105	223	199	120
108	100	74	129	118	83	149	135	91	169	153	100	210	188	118	251	223	135
120	111	82	143	131	92	166	151	102	188	170	111	233	209	131	....	248	151
132	122	90	157	144	101	182	166	112	207	187	122	...	230	144	...	....	166
144	134	98	171	157	110	199	181	122	226	204	134	....	251	157	....	..	181
156	145	107	186	170	119	215	196	132	244	221	145	....	..	170	....	....	196
168	156	115	200	183	128	232	211	142	...	238	156	....	....	183	....	....	211
181	167	123	214	196	138	248	226	152	....	255	167	....	....	196	....	...	226
193	178	131	229	209	147	....	241	162	..	....	178	....	....	209	....	....	241
205	189	139	243	222	156	....	256	173	....	...	189	....	....	222	....	....	....
217	200	147	257	236	165	...	..	183	....	....	200	....	....	236	....	...	....
229	211	156	...	249	174	...	....	193	....	..	211	...	....	249	....	....	....
241	223	164	....	....	183	...	..	203	...	..	223	...	...	...	....	....	....

TAKE NOTICE.—The above is pressure at hydrant head *while stream is flowing.* The corresponding Static Reservoir Pressure, or Fire-pump Pressure, must be greater than hydrant pressure by an amount equal to friction loss between hydrant-head and pump or reservoir.

1⅛-INCH SMOOTH

Hydrant Pressure Required—Discharge

Indicated Pressure by Gauge attached at Base of Play-Pipe, and set level with End of Nozzle.		Effective or Static Pressure at Base of Play-Pipe.	Extreme Height of Jet. Average of Highest Drops, Vertical Jet, Still Air	Extreme Height of Jet. Maximum Limit of Height as Good Effective Fire Stream with Moderate Wind.	Extreme Horizontal Distance reached by Jet. Average Extreme Drops at level of Nozzle, with Still Air.	Extreme Horizontal Distance reached by Jet. Maximum Limit of Distance as Good Effective Fire Stream with Moderate Wind.	Gallons per Minute Discharged.	Pounds Pressure Required at Hydrant (or first column, through) Length 50 ft. Unlined Linen Hose.	Length 50 ft. Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 50 ft. Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Length 100 ft. Unlined Linen Hose.	Length 100 ft. Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 100 ft. Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Length 200 ft. Unlined Linen Hose.	Length 200 ft. Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 200 ft. Ordinary best quality Rubber-lined Hose.—Inside Smooth.
	lbs.	lbs.	ft.	ft.	ft.	ft.		lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
Feeble Streams.	5.	5.2	11	.....	19	.....	84	7	7	6	9	9	7	13	12	9
	10.	10.4	22	18	39	22	119	15	14	12	18	17	14	26	24	17
	15.	15.6	32	27	59	31	146	22	21	19	27	26	21	38	35	26
	20.	20.8	43	36	80	38	168	29	28	25	36	34	28	51	47	34
Ordinary Fire Streams. Fair.	25.	26.0	54	44	99	44	188	36	35	31	45	43	35	64	59	43
	30.	31.2	64	52	115	50	206	44	42	37	55	52	42	77	71	52
Good.	35.	36.4	74	59	130	54	222	51	49	43	64	60	49	89	82	60
	40.	41.7	84	65	142	59	238	58	56	50	73	69	56	102	94	69
	45.	46.9	94	70	152	63	252	65	63	56	82	77	63	115	106	77
Excellent.	50.	52.1	104	75	162	66	266	72	70	62	91	86	70	128	118	86
	55.	57.3	113	80	170	69	279	80	77	68	100	95	77	140	130	95
	60.	62.5	122	83	178	72	291	87	84	74	109	103	84	153	141	103
Unusually Strong Streams. (Difficult to hold Nozzle without special appliances.)	65.	67.7	130	86	185	75	303	94	91	81	118	112	91	166	153	112
	70.	72.9	136	88	191	77	314	101	98	87	127	120	98	179	165	120
	75.	78.1	142	90	197	79	325	109	105	93	136	129	105	191	177	129
	80.	83.3	146	92	203	81	336	116	112	99	145	138	112	204	188	138
	85.	88.5	150	94	209	83	346	123	119	106	154	146	119	217	200	146
	90.	93.7	153	96	214	85	356	130	126	112	164	155	126	230	212	155
	95.	98.9	156	98	219	87	366	138	133	118	173	163	133	242	224	163
	100.	104.1	158	99	224	89	376	145	140	124	182	172	140	255	236	172

Fair: 26.0–46.9. Good: 25.–45. Excellent: 50.–60. Maximum: 65.–100.

Maximum Limit of Height: 25.–40. is about 10 per cent. higher; 45.–60. fire stream "; 65.–100. limit as a "fair

Maximum Limit of Distance: 25.–40. is about 12 per cent. farther; 45.–60. fire stream "; 65.–100. limit as a "fair

80 pounds per square inch is now considered best hydrant pressure for general use; 100 lbs. per sq. in. should not be exceeded, except occasionally for very high buildings, or lengths of Hose exceeding 300 feet.

If nozzle is much higher or lower than hydrant, allowance for difference of level must be made on hydrant pressure (10 feet in height corresponds to 4.33 lbs. water pressure).

NOZZLE. (This is the size preferred for general outside use.) —Height and Distance of Jet.

TABLE A.—No. 4. (From experiments of J. R. FREEMAN, 1888.)

Steamer) while stream is flowing, to maintain pressure at base of play-pipe, as per various lengths and kinds of 2½-inch Hose, as below.

Length 300 ft.			Length 400 ft.			Length 500 ft.			Length 600 ft.			Length 800 ft.			L'gth. 1000 ft.		
Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs	lbs.	lbs.	lbs.	lbs.	lbs.	.os.
16	15	10	20	18	12	24	21	13	28	25	15	35	31	18	42	37	21
33	30	20	40	36	24	48	43	27	55	49	30	70	62	36	84	75	43
49	45	31	60	54	35	71	64	40	82	74	45	105	93	54	127	112	64
66	60	41	80	73	47	95	85	54	110	98	60	139	123	73	168	149	85
82	75	51	101	91	59	119	107	67	137	123	75	174	154	91	211	186	107
99	90	61	121	109	71	143	128	80	165	147	90	209	185	109	253	223	128
115	105	71	141	127	82	166	149	94	192	172	105	243	216	127	....	261	149
131	120	81	161	145	94	190	171	107	220	196	120	278	247	145	....	....	171
148	135	92	181	163	106	214	192	120	247	221	135	.. .	278	163	....	....	192
164	150	102	201	181	118	238	213	134	274	245	150	...	....	181	....	....	213
181	165	112	221	200	130	262	235	147	....	270	165	...	....	200	....	....	235
197	180	122	241	218	141	....	256	160	....	. .	180	....	....	218	....	...	256
214	195	132	261	236	153	....	....	174	....	....	195	....	. ..	236	....	....	....
230	209	143	....	254	165	....	....	187	....	....	209	....	...	254	...	....	...
246	224	153	....	.. .	177	....	....	201	....	....	224	....	....	. .	...	...	....
263	239	163	....	....	188	....	....	214	..	....	239	....	....	....	....	....	....
....	254	173	....	....	200	.. .	....	227	...	...	254	....	....	....	...	. .	....
....	....	183	...	....	212	....	. ..	241	....	....	....	....	....	...	....	...	...
....	....	194	....	..	224	....	....	254	....	....	....	...	....	....	...	....	....
....	....	204	....	....	236	....	....	...	...	..	....	....	....	....	....	....	....

TAKE NOTICE.—The above is pressure at hydrant head *while stream is flowing.* The corresponding Static Reservoir Pressure, or Fire-pump Pressure, must be greater than hydrant pressure by an amount equal to friction loss between hydrant-head and pump or reservoir.

1¼-INCH SMOOTH

Hydrant Pressure Required—Discharge

INDICATED PRESSURE by Gauge attached at Base of Play-Pipe, and set level with End of Nozzle.		Effective or Static Pressure at Base of Play-Pipe.	Extreme Height of Jet. Average of Highest Drops, Vertical Jet, Still Air.	Extreme Height of Jet. Maximum Limit of Height as Good Effective Fire Stream with Moderate Wind.	Extreme Horizontal Distance reached by Jet. Average Extreme Drops at level of Nozzle, with Still Air.	Extreme Horizontal Distance reached by Jet. Maximum Limit of Distance as Good Effective Fire Stream with Moderate Wind.	Gallons per Minute Discharged.	Pounds Pressure Required at Hydrant (or first column, through								
								Length 50 ft.			Length 100 ft.			Length 200 ft.		
								Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
	lbs.	lbs.	ft.	ft.	ft.	ft.		lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
Feeble Streams.	5	5.3	11	....	19	...	105	8	8	7	11	11	8	17	15	11
	10.	10.6	22	cent. higher.	40	cent. farther.	148	17	16	14	23	21	16	34	31	21
	15.	15.9	33	28	62	32	181	25	24	21	34	32	24	51	47	32
	20.	21.2	44	37	83	40	209	34	32	27	45	42	32	68	62	42
Ordinary Fire Streams. Fair.	25.	26.5	55	46	103	47	234	42	40	34	57	53	40	85	77	53
	30.	31.8	66	53	119	54	256	51	49	41	68	63	49	102	93	63
Good.	35.	37.1	76	60	134	59	277	59	57	48	79	74	57	119	109	74
	40.	42.4	86	67	148	63	296	68	65	55	91	84	65	136	124	84
	45.	47.8	97	72	159	67	314	76	73	62	102	95	73	153	140	95
Excellent.	50.	53.1	107	77	169	70	331	85	81	68	113	106	81	170	155	106
	55.	58.4	117	81	178	73	347	93	89	75	124	116	89	187	170	116
	60.	63.7	126	85	186	76	363	102	97	82	136	127	97	204	186	127
Unusually Strong Streams. (Difficult to hold Nozzle without special appliances.)	65.	69.0	133	88	193	79	377	110	105	89	147	137	105	221	201	137
	70.	74.3	140	91	200	81	392	118	113	96	158	148	113	238	217	148
	75.	79.6	145	93	207	83	405	127	121	103	170	158	121	255	232	158
	80.	84.9	150	95	213	85	419	135	129	110	181	169	129	....	248	169
	85.	90.2	154	97	219	88	432	144	137	116	192	179	137	....	263	170
	90.	95.5	157	99	225	90	444	152	145	123	204	190	145	....	....	190
	95.	100.8	159	100	231	92	456	161	154	130	215	201	154	....	....	201
	100.	106.1	161	101	236	93	468	169	162	137	226	211	162	....	....	211

Fair: 25. 30. Good: 35. 40. Excellent: 50. 55. 60.

Feeble Streams: 10. is about 10 per cent. higher; 20. is about 12 per cent. farther.

Ordinary Fire Streams: 45. "fire stream"; 65. "fire stream" limit as a "fair".

Unusually Strong Streams: 75. "fair" limit as a "fair"; 95. Maximum; 100. Maximum.

80 pounds per square inch is now considered best hydrant pressure for general use; 100 lbs. per sq. in. should not be exceeded, except occasionally for very high buildings, or lengths of Hose exceeding 300 feet.

If nozzle is much higher or lower than hydrant, allowance for difference of level must be made on hydrant pressure (10 feet in height corresponds to 4.33 lbs. water pressure).

NOZZLE. (This size is well adapted for serious fires where water supply is ample and line of hose not too long.) — Height and Distance of Jet.

TABLE A.—No. 5. (From experiments of J. R. FREEMAN, 1888.)

Steamer) while stream is flowing, to maintain pressure at base of play-pipe, as per various lengths and kinds of 2½-inch Hose, as below.

Length 300 ft.			Length 400 ft.			Length 500 ft.			Length 600 ft.			Length 800 ft.			L'gth. 1000 ft.		
Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
23	20	13	28	25	15	34	30	18	40	35	20	51	45	25	63	55	30
46	41	26	57	51	31	68	61	36	80	71	41	103	90	51	125	110	61
68	61	39	85	76	47	102	91	54	120	106	61	154	136	76	188	165	91
91	82	52	114	102	62	137	121	72	159	141	82	205	181	101	251	220	121
114	102	65	142	127	77	171	152	90	199	176	102	256	226	127	....	275	152
136	123	78	171	152	93	205	182	108	239	212	123	....	271	152	....	....	182
159	143	91	199	178	109	239	212	126	279	247	142	....	....	178	....	....	212
182	164	104	227	203	124	273	243	144	....	283	164	....	....	203	....	....	243
205	184	117	256	229	140	....	273	162	....	....	184	....	....	229	....	....	....
227	204	130	....	254	155	....	....	180	....	....	204	....	....	254	....	....	....
249	225	143	....	....	170	....	....	198	....	....	225	....	....	....	....	....	....
....	245	156	....	....	186	....	....	216	....	....	245	....	....	....	....	....	....
....	266	169	....	....	201	....	....	234	....	....	....	....	....	....	....	....	....
....	....	182	....	....	217	....	....	252	....	....	....	....	....	....	....	....	....
....	....	195	....	....	232	....	....	....	....	....	....	....	....	....	....	....	....
....	....	208	....	....	248	....	....	....	....	....	....	....	....	....	....	....	....
....	....	221	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....
....	....	234	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....
....	....	247	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....
....	....	261	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....

TAKE NOTICE.—The above is pressure at hydrant head *while stream is flowing*. The corresponding Static Reservoir Pressure, or Fire-pump Pressure, must be greater than hydrant pressure by an amount equal to friction loss between hydrant-head and pump or reservoir.

1⅜-INCH SMOOTH

Hydrant Pressure Required—Discharge

INDICATED PRESSURE by Gauge attached at Base of Play-Pipe, and set level with End of Nozzle.		Effective or Static Pressure at Base of Play-Pipe.	Extreme Height of Jet. Average of Highest Drops, Vertical Jet, Still Air.	Extreme Height of Jet. Maximum Limit of Height as Good Effective Fire Stream with Moderate Wind.	Extreme Horizontal Distance reached by Jet. Average Extreme Drops at Level of Nozzle, with Still Air.	Extreme Horizontal Distance reached by Jet. Maximum Limit of Distance as Good Effective Fire Stream with Moderate Wind.	Gallons per Minute Discharged.	Pounds Pressure Required at Hydrant (or first column, through — Length 50 ft. — Unlined Linen Hose.	Length 50 ft. — Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 50 ft. — Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Length 100 ft. — Unlined Linen Hose.	Length 100 ft. — Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 100 ft. — Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Length 200 ft. — Unlined Linen Hose.	Length 200 ft. — Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 200 ft. — Ordinary best quality Rubber-lined Hose.—Inside Smooth.
	lbs.	lbs.	ft.	ft.	ft.	ft.		lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
Feeble Streams.	5.	5.5	12	....	19	....	128	10	10	8	15	13	10	23	21	13
	10.	10.9	23	20	41	23	182	20	19	16	29	27	19	46	42	27
	15.	16.4	34	29	63	33	222	31	29	23	44	40	29	69	62	40
	20.	21.8	45	38	85	42	257	41	39	31	58	53	39	92	83	53
Ordinary Fire Streams. Fair.	25.	27.3	56	47	106	49	287	51	48	39	73	67	48	115	104	67
	30.	32.7	67	55	123	56	315	61	58	47	87	80	58	139	125	80
Good.	35.	38.2	78	62	138	62	340	71	67	54	102	94	67	162	146	94
	40.	43.7	89	69	152	66	363	82	77	62	116	107	77	185	166	107
	45.	49.1	100	74	164	70	385	92	87	70	131	120	87	208	187	120
Excellent.	50.	54.6	111	79	175	73	406	102	96	78	145	134	96	231	208	134
	55.	60.0	121	83	184	76	426	112	106	86	160	147	106	254	229	147
	60.	65.5	131	87	192	79	445	122	116	93	174	160	116	....	250	160
Unusually Strong Streams. (Difficult to hold Nozzle without special appliances.)	65.	70.9	138	90	200	82	463	133	125	101	189	174	125	....	....	174
	70.	76.4	144	92	207	84	480	143	135	109	203	187	135	....	....	187
	75.	81.9	149	95	214	86	497	153	145	117	218	200	145	....	....	200
	80.	87.3	154	97	220	88	514	163	154	124	232	214	154	....	....	214
	85.	92.8	158	99	226	90	529	174	164	132	247	227	164	....	....	227
	90.	98.2	161	100	232	92	545	184	173	140	261	240	173	....	....	240
	95.	103.7	163	101	238	94	560	194	183	148	....	254	183	....	....	254
	100.	109.1	165	103	243	96	574	204	193	156	....	....	193	....	....	....

Fair: limits a "fair fire stream" is about 10 per cent. higher.
Good: limits as "good fire stream" is about 12 per cent. farther.
Maximum: limits a "fair" Maximum.

80 pounds per square inch is now considered best hydrant pressure for general use; 100 lbs. per sq. in. should not be exceeded, except occasionally for very high buildings, or lengths of Hose exceeding 300 feet.

If nozzle is much higher or lower than hydrant, allowance for difference of level must be made on hydrant pressure (10 feet in height corresponds to 4.33 lbs. water pressure).

NOZZLE.

—Height and Distance of Jet.

TABLE A.—No. 6.
(From experiments of J. R. FREEMAN, 1888.)

Steamer) while stream is flowing, to maintain pressure at base of play-pipe, as per various lengths and kinds of 2½-inch Hose, as below.

Length 300 ft.			Length 400 ft.			Length 500 ft.			Length 600 ft.			Length 800 ft.			L'gth. 1000 ft.		
Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
32	28	17	40	36	21	49	43	25	57	51	28	74	66	36	92	80	43
63	56	34	81	71	42	98	86	49	115	101	56	149	131	71	184	161	86
95	85	51	121	107	62	147	129	74	172	152	85	224	196	107	275	241	129
127	113	68	161	143	83	195	173	98	230	202	113	299	262	143	....	321	173
158	141	85	201	178	104	244	216	123	287	253	141	....	....	178	....	....	216
190	169	103	242	214	125	293	259	147	....	....	169	....	....	2..	....	....	259
222	198	120	282	250	146	....	....	172	....	....	198	....	....	250	....	....	....
253	226	137	....	....	166	....	....	196	....	....	226	....	....	....	....	....	....
....	254	154	....	....	187	....	....	221	....	....	254	....	....	....	....	....	....
....	....	171	....	....	208	....	....	245	....	....	....	....	....	....	....	....	....
....	....	188	....	....	229	....	....	270	....	....	....	....	....	....	....	....	....
....	....	205	....	....	250	....	....	....	....	....	....	....	....	....	....	....	....
....	....	222	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....
....	....	239	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....
....	....	256	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....
....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....
....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....
....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....
....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....
....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....

TAKE NOTICE.—The above is pressure at hydrant head *while stream is flowing*. The corresponding Static Reservoir Pressure, or Fire-pump Pressure, must be greater than hydrant pressure by an amount equal to friction loss between hydrant-head and pump or reservoir.

Ring Nozzles.

Although it was conclusively proved by the experiments described above that the ring nozzle does not possess the slightest advantage over the smooth nozzle, yet, so many ring nozzles are in use that it was

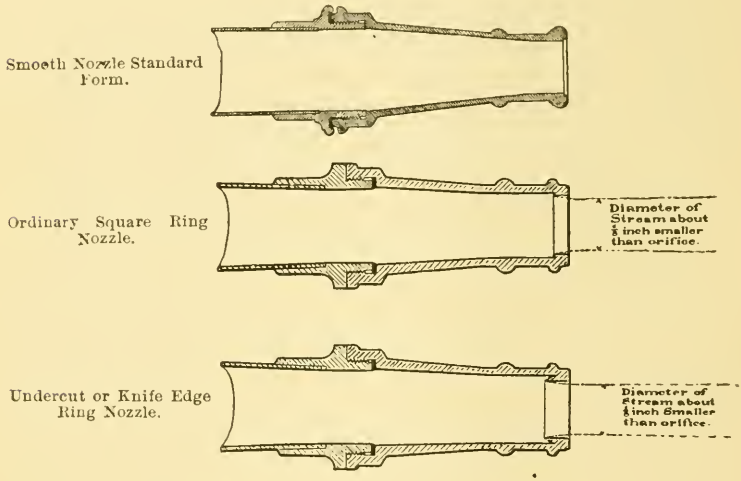


Fig. 59.—Ring Nozzles.

thought best to prepare tables giving their discharge. Drawings of the three kinds of nozzles are repeated above for ready reference. The experiments previously described (see page 334) show that the ring nozzle discharges only three-fourths as much water per minute as the smooth nozzle of the same size, and if any one will measure the diameter of the jet at about one-half inch from end of nozzle by a pair of common machinist's calipers, he will find it to be about $\frac{1}{8}$ inch smaller than the hole from which it issues. Thus the only use of the ring nozzle is to make a show of playing a larger stream than is the fact.

The apparent advantage of the ring nozzle, which has misled many firemen, is easily explained. The result is the same as if a smaller nozzle were used while hydrant pressure remained the same; thus the

number of gallons per minute flowing being less, the pressure lost by friction through hose is less, and therefore there remains a greater pressure at base of play-pipe, and this carries the stream to a somewhat greater height and distance, but with greatly diminished volume. For $\frac{7}{8}$-inch and 1-inch ring nozzles the discharge is almost exactly the same as for smooth nozzle $\frac{1}{8}$ inch smaller. Therefore, no special tables for these two sizes of ring nozzle are given.

As a basis for computing these discharges of ring nozzles the value 0.74 was adopted for co-efficient of discharge.

(Discharge of 1-in. Ring is same as 7/8-in. Smooth.
Discharge of 7/8-in. Ring is same as 3/4-in. Smooth.)

1 1/8-INCH RING

Hydrant Pressure Required—Discharge

INDICATED PRESSURE by Gauge attached at Base of Play-Pipe, and set level with End of Nozzle.		Effective or Static Pressure at Base of Play Pipe.	Extreme Height of Jet. Average of Highest Drops, Vertical Jet, Still Air.	Extreme Height of Jet. Maximum Limit of Height as Good Effective Fire Stream with Moderate Wind.	Extreme Horizontal Distance reached by Jet. Average Extreme Drops at Level of Nozzle, with Still Air.	Extreme Horizontal Distance reached by Jet. Maximum Limit of Distance as Good Effective Fire Stream with Moderate Wind.	Gallons per Minute Discharged.	Pounds Pressure Required at Hydrant (or first column, through								
								Length 50 ft.			Length 100 ft.			Length 200 ft.		
								Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
	lbs.	lbs.	ft.	ft.	ft.	ft.		lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
Feeble Streams.	5.	5.1	10	....	17	....	63	6	6	6	7	7	6	9	9	7
	10.	10.2	21	17	37	20	89	13	12	11	15	14	12	19	18	14
	15.	15.4	32	26	57	29	110	19	18	17	22	21	18	28	27	21
	20.	20.5	42	34	76	36	126	25	25	23	29	28	25	38	35	28
Ordinary Fire Streams. Fair.	25.	25.6	52	42	93	41	141	31	31	28	37	35	31	47	44	35
	30.	30.7	62	49	107	45	155	38	37	34	44	42	37	56	53	42
Good.	35.	35.8	72	56	120	49	167	44	43	40	51	49	43	66	62	49
	40	40.9	82	62	131	53	179	50	49	45	59	56	49	75	71	56
	45.	46.0	91	67	141	56	190	56	55	51	66	63	55	85	80	63
Excellent.	50.	51.2	99	70	150	59	200	63	61	57	73	70	61	94	88	70
	55.	56.3	107	73	158	62	210	69	67	63	80	77	67	103	97	77
	60.	61.4	115	76	164	65	219	75	74	68	88	84	74	113	106	84
Unusually Strong Streams. (Difficult to hold Nozzle without special appliances.)	65.	66.5	122	79	170	67	228	82	80	74	95	91	80	122	115	91
	70.	71.6	128	82	176	69	237	88	86	80	102	98	86	132	124	98
	75.	76.7	133	84	181	71	245	94	92	85	110	106	92	141	133	106
	80.	81.8	138	86	186	73	253	100	98	91	117	113	98	150	141	113
	85.	87.0	142	88	190	75	261	107	104	97	124	120	104	160	150	120
	90.	92.1	145	89	194	77	268	113	110	102	132	127	110	169	159	127
	95.	97.2	148	91	198	79	276	119	117	108	139	134	117	179	168	134
	100.	102.3	150	93	202	80	283	125	123	114	146	141	123	188	177	141

Feeble Streams: Maximum Limit of Height as Good Effective Fire Stream with Moderate Wind — 17 cent. higher. Maximum Limit of Distance as Good Effective Fire Stream with Moderate Wind — 20 cent. farther.

Ordinary Fire Streams: Maximum Limit of Height as Good Effective Fire Stream with Moderate Wind — 42 is about 10 per cent. higher. Maximum Limit of Distance as Good Effective Fire Stream with Moderate Wind — 41 is about 12 per cent. farther.

Unusually Strong Streams: Maximum Limit of Height as Good Effective Fire Stream with Moderate Wind — 88 limit as a "fair fire stream." Maximum Limit of Distance as Good Effective Fire Stream with Moderate Wind — 75 limit as a "fair fire stream."

NOTE.—The above figures for Ring Nozzle Discharges will apply to any ordinary form of Ring accurately enough for practical purposes, but apply especially to ordinary form of Ring Nozzle with square shoulder $\frac{1}{16}$ or $\frac{1}{8}$ inch deep.

Ring Nozzles with "underout" or "knife-edge" shoulder, discharge, as ordinarily constructed, about three per cent. less than quantity given above.

NOZZLE.
—Height and Distance of Jet.

TABLE A.—No. 7.
(From experiments of J. R. FREEMAN, 1888.)

Steamer) while stream is flowing, to maintain pressure at base of play-pipe, as per various lengths and kinds of 2½-inch Hose, as below.

Length 300 ft.			Length 400 ft.			Length 500 ft.			Length 600 ft.			Length 800 ft.			L'gth. 1000 ft.		
Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
12	11	8	14	12	9	16	14	10	18	16	11	22	20	12	26	23	14
23	21	16	27	25	18	31	29	19	35	32	21	44	39	25	52	47	29
34	32	24	41	37	27	47	43	29	53	48	32	66	59	37	78	70	43
46	43	32	54	50	35	63	57	39	71	64	43	88	79	50	104	93	57
57	53	40	68	62	44	78	71	49	89	80	53	110	98	62	130	116	71
69	64	48	81	75	53	94	86	58	106	96	64	131	118	75	156	140	86
80	75	56	95	87	62	110	100	68	124	112	75	153	138	87	182	163	100
92	85	64	108	100	71	125	114	78	142	128	85	175	157	100	208	186	114
103	96	71	122	112	80	141	128	88	160	145	96	197	177	112	234	209	128
115	106	79	136	125	88	156	143	97	177	161	106	219	197	125	260	233	143
126	117	87	149	137	97	172	157	107	195	177	117	241	216	137	....	256	157
138	128	95	163	149	106	188	171	117	213	193	128	263	236	149	....	....	171
149	138	103	176	162	115	203	185	127	230	209	138	....	256	162	....	....	185
161	149	111	190	174	124	219	200	136	248	225	149	....	....	174	....	....	200
172	160	119	203	187	132	235	214	146	266	241	160	....	....	187	....	....	214
184	170	127	217	199	141	250	228	156	....	257	170	....	....	199	....	....	228
195	181	135	231	212	150	....	242	166	....	....	181	....	....	212	....	....	242
207	192	143	244	224	159	....	257	175	....	....	192	....	....	224	....	....	257
218	202	151	258	237	168	....	....	185	....	....	202	....	....	237	....	....	....
230	213	159	....	249	177	....	....	195	....	....	213	....	....	249	....	....	....

TAKE NOTICE.—The above is pressure at hydrant head *while stream is flowing*. The corresponding Static Reservoir Pressure, or Fire-pump Pressure, must be greater than hydrant pressure by an amount equal to friction loss between hydrant-head and pump or reservoir.

1 1/4-INCH RING

Hydrant Pressure Required — Discharge

Indicated Pressure by Gauge attached at Base of Play-Pipe, and set level with End of Nozzle.	Effective or Static Pressure at Base of Play-Pipe.	Extreme Height of Jet. Average of Highest Drops, Vertical Jet, Still Air.	Extreme Height of Jet. Maximum Limit of Height as Good Effective Fire Stream with Moderate Wind.	Extreme Horizontal Distance reached by Jet. Average Extreme Drops at Level of Nozzle, with Still Air.	Extreme Horizontal Distance reached by Jet. Maximum Limit of Distance as Good Effective Fire Stream with Moderate Wind.	Gallons per Minute Discharged.	Pounds Pressure Required at Hydrant (or first column, through — Length 50 ft. Unlined Linen Hose.	Length 50 ft. Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 50 ft. Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Length 100 ft. Unlined Linen Hose.	Length 100 ft. Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 100 ft. Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Length 200 ft. Unlined Linen Hose.	Length 200 ft. Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Length 200 ft. Ordinary best quality Rubber-lined Hose.—Inside Smooth.
	lbs.	lbs.	ft.	ft.	ft.	ft.		lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
Feeble Streams.	5.	5.2	11	....	20	...	79	7	7	6	9	8	7	12	11	8
	10.	10.4	21	17	39	21	111	14	13	12	17	16	13	24	22	16
	15.	15.5	32	26	58	30	136	21	20	18	26	24	20	35	33	24
	20.	20.7	43	35	78	37	157	28	27	24	34	33	27	47	44	33
Ordinary Fire Streams. Fair.	25.	25.9	53	43	97	42	176	35	34	30	43	41	34	59	55	41
	30.	31.1	63	49	112	48	192	42	40	36	51	49	40	71	66	49
Good.	35.	36.2	73	56	126	51	208	49	47	42	60	57	47	82	76	57
	40.	41.4	83	63	138	55	222	56	54	48	69	65	54	94	87	65
	45.	46.6	93	68	149	59	236	63	61	54	77	73	61	106	98	73
Excellent.	50.	51.8	102	72	157	62	248	70	67	61	86	81	67	118	109	81
	55.	57.0	111	76	164	65	260	77	74	67	94	90	74	130	120	90
	60.	62.1	119	79	172	68	272	84	81	73	103	98	81	141	131	98
Unusually Strong Streams. (Difficult to hold Nozzle without special appliances.)	65.	67.3	127	81	178	71	283	90	88	79	111	106	88	153	142	106
	70.	72.5	133	84	185	73	294	97	94	85	120	114	94	165	153	114
	75.	77.7	138	86	191	75	304	104	101	91	129	122	101	177	164	122
	80.	82.8	143	88	196	77	314	111	108	97	137	130	108	188	175	130
	85.	88.0	147	90	201	79	324	118	115	103	146	138	115	200	186	138
	90.	93.2	149	92	206	81	333	125	121	109	154	147	121	212	197	147
	95.	98.4	152	94	211	82	342	132	128	115	163	155	128	224	208	155
	100.	103.5	154	95	215	84	351	139	135	121	171	163	135	236	218	163

Maximum limit of height as "fair" limit as "fair" limit as "fair" limit

NOZZLE.

—Height and Distance of Jet.

TABLE A.—No. 8.
(From experiments of J. R. FREEMAN, 1888.)

Steamer) while stream is flowing, to maintain pressure at base of play-pipe, as per various lengths and kinds of 2½-inch Hose, as below.

Length 300 ft.			Length 400 ft.			Length 500 ft.			Length 600 ft.			Length 800 ft.			L'gth. 1000 ft.		
Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs	lbs.
15	14	10	18	17	11	21	19	12	25	22	14	31	28	17	38	33	19
30	27	19	36	33	22	43	39	25	49	44	27	62	55	33	75	66	39
45	41	29	55	49	33	64	58	37	74	66	41	93	83	49	112	100	58
60	55	38	73	66	44	86	77	49	99	88	55	124	111	66	150	133	77
75	69	48	91	83	55	107	96	62	123	110	69	155	138	83	187	166	96
90	82	57	109	99	66	128	116	74	148	132	82	186	166	99	225	199	116
105	96	67	127	115	76	150	135	86	172	154	96	217	193	115	262	232	135
120	110	76	146	132	87	171	154	99	197	176	110	248	221	132	....	266	154
135	123	86	164	148	98	193	173	111	222	199	123	279	249	148	....	....	173
150	137	95	182	165	109	214	193	123	246	221	137	....	276	165	....	....	193
165	151	105	200	181	120	236	212	135	271	243	151	....	....	181	....	....	212
180	164	114	218	198	131	257	231	148	....	265	164	....	....	198	....	....	231
195	178	124	237	214	142	....	251	160	....	....	178	....	....	214	....	....	251
210	192	133	255	231	153	....	....	172	....	....	192	....	....	231	....	....	....
225	206	143	....	247	164	....	....	185	....	....	206	....	....	247	....	....	....
240	219	152	....	264	175	....	....	197	....	....	219	....	....	264	....	....	....
255	233	162	....	....	186	....	....	209	....	....	233	....	....	....	....	....	....
....	247	172	....	....	197	....	....	222	....	....	247	....	....	....	....	....	....
....	260	181	....	....	208	....	....	234	....	....	260	....	....	....	....	....	....
....	....	191	....	....	218	....	....	246	....	....	....	....	....	....	....	....	....

TAKE NOTICE.—The above is pressure at hydrant head *while stream is flowing.* The corresponding Static Reservoir Pressure, or Fire-pump Pressure, must be greater than hydrant pressure by an amount equal to friction loss between hydrant-head and pump or reservoir.

1 3/8-INCH RING

Hydrant Pressure Required—Discharge

INDICATED PRESSURE by Gauge attached at Base of Play-Pipe, and set level with End of Nozzle.		Effective or Static Pressure at Base of Play-Pipe.	Extreme Height of Jet. Average of Highest Drops, Vertical Jet, Still Air.	Extreme Height of Jet. Maximum Limit of Height as Good Effective Fire Stream with Moderate Wind.	Extreme Horizontal Distance reached by Jet. Average Extreme Drops at level of Nozzle, with Still Air.	Extreme Horizontal Distance reached by Jet. Maximum Limit of Distance as Good Effective Fire Stream with Moderate Wind.	Gallons per Minute Discharged.	Pounds Pressure Required at Hydrant (or first column, through								
								Length 50 ft.			Length 100 ft.			Length 200 ft.		
								Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
	lbs.	lbs.	ft.	ft.	ft.	ft.		lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
Feeble Streams.	5.	5.3	11	....	20	..	96	8	8	7	10	10	8	15	14	10
	10.	10.5	22	17	39	21	136	16	15	13	21	19	15	30	28	19
	15.	15.8	32	27	59	31	166	24	23	20	31	29	23	45	41	29
	20.	21.1	43	36	79	38	192	32	30	26	41	39	30	60	55	39
Ordinary Fire Streams. Fair.	25.	26.3	53	44	99	45	214	40	38	33	52	48	38	75	69	48
	30.	31.6	64	51	116	50	235	48	46	39	62	58	46	91	83	58
Good.	35.	36.8	74	58	131	55	253	55	53	46	72	68	53	106	97	68
	40.	42.1	85	64	144	59	271	63	61	53	82	77	61	121	111	77
Excellent.	45.	47.4	95	69	154	63	287	71	68	59	93	87	68	136	124	87
	50.	52.6	105	74	164	66	303	79	76	66	103	97	76	151	138	97
	55.	57.9	114	78	172	69	318	87	84	72	113	106	84	166	152	106
	60.	63.2	123	81	180	72	332	95	91	79	124	116	91	181	166	116
Unusually Strong Streams. (Difficult to hold Nozzle without special appliances.)	65.	68.4	131	84	187	75	346	103	99	85	134	126	99	196	180	126
	70.	73.7	137	86	194	77	359	111	106	92	144	135	106	211	193	135
	75.	79.0	142	88	200	79	371	119	114	98	155	145	114	226	207	145
	80.	84.2	147	90	206	81	383	127	122	105	165	155	122	241	221	155
	85.	89.5	150	92	212	82	395	135	129	112	175	164	129	257	235	164
	90.	94.7	153	94	217	84	407	143	137	118	186	174	137	....	249	174
	95.	100.0	156	96	222	86	418	150	144	125	196	184	144	....	263	184
	100.	105.3	158	97	227	88	429	158	152	131	206	193	152	....	....	193

Maximum limit as a "fair fire stream" is about 10 per cent. higher.

Maximum limit as a "fair fire stream" is about 12 per cent. farther.

NOTE.—The above figures for Ring Nozzle Discharges will apply to any ordinary form of Ring accurately enough for practical purposes, but apply especially to ordinary form of Ring Nozzle with square shoulder $\frac{1}{16}$ or $\frac{1}{8}$ inch deep.

Ring Nozzles with "undercut" or "knife-edge" shoulder, discharge, as ordinarily constructed, about three per cent. less than quantity given above.

NOZZLE.
—Height and Distance of Jet.

Table A.—No. 9.
(From experiments of J. R. Freeman, 1888.)

Steamer) while stream is flowing, to maintain pressure at base of play-pipe, as per various lengths and kinds of 2½-inch Hose, as below.

Length 300 ft.			Length 400 ft.			Length 500 ft.			Length 600 ft.			Length 800 ft.			L'gth. 1000 ft.		
Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
20	18	12	25	22	14	29	26	16	34	30	18	44	39	22	53	47	26
40	36	24	49	44	28	59	53	32	68	61	36	88	77	44	107	94	53
60	54	35	74	66	41	88	79	48	103	91	54	131	116	66	160	141	79
79	72	47	99	88	55	118	105	64	137	122	72	175	155	88	213	188	105
99	90	59	123	111	69	147	131	79	171	152	90	219	193	111	267	235	131
119	108	70	148	133	83	177	157	95	205	182	108	263	232	133	....	282	157
139	126	82	173	155	97	206	184	111	240	213	126	....	271	155	....	....	184
159	144	94	197	177	111	236	210	127	274	243	144	....	....	177	....	....	210
179	162	106	222	199	124	265	236	143	....	274	162	....	....	199	....	....	236
199	180	117	247	221	138	....	263	159	....	....	180	....	....	221	....	....	263
219	198	129	271	243	152	....	....	175	....	....	198	....	....	243	....	....	....
238	216	141	....	265	166	....	....	191	....	....	216	....	....	265	....	....	....
258	234	153	....	....	180	....	....	207	....	....	234	....	....	....	....	....	....
....	251	164	....	....	193	....	....	222	....	....	251	....	....	....	....	....	....
....	....	176	....	....	207	....	....	238	....	....	....	....	....	....	....	....	....
....	....	188	....	....	221	....	....	254	....	....	....	....	....	....	....	....	....
....	....	200	....	....	235	....	....	....	....	....	....	....	....	....	....	....	....
....	....	211	....	....	249	....	....	....	....	....	....	....	....	....	....	....	....
....	....	223	....	....	263	....	....	....	....	....	....	....	....	....	....	....	....
....	....	235	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....

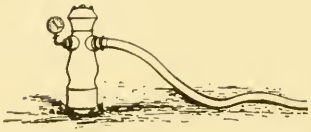
TAKE NOTICE.—The above is pressure at hydrant head *while stream is flowing.* The corresponding Static Reservoir Pressure, or Fire-pump Pressure, must be greater than hydrant pressure by an amount equal to friction loss between hydrant-head and pump or reservoir.

Fire Pump Inspection Tables.

Tables B No. 1 and B No. 2 are designed for the convenient use of water-works superintendents in measuring water supplied for filling tanks or other purposes, and are also of much convenience in testing a pump to see if it will throw its rated capacity.

Certain kinds of pumps, particularly rotary piston pumps, so-called, deteriorate rapidly by use or by long standing, and it is one of the regular duties of the insurance inspector or of the prudent mill superintendent to test such pumps from time to time. These tables, B No. 1 and B No. 2, may, by intelligent interpolation, give results certain to within about five per cent., but are not designed for the very accurate gauging of discharge of pumping engines referred to at the beginning of this paper (p. 304 and p. 330). For such work it is necessary to measure the pressure close to the base of play-pipe and will generally be necessary to caliper the diameter of nozzle very exactly, and to make some allowance for velocity of flow past piezometer dependent upon the peculiar conditions which may happen to exist. For such cases, therefore, special computations will generally have to be made. Had time allowed, the writer would have presented some tables containing the elements for making such computations expeditiously. They can, however, be very expeditiously computed by the aid of the very convenient and accurate velocity tables in the "Lowell Hydraulic Experiments."

To return to Tables B No. 1 and B No. 2, somewhat greater accuracy can be obtained with Table B No. 1 using only fifty feet of hose, than with greater lengths. The "Open Butt Table" is of necessity less accurate in its results than the other tables, since small differences in diameter of hose and in smoothness of its interior surface have relatively a very great effect in modifying the discharge. There are, however, very many practical purposes for which such a table is of a great deal of convenience, such, for instance, as in determining the number of gallons per minute which can be supplied at any portion of a water-works distribution system without lowering the pressure below a certain point. And is also of convenient application sometimes for roughly measuring water supplied to fill tanks.



PUMP INSPECTION

Discharge of Nozzles attached

HYDRANT PRESSURE indicated while Stream is flowing by Gauge attached to Hydrant, as shown.	1⅜-Inch Smooth Nozzle. Gals. per Min.			1¼-Inch Smooth Nozzle. Gals. per Min.			1⅛-Inch Smooth Nozzle. Gals. per Min.			1-Inch Smooth Nozzle. Gals. per Min.			⅞-Inch Smooth Nozzle, or 1-inch Ring Nozzle. Gals. per Min.		
Lbs. per sq. in.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
5	90	92	103	80	82	90	70	71	75	59	59	62	47	47	48
10	127	131	146	114	116	127	99	101	107	83	84	87	66	67	68
15	155	160	179	140	143	154	121	123	131	101	102	106	81	81	84
20	180	185	206	161	164	179	140	142	151	117	118	123	93	94	96
25	201	207	230	180	184	200	156	158	169	131	132	137	104	105	107
30	220	226	251	197	202	219	171	173	184	143	144	150	114	115	118
35	238	245	272	213	218	236	184	188	199	154	156	162	123	124	127
40	255	262	291	227	233	253	197	201	213	165	167	173	132	133	136
45	270	278	309	241	247	269	209	213	226	175	177	184	140	141	144
50	284	293	325	255	260	283	221	224	238	184	186	194	147	148	152
55	298	307	341	267	273	296	232	235	250	193	195	204	154	155	159
60	311	320	357	279	285	309	242	245	261	202	204	213	161	162	167
65	324	333	371	290	296	322	252	255	272	210	213	221	168	169	173
70	336	346	385	301	307	334	261	265	281	218	221	230	174	176	180
75	348	358	399	311	318	344	270	275	291	226	228	238	181	182	186
80	359	370	412	322	329	357	279	284	301	233	236	246	186	188	192
85	371	382	425	332	339	369	288	293	310	240	243	253	192	193	198
90	381	393	437	341	349	379	296	301	319	247	250	261	197	199	204
95	392	403	449	350	358	390	304	309	328	253	257	268	203	204	209
100	402	414	461	359	368	400	312	317	337	260	264	275	208	210	215

Quantities are stated in United States gallons of 231 cubic inches.

TABLES.

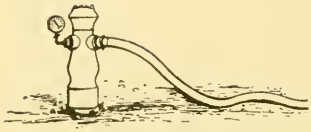
TABLE B.—No. 1
(From experiments of J. R. FREEMAN, 1888.)

to 50 Feet of $2\frac{1}{2}$-inch Hose.

¾-Inch Smooth Nozzle, or ⅞-inch Ring Nozzle. Gals per Min.			1⅜-Inch Ring Nozzle. Gals. per Min.			1¼-Inch Ring Nozzle. Gals. per Min.			1⅛-Inch Ring Nozzle. Gals. per Min.			HYDRANT PRESSURE.
Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Interior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Lbs. per sq. in.
36	36	37	75	76	84	66	67	70	56	56	59	5
50	50	51	108	110	118	94	96	101	80	81	84	10
61	61	62	132	135	144	115	117	124	98	99	103	15
71	71	72	152	155	167	133	135	143	113	114	119	20
79	80	81	170	174	187	149	151	159	126	128	133	25
86	87	88	187	191	205	163	165	175	138	140	145	30
93	93	95	201	206	221	176	179	189	149	151	157	35
100	100	101	215	219	237	188	191	202	159	162	168	40
106	106	108	229	233	251	200	203	214	169	172	178	45
112	112	113	241	245	264	211	214	226	179	181	188	50
117	117	119	253	257	277	221	224	237	187	189	197	55
122	122	124	264	269	289	231	234	247	196	198	205	60
127	127	129	275	280	301	240	244	257	205	206	214	65
132	132	134	285	291	313	249	253	267	212	213	222	70
137	137	139	295	301	324	258	262	276	219	221	230	75
141	142	144	305	311	334	266	270	285	226	228	237	80
145	146	148	314	320	345	274	279	294	233	235	244	85
149	150	152	323	329	355	282	287	303	239	242	252	90
153	154	156	332	338	364	290	295	311	246	249	259	95
158	159	161	340	347	374	298	303	319	253	255	266	100

NOTE.—The above figures for Ring Nozzle Discharges will apply to any ordinary form of Ring accurately enough for practical purposes, but apply especially to ordinary form of Ring Nozzle with square shoulder $\frac{1}{16}$ or $\frac{1}{8}$ inch deep.

Ring Nozzles with "under-cut" or "knife-edge" shoulder, discharge, as ordinarily constructed, about 3 per cent. less than quantity given above.



PUMP INSPECTION

Discharge of Nozzles attached

HYDRANT PRESSURE indicated while Steam is flowing by Gauge attached to Hydrant, as shown.	1⅜-Inch Smooth Nozzle. Gals. per Min.			1¼-Inch Smooth Nozzle. Gals. per Min.			1⅛-Inch Smooth Nozzle. Gals. per Min.			1-Inch Smooth Nozzle. Gals. per Min.			⅞-Inch Smooth Nozzle, or 1-inch Ring Nozzle. Gals. per Min.		
Lbs. per sq. In.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.
5	75	79	93	70	72	82	62	64	71	54	55	59	44	45	47
10	107	111	131	98	102	116	88	91	100	76	78	83	63	63	66
15	131	136	160	121	125	143	108	111	123	93	95	102	76	77	81
20	151	157	185	139	144	164	125	128	142	107	110	118	88	89	94
25	169	176	207	155	161	184	139	143	158	120	122	132	99	100	105
30	185	192	226	170	176	202	152	156	173	132	134	144	108	110	115
35	200	208	245	184	190	218	165	169	188	142	145	156	117	119	124
40	214	222	262	197	204	233	176	181	201	152	155	167	125	127	133
45	226	236	278	209	216	247	187	192	213	161	165	177	132	134	141
50	239	249	293	220	228	260	197	202	224	170	174	186	139	142	148
55	251	261	307	231	239	273	207	212	235	178	182	195	146	148	155
60	261	273	320	241	250	285	216	222	245	186	190	204	153	155	162
65	272	284	333	251	260	296	225	231	255	194	198	213	159	161	169
70	282	294	346	261	270	307	233	240	265	201	205	221	165	167	176
75	292	304	358	270	279	319	241	248	275	208	212	228	171	173	182
80	301	314	370	278	288	329	249	256	284	215	219	236	177	179	188
85	311	324	382	287	297	339	257	264	293	222	226	243	182	185	193
90	320	333	393	295	306	349	264	272	301	228	233	250	188	190	199
95	329	342	403	303	314	358	272	279	309	234	239	257	192	195	204
100	337	351	414	311	322	368	279	287	317	240	245	264	197	200	210

Quantities are stated in United States gallons of 231 cubic inches.

TABLES.

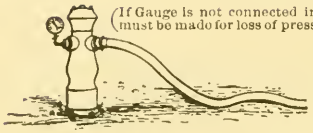
to 100 Feet of 2½-inch Hose.

Table B.—No. 2.
(From experiments of J. R. Freeman, 1888.)

¾-Inch Smooth Nozzle, or ⅞-inch Ring Nozzle. Gals. per Min.			1⅜-Inch Ring Nozzle. Gals. per Min.			1¼-Inch Ring Nozzle. Gals. per Min.			1⅛-Inch Ring Nozzle. Gals. per Min.			HYDRANT PRESSURE.
Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Unlined Linen Hose.	Inferior Rubber-lined Cotton "Mill Hose."—Inside Rough.	Ordinary best quality Rubber-lined Hose.—Inside Smooth.	Lbs. per sq. in.
34	34	35	67	69	78	60	62	68	52	53	57	5
48	49	50	94	97	110	85	87	96	74	75	81	10
59	60	61	115	120	135	104	107	117	90	92	99	15
68	69	71	133	138	155	120	123	135	105	107	114	20
76	77	79	149	154	174	134	138	151	117	119	128	25
84	84	87	163	169	191	147	151	165	128	130	140	30
90	91	93	176	182	206	159	163	179	139	141	151	35
97	97	100	188	195	219	169	174	191	148	151	162	40
103	103	106	200	207	233	180	185	203	157	160	172	45
108	109	112	211	218	245	190	195	214	165	169	181	50
113	114	117	221	229	257	199	204	224	173	177	189	55
118	119	122	231	239	269	208	213	234	181	185	198	60
123	124	127	241	248	280	217	222	244	189	192	206	65
128	129	132	250	258	291	225	230	253	195	199	213	70
132	133	137	259	267	301	233	239	262	203	206	221	75
137	138	142	267	275	311	240	246	270	209	213	228	80
141	142	146	275	284	320	247	254	279	215	219	235	85
145	146	150	283	292	329	255	261	287	222	226	242	90
149	150	154	291	300	338	262	269	295	228	232	249	95
153	154	158	299	308	347	269	275	302	234	238	255	100

NOTE.—The above figures for Ring Nozzle Discharges will apply to any ordinary form of Ring accurately enough for practical purposes, but apply especially to ordinary form of Ring Nozzle with square shoulder $\frac{1}{16}$ or $\frac{1}{8}$ inch deep.

Ring Nozzles with "under-cut" or "knife-edge" shoulder, discharge, as ordinarily constructed, about 3 per cent. less than quantity given above.



(If Gauge is not connected in this manner, proper allowance must be made for loss of pressure between Gauge and Hydrant.)

PUMP INSPECTION

The degree of accuracy attained in estimating of the two preceding tables — B, No. 1 and

HYDRANT PRESSURE indicated while Stream is flowing by Gauge attached to Hydrant, as shown. Lbs. per sq. in.	Quantity of Water Discharged per minute through ordinary 2½-inch Fire Hose, (United States Gallons of 231 cubic inches.) Open Hose Butt. No Play. Length 25 feet. Unlined Linen Hose.	Length 25 feet. Inferior Rubber-lined Cotton "Mill Hose." Inside Rough.	Length 25 feet. Ordinary best quality Rubber-lined Hose. Inside Smooth.	Length 50 feet. Unlined Linen Hose.	Length 50 feet. Inferior Rubber-lined Cotton "Mill Hose." Inside Rough.	Length 50 feet. Ordinary best quality Rubber-lined Hose. Inside Smooth.	Length 100 feet. Unlined Linen Hose.	Length 100 feet. Inferior Rubber-lined Cotton "Mill Hose." Inside Rough.	Length 100 feet. Ordinary best quality Rubber-lined Hose. Inside Smooth
10	231	242	297	176	188	242	132	140	188
15	283	297	363	217	230	297	161	171	230
20	326	343	419	251	266	343	186	199	266
25	365	383	468	281	297	383	208	222	297
30	400	420	514	307	326	420	228	243	326
35	432	453	554	332	352	453	246	262	352
40	462	484	593	355	376	484	264	280	376
45	490	514	630	377	399	514	280	297	399
50	516	542	664	398	420	542	294	313	420
55	541	568	696	416	440	568	308	328	440
60	565	594	726	434	460	594	322	343	460
65	588	618	756	452	480	618	336	358	480
70	610	641	784	470	498	641	348	371	498
75	632	663	812	486	515	663	360	384	515
80	652	685	....	502	532	685	372	397	532
85	672	706	....	518	548	706	383	409	548
90	692	727	....	533	564	727	394	421	564
95	712	747	....	547	579	747	405	432	579
100	730	766	....	561	594	766	416	443	594
110	766	806	....	588	623	806	436	465	623
120	800	....	....	614	651	....	456	486	651

Unlined Linen Hose: greater be 5 per cent. discharge would were of Style A, If Hydrant

Length 50 feet: greater be 3 per cent. discharge would were of Style A, If Hydrant

Length 100 feet: greater be 1½ per cent. discharge would were of Style A, If Hydrant

NOTE. — The above values are based on experiments with these kinds of Hose attached to a Chapman 4-way Independent Gate Hydrant (Coeff. Disch. by Expt. 0.71). So far as influence of kind of Hydrant upon discharge is concerned, the same values are correct enough for practical purposes, except as noted in margin of columns.

It will be noted that this table gives, for each length, the discharge through the best or smoothest hose, and gives, also, discharge for same length of Hose with roughest water-way. By use of a little judgment in interpolating between these two values, error, in ordinary use of table need not exceed 10 per cent.

TABLES.—"Open Butts." TABLE B.—No. 3.

discharge through "Open Butt" is, not nearly so great as may be attained by the methods B, No. 2—by reason of greater influence of form of Hydrant and differences in Hose.

with Couplings of 2½-inch Bore. Pipe or Nozzle attached.				Discharge through Open Butt of Hydrant, without Hose attached.	
Length 200 feet.		Length 400 feet.		HYDRANT PRESSURE indicated while Stream is flowing by Gauge attached to Hydrant, as shown. Lbs. per sq. in.	Discharge of One Nipple of Chapman 4-Way Independent Gate Hydrant. Diameter of Outlet exactly 2½ inches. Gals. per Min.
Inferior Rubber-lined Cotton "Mill Hose." Inside Rough.	Ordinary best quality Rubber-lined Hose. Inside Smooth	Inferior Rubber-lined Cotton "Mill Hose." Inside Rough.	Ordinary best quality Rubber-lined Hose. Inside Smooth.		
102	140	74	102	10	419
125	171	90	125	15	513
144	199	104	144	20	592
161	222	116	161	25	663
177	243	127	177	30	725
190	262	137	190	35	783
204	280	146	204	40	837
217	297	155	217	45	888
228	313	164	228	50	936
239	328	172	239	55	982
250	343	179	250	60	1026
260	358	186	260	65	1067
270	371	194	270	70	1108
279	384	201	279	75	1147
288	397	208	288	80	1184
297	409	215	297	85	1221
306	421	221	306	90	1256
314	432	227	314	95	1290
322	443	232	322	100	1326
338	465	243	338	110	....
354	486	254	354	120	....

NOTE.—Slight peculiarities of construction in different makes of Hydrants so affect discharge from open butts of Hydrants without Hose, that these figures apply only approximately to discharge of Hydrants in general.

By using good judgment in applying corrections as per notes below, results accurate to within 10 per cent. may generally be obtained.

(Style A.)—Ordinary Matthews (R. D. Wood & Co.'s) Hydrant without independent gates, inside corner being rounded off, will probably discharge about 20 per cent. more.

(Style B.)—Ordinary Chapman Hydrants without independent gates have square inside corners, and will probably discharge about 10 per cent. more.

For the ordinary Ludlow and Lowry Independent-Gate Hydrants or Chucks, figures in table are probably nearly correct.

If diameter of outlet is not exactly 2½ inches, an additional correction, as follows, must be made:—

Diameter.	Add.	Deduct.
2⅝	10 %	....
$2\frac{9}{16}$	5 %	....
$2\frac{7}{16}$	....	5 %
2⅜	....	10 %
2¼	....	19 %

TABLE C.

This is of much interest in illustrating what can be done with a given hydrant pressure, according to length and kind of hose used, and a study of this table shows up plainly facts which are far too little appreciated.

Note the difference in effective height and force of jet dependent on whether 500 or even 250 feet of hose is used, instead of 50 feet as is often used in practice tests by mill fire brigades.

Note, also, the inferiority of linen hose for use in long lines, such as are called for in connection with yard hydrants.

Thus, suppose that at a given mill there is available a static pressure of 80 pounds, and that the hydrant pipes are of such ample size that pressure at hydrant will be 70 pounds when streams are playing.

We see from table that if 1⅛-inch nozzles are used the height of the effective fire streams obtainable will be:

LENGTH OF HOSE.	GREATEST HEIGHT IN A MODERATE WIND AS A GOOD EFFECTIVE FIRE STREAM.	
	With ordinary 2½-inch unlined linen hose.	With smoothest 2½-inch rubber-lined hose.
Line 50 feet long	73 feet high.	81 feet high.
" 250 " "	42 "	61 "
" 500 " "	27 "	46 "

Note how quickly the inferiority of 2-inch hose develops when length much exceeds 50 or 100 feet.

Thus: with 250 feet of 2½-inch linen, effective height of stream = ... 42 feet.

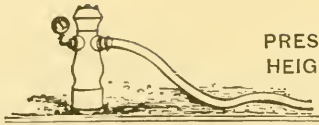
With 250 feet of 2-inch linen, effective height of stream = ... 20 "

Note, also, the effect which even ⅛ inch increase in diameter has in reducing friction loss in the hose.

Thus, with 500 feet of smoothest and best rubber-lined hose—

If diameter be exactly 2½ inches, effective height of stream will be..... 39 feet.

If diameter be ⅛-inch larger, effective height of stream will be..... 46 "



PRESSURE in lbs. per sq. in. indicated at base of play-pipe,
HEIGHT, greatest, as good effective fire stream in moderate wind,
VOLUME in U. S. gallons per minute,
} of Jet obtainable with a given Hydrant Pressure, according to Size, Kind, and Length of Hose used.

TABLE C.

(From experiments of J. R. FREEMAN, 1888.)

TAKE NOTICE.—This "Given Hydrant Pressure" is pressure at the Hydrant, measured as shown in sketch, *while stream is flowing.* The Fire Pump Pressure (or Static Reservoir Pressure) must, meanwhile, be greater than this Hydrant Pressure by an amount equal to friction-loss between Pump and Hydrant. No general statement of the magnitude of this friction-loss can be made, as it depends on length and size of pipe-system, and number of streams being drawn, and may be anywhere from 5 to 40 or more pounds. In a thoroughly first-class Mill Hydrant System, this friction-loss through main pipes and valves ought not to generally exceed 10 or 15 lbs.

		1-Inch Smooth Nozzle.				1⅛-Inch Smooth Nozzle. This is size now generally preferred for outside use.								1¼-Inch Smooth Nozzle.			
Available Hydrant Pressure while Stream is Flowing—>		40	60	80	100	40	50	60	70	80	90	100	120	40	60	80	100
50 ft. Ordinary 2-in. Unlined Linen Hose. (Diam. couplings 2-in., but inside diam. hose 2⅛-in.)	Noz. Pressure	24	35	47	50	19	23	28	33	37	42	47	56	15	22	29	36
	Height for Fire	42	58	71	78	34	41	49	56	61	67	72	81	28	41	52	61
	Galls. per Min.	144	174	202	226	164	180	199	216	229	244	258	282	181	219	252	282
50 ft. Ordinary 2½-in. Unlined Linen Hose. (Diam. couplings 2½-in., but inside diam. hose 2⅝-in.)	Noz. Pressure	31	47	63	78	23	35	41	48	55	62	69	83	24	36	47	59
	Height for Fire	52	71	81	88	49	59	66	73	80	84	88	93	44	61	74	84
	Galls. per Min.	164	202	233	261	199	222	241	261	279	296	312	342	230	282	322	359
50 ft. Best 2½-in. Rubber-Lined Cotton Hose. Inside surface very smooth. (Diam. couplings 2½-in., inside diam. hose 2⅝-in.)	Noz. Pressure	35	52	70	87	32	40	48	56	64	73	81	97	29	44	58	73
	Height for Fire	58	74	85	91	55	65	73	81	85	89	92	98	52	71	83	92
	Galls. per Min.	174	212	246	275	212	238	261	282	301	321	338	370	252	307	358	400
250 ft. Ordinary 2-in. Unlined Linen Hose. (Diam. couplings 2-in., but inside diam. hose 2⅛-in.)	Noz. Pressure	9	14	19	23	6	8	9	11	13	14	16	19	4	6	9	11
	Height for Fire	17	24	33	40	13	15	17	20	23	25	29	34	9	13	17	21
	Galls. per Min.	90	110	128	141	102	106	16	126	140	147	150	164	97	120	139	155
250 ft. Ordinary 2½-in. Unlined Linen Hose. (Diam. couplings 2½-in., but inside diam. hose 2⅝-in.)	Noz. Pressure	18	28	37	46	14	17	21	24	27	31	34	41	10	15	20	25
	Height for Fire	32	49	60	70	25	31	38	42	47	54	58	66	19	28	37	46
	Galls. per Min.	126	155	179	200	147	156	172	184	196	209	220	241	148	181	209	234
250 ft. Inferior 2½-in. Rubber-Lined "Mill Hose." Inside surface rough and uneven. (Inside diam. hose and coupling each exactly 2½-in.)	Noz. Pressure	20	30	39	49	15	19	22	26	30	34	37	45	11	17	22	28
	Height for Fire	35	51	63	72	27	34	39	46	52	58	61	70	21	32	41	50
	Galls. per Min.	132	161	184	206	146	164	177	192	206	220	229	252	155	192	219	247
250 ft. Best 2½-in. Rubber-Lined Cotton Hose. Inside surface very smooth. (Diam. couplings 2½-in., inside diam. hose 2⅝-in.)	Noz. Pressure	26	39	52	65	21	27	32	37	43	48	53	64	17	25	34	42
	Height for Fire	45	63	74	82	38	47	55	61	68	73	78	85	32	46	59	69
	Galls. per Min.	150	184	212	237	172	196	212	229	247	261	274	301	192	234	273	304

50p

500 ft. Ordinary 2½-in. Unlined Linen Hose. (Diam. coupling 2½-in., but inside diam. hose 2⅝-in.)	Noz. Pressure	12	18	24	30	8	11	13	15	17	19	21	25	6	9	12	15
	Height for Fire	21	32	42	51	15	20	23	27	31	34	38	44	13	17	23	28
	Galls. per Min.	101	125	144	161	106	126	140	146	156	164	172	188	120	139	162	181
500 ft. Inferior 2½-in. Rubber-Lined "Mill Hose." Inside surface rough and uneven. (Inside diam. hose and coupling each exactly 2½-in.)	Noz. Pressure	13	20	27	33	9	12	14	16	19	21	23	28	7	10	13	17
	Height for Fire	23	35	46	55	17	22	25	29	34	38	41	49	13	19	24	32
	Galls. per Min.	106	132	153	169	116	130	147	150	164	172	180	199	120	148	169	192
500 ft. 2½-in. Rubber-Lined Cotton Hose. Inside surface very smooth. (Inside diam. hose and coupling each exactly 2½-in.)	Noz. Pressure	18	20	35	44	13	16	19	22	26	29	32	38	9	14	19	23
	Height for Fire	32	45	58	68	23	29	34	39	46	50	55	63	17	26	35	42
	Galls. per Min.	125	150	174	196	140	150	164	177	192	202	212	232	139	175	204	225
500 ft. Standard 2½-in. Rubber-Lined Cotton Hose. Inside surface very smooth. (Diam. coupling 2½-in., but inside diam. hose 2⅝-in.)	Noz. Pressure	20	30	39	49	15	19	22	26	30	34	37	45	11	17	22	28
	Height for Fire	35	51	63	72	27	34	39	46	52	58	61	70	21	32	41	50
	Galls. per Min.	132	161	184	206	146	164	177	192	206	220	229	252	155	192	219	247
500 ft. 2¾-in. Rubber-Lined Cotton Hose. Inside surface very smooth. (Coupling 2½-in. bore, with taper. Inside diam. hose 2¾-in.)	Noz. Pressure	22	33	44	55	17	21	26	30	34	38	43	51	13	20	26	32
	Height for Fire	38	55	68	76	31	38	46	52	58	63	68	76	24	37	47	56
	Galls. per Min.	138	169	196	218	156	172	192	206	220	232	247	269	169	209	239	265
500 ft. 3-in. Rubber-Lined Cotton Hose. Inside surface very smooth. (Coupling 2½-in. bore, with taper. Inside diam. hose 3-in.)	Noz. Pressure	26	39	52	64	21	26	32	37	42	47	53	63	17	25	33	42
	Height for Fire	45	63	74	81	38	46	55	61	67	72	78	85	32	46	57	69
	Galls. per Min.	150	184	212	235	172	192	212	229	244	258	274	298	192	234	270	304
800 ft. Inferior 2½-in. Rubber-Lined "Mill Hose." Inside surface rough and uneven. (Inside diam. hose and coupling each exactly 2½-in.)	Noz. Pressure	10	14	19	24	7	8	10	11	13	15	16	19	5	7	9	11
	Height for Fire	18	24	33	42	12	15	18	20	23	27	29	34	9	13	17	21
	Galls. per Min.	93	110	128	144	96	106	119	126	140	146	150	164	105	123	139	155
800 ft. 2½-in. Rubber-Lined Cotton Hose. Inside surface very smooth. (Inside diam. hose and coupling each exactly 2½-in.)	Noz. Pressure	13	20	29	33	9	12	14	16	18	21	23	28	7	10	13	16
	Height for Fire	23	35	45	55	17	22	25	29	32	38	41	49	13	19	24	30
	Galls. per Min.	106	132	150	169	116	130	147	150	159	172	180	199	123	148	169	187
800 ft. Standard 2½-in. Rubber-Lined Cotton Hose. Inside surface very smooth. (Diam. coupling 2½ in., but inside diam. hose 2⅝-in.)	Noz. Pressure	15	23	31	38	11	14	17	19	22	25	28	33	8	12	16	20
	Height for Fire	26	40	52	62	20	25	31	34	39	44	49	56	15	23	30	37
	Galls. per Min.	114	141	164	181	126	147	156	164	177	188	199	216	132	162	187	209
800 ft. 2¾-in. Rubber-Lined Cotton Hose. Inside surface very smooth. (Coupling 2½-in. bore, with taper. Inside diam. hose 2¾-in.)	Noz. Pressure	17	26	35	44	13	16	19	22	26	29	32	39	9	14	19	24
	Height for Fire	30	45	58	68	23	29	34	39	46	50	55	64	18	26	35	44
	Galls. per Min.	121	150	174	196	140	150	164	177	192	202	212	234	139	175	204	230
800 ft. 3-in. Rubber-Lined Cotton Hose. Inside surface very smooth. (Coupling 2½-in. bore, with taper. Inside diam. hose 3-in.)	Noz. Pressure	22	32	43	54	17	21	25	29	33	38	42	50	13	19	25	32
	Height for Fire	38	54	67	75	31	38	44	50	56	63	67	75	24	35	46	56
	Galls. per Min	138	166	193	216	156	172	188	202	216	232	244	266	169	204	234	266

Jet Diagram.

The diagram shown in Fig. 63 is constructed for convenient practical use in determining the reach of a jet under various pressures, and thus may be classed along with Tables A, B and C. It is of interest as illustrating what points of a structure are within reach of a jet of a given pressure from a given stand-point.

It is constructed solely from data in Tables 9, 10, 11 and 12, and in part by averages of these same data shown in Figs. 56 and 57. Had its construction been in view at the time of the experiments, more co-ordinates for the path of the jet would have been observed, and some experiments with 45 degrees inclination of nozzle would have been made for defining that portion of the curves with greater exactness, and some photographs of the exact path of the jet would also have been made.

The three curves for the path of a jet under 50 pounds pressure at angles of 75, 60 and 33 degrees, shown by the light dotted lines of Fig. 63, were not defined by such frequent co-ordinates as desirable; those actually determined are shown by the triangles, and between these points the curves were sketched in along what appeared the probable course.

It was thought they were of interest for comparison with the theoretical values for path of jets in which no allowance is made for resistance of the air, as given in various text books, and certainly they have greater practical value. The writer regrets he did not determine the path by photography, as could have been very accurately done, and presents these few curves for the path of jets merely until some other observer shall present more complete ones.

The curves for showing extreme distance at which stream can be classed as a good fire stream were drawn from the average results of the experiments recorded in Tables 9 to 12. The values recorded in the several experiments do not agree very closely; and, indeed, it is, in observing an ordinary fire stream, hard to say just exactly where it ceases to be good, within 5 or perhaps even 10 feet. On bringing together into this plotting the data from the several tables the results for the different elevations did not harmonize in some cases within 10 per cent. Perhaps on some days I was inclined to be a little more critical in defining extreme point of a good stream than on others. Thus we are now inclined to think that in defining limits as good and fair streams

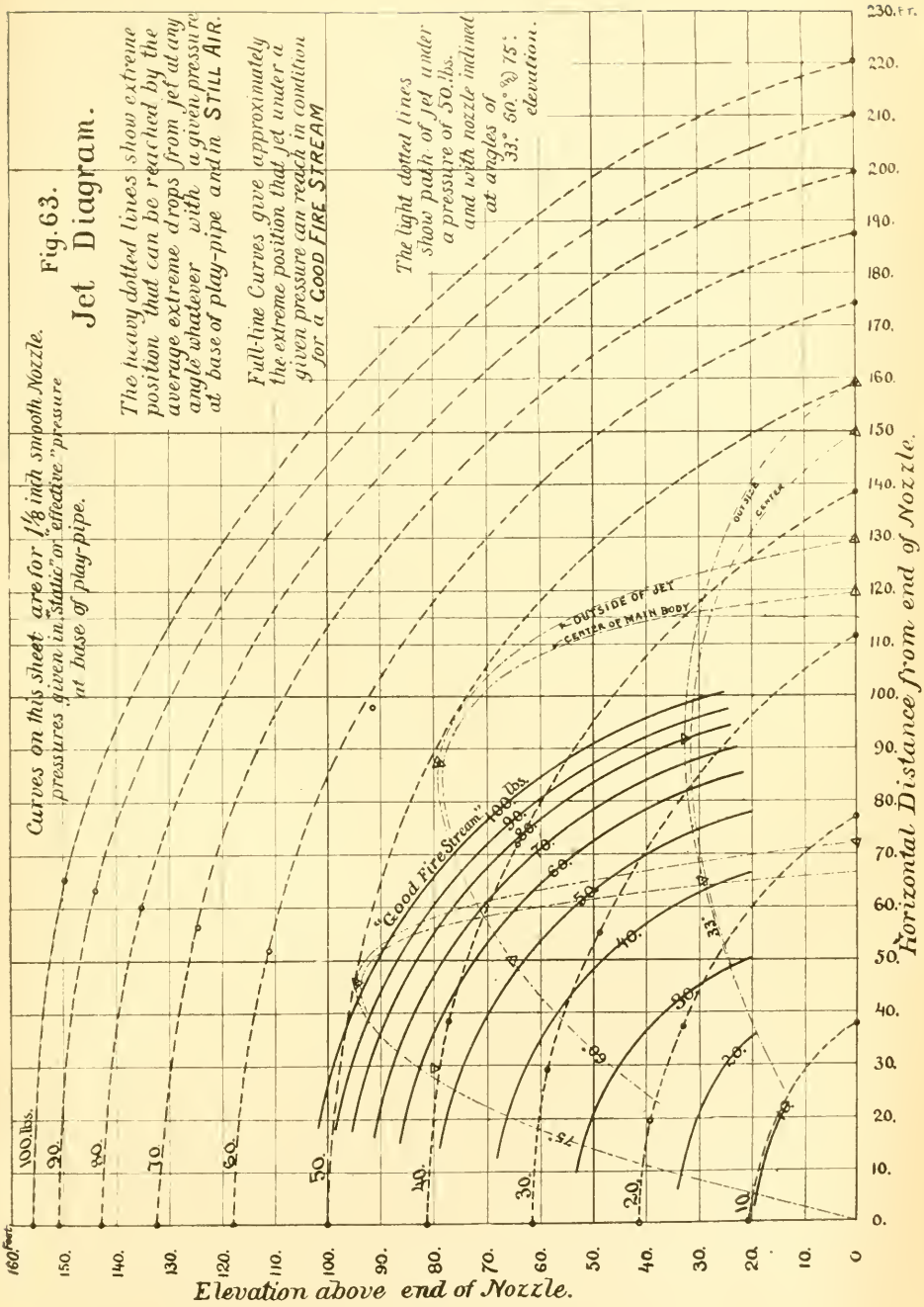


Fig. 63.
Jet Diagram.
The heavy dotted lines show extreme position that can be reached by the average extreme drops from jet at any angle whatever with a given pressure at base of play-pipe and in STILL AIR.
Full-line Curves give approximately the extreme position that jet under a given pressure can reach in condition for a GOOD FIRE STREAM
The light dotted lines show path of jet under a pressure of 50. lbs. and with nozzle inclined at angles of 33°, 50°, 75° elevation.
Curves on this sheet are for 1⅛ inch smooth Nozzle.
pressures given in "Static" or "effective" pressure at base of play-pipe.
"Good Fire Stream"
OUTSIDE OF JET
CENTER OF MAIN BODY
OUTSIDE
CENTER
100 lbs.
Elevation above end of Nozzle.
Horizontal Distance from end of Nozzle.

in Table No. 12 the judgment was, perhaps, a little severe. After studying the various observations, it appeared to the writer that circular arcs, as shown in Fig. 63, would represent the values for the several angles of inclination of nozzle with no greater probable error than for a more complicated form. In other words, the necessary indefiniteness of the determination and the variations actually found in extreme distance to which a fire stream can for all circumstances be classed as "good" do not warrant giving a more complicated form to the curve. For pressures of 10 pounds and 20 pounds, the fact that water continues in a solid body more nearly to end of jet makes it necessary to deviate these two curves from circular arcs. The curves for limiting positions of extreme drops of jet are at most points probably about as nearly correct as the nature of the problem admits of determining with any ordinary means and appliances, and probably error does not exceed 2 or 3 per cent.; but possibly these curves at points lying between lines passing through origin of co-ordinates, and at angles of 20 and 40 degrees to horizontal axis, being in the gap between the observations, are drawn a little too flat; or, in other words, perhaps actually the drops might reach an additional 5 per cent. or so beyond the curve. However, it was thought best not to depart too widely from the line joining the experiments. The points determined by experiment in the several curves are shown by small circles on the plotting.

APPENDIX.

Formulas for Practical Use.

The following formulas deduced from well-known hydraulic laws, and based upon data from the experiments described in the foregoing paper, are inserted here for handy reference. They are useful for special cases or for dimensions not included in the practical tables presented above.

Case I.—To compute the discharge of a nozzle having given the pressure at its base.

Let d = diameter of orifice of discharge in inches.

D = diameter of channel past gauge connection in inches.

c = co-efficient of discharge of the particular kind of nozzle used. (For nozzles of ordinary forms we may assume $c = 0.974$ for smooth nozzles, and $c = 0.74$ for ring nozzles. See pages 316 to 332, and Figs. 8 to 26, as a guide to values of co-efficient for special shapes of smooth nozzles, and see page 334 as a guide to proper value of co-efficient for special shapes of ring nozzles.)

h_1 = head in feet indicated by gauge at base of play-pipe (first corrected, if necessary, for index error and for difference in level between gauge and orifice).

p_1 = pressure in pounds per square inch indicated by gauge at base of play-pipe (corrected for level, if necessary).

h = effective head or static head at base of play-pipe in feet.

p = effective pressure or static pressure at base of play-pipe in pounds.

$\triangle$ = diameter of hose.

The pressure at base of play-pipe or nozzle may be stated either as indicated pressure h_1 or effective pressure h.

The pressure h_1 indicated by the gauge is less than h, the static pressure or pressure effective in producing discharge and projection of jet, by an amount equal to the pressure theoretically due to the mean velocity of flow past the piezometer. If indicated pressure is given we can obtain the effective pressure by the following formula: $h = \dfrac{h_1}{1 - c^2 \left(\dfrac{d}{D}\right)^4}$.

If we have given the indicated pressure as represented in Fig. 64, the discharge in cubic feet per second = Q may be computed thus:—

$$Q = \frac{.04\,374 \quad c\,d^2}{\sqrt{1 - c^2 \left(\frac{d}{D}\right)^4}} \sqrt{h_1} \qquad (1)$$

Or the discharge in gallons per minute = G.

$$G = \frac{29.83 \quad c\,d^2}{\sqrt{1 - c^2 \left(\frac{d}{D}\right)^4}} \sqrt{p_1} \qquad (2)$$

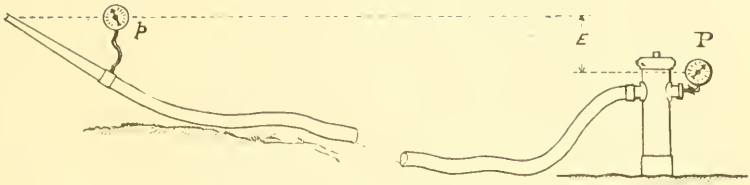


FIG. 64.

If we have the effective pressure $= h$ given, then

$$Q = .04\,374\ c\ d^2 \sqrt{h_1} \qquad (3)$$

$$G = 29.83\ c\ d^2 \sqrt{p_1} \qquad (4)$$

For approximate purposes, since h is ordinarily but little more than h_1, we may insert the indicated pressures in place of h and p in formulas Nos. 3 and 4.

To illustrate the error caused by this approximation, we may say that for the case of a $1\frac{1}{8}$-inch smooth nozzle on $2\frac{1}{2}$-inch hose, the computed discharge will be only 2 per cent. in error by its use.

For certain special cases other than fire stream nozzles, where D at base of nozzle is large, and gauge not perceptibly influenced by velocity of approach, the gauge reading (corrected for elevation and index error) will, without further correction, give h or p and thus permit formulas Nos. 3 or 4 to be accurately used.

Case II.—Given the hydrant pressure. Required the nozzle pressure: Let $P =$ pressure in top of hydrant in pounds per square inch.

(If orifice of nozzle is at an elevation higher or lower than hydrant gauge, then this gauge reading must first be reduced to the nozzle level by adding the pressure in pounds corresponding to difference of level if nozzle is lower than hydrant gauge, or subtracting this correction if nozzle is higher than gauge.)

Let $c =$ co-efficient of discharge of nozzle, either ring or smooth.

$o =$ co-efficient of discharge of hydrant nipple. For a sharp square-cornered entrance $o = 0.82$. For a perfect smooth round-cornered entrance $o = 1.0$. For ordinary hydrants this may be taken as 0.707, which was value found for a hydrant experimented upon.

$d =$ diameter of orifice of nozzle in inches.

$D =$ diameter of hydrant nipple in inches.

$L =$ length of hose in feet.

$F =$ the hose constant, for the particular size and kind of hose used.

$p =$ static pressure at base of play pipe in pounds per square inch.

$$p = \frac{P}{c^2\, d^4 \left[\frac{1}{D^4}\left(\frac{1}{o^2} - 1\right) + \frac{LF}{6472}\right] + 1.} \qquad (5)^*$$

This, being a general formula, is complicated.

Simpler formulas for special cases are given below.

The hose constant F is in fact the friction loss in pounds per 100 feet, with 240 gallons per minute flowing, for any given kind of hose, and depends for its value upon the diameter, roughness, obstructions and curves of the given line of hose.

In computing the foregoing practical tables, the following values of F were used:

For unlined linen hose.....$F = 30.$
Inferior rubber lined hose.....$F = 26.$
Smoothest rubber lined hose.....$F = 13.$

We can deduce the value of F for any given line of hose, as follows:

$$F = \frac{97{,}66}{\triangle^5}\, f + S + R \qquad (6)$$

In which f = friction loss in pounds per 100 feet for a flow of 240 gallons per minute in hose of exactly $2\frac{1}{2}$ inches diameter, and having the given quality of surface.

Experimental values of f for various given qualities of surface are given in Table No. 6, page 348, and the photographs of Fig. 47 may be referred to as showing with exactness the quality of surface to which these values apply.

---

* Formula No. 5 may be derived as follows (all heads being measured above horizontal datum plane passing through orifice of nozzle):

Let H = head in hydrant.
h = static head at base of play pipe.
h_1 = indicated head at base of play pipe.
h_2 = head due velocity in hydrant nipple.
h_3 = head lost by eddies due contraction in hydrant nipple.
h_4 = reduction of head at entrance, at hydrant nipple.
h_5 = head lost by friction in going whole length of hose.
V = mean velocity in hydrant nipple.
v = mean velocity in nozzle orifice.
c = co-efficient of discharge of nozzle.
o = co-efficient of discharge of hydrant nipple.

The total head is accounted for as follows:

$$H = h_4 + h_5 + h_1.$$

Diameter of up-stream and down-stream ends of hose may be assumed the same, and therefore $h_1 = h - h_2$. Also $h_4 = h_3 + h_2$.

Thus we may write $H = h_3 + h_5 + h$, we desire to express all these heads in terms of h Evidently $h_3 = h_4 - h_2$, but since, by a general hydraulic law,

$$h_4 = \frac{h_2}{o} \therefore h_3 = h_2\left(\frac{1}{o^2} - 1\right)$$

From these sources we transcribe the following values:

Sample A very smoothest solid rubber hose..... $f = 13.4$
" B ordinary solid rubber hose..... $f = 14.0$
" C very smoothest rubber lined fabric hose... $f = 14.1$
" K very rough rubber lined fabric hose..... $f = 25.5$
" J excellent leather hose..... $f = 21.5$
" L ordinary unlined linen or canvas hose..... $f = 33.3$

If the data for roughness of water way in any particular case happens to be most conveniently given in the shape of the constant commonly denoted by n in the well-known Chezy formula $v = n\,(r\,s)^{\frac{1}{2}}$, see page 358 (or sometimes denoted by c in the Kutter formula), we may, from the given value of n (or c), deduce f, since $f = \dfrac{204600.}{n^2}$

S, in formula No. 6, denotes the pounds pressure lost per 100 feet in length, due to sinuosity in course of hose, and with 240 gallons per minute flowing experiments on page 361 indicate that we may ordinarily call $S = 1$.

R, in formula No. 6, denotes the pounds pressure lost per 100 feet of hose, with 240 gallons per minute flowing, due to reductions of area or obstructions in the hose.

For the best hose this term R is zero, and may thus ordinarily be omitted, while for old style $2\frac{1}{4}$-inch-bore couplings on $2\frac{1}{2}$-inch hose, we see, from page 370, $R = 0.50$.

Obstructions, such as diaphragms or washers, may be allowed for as per values on page 372.

For all ordinary purposes we may

Assume $o = 0.707$ or the value actually found for co-efficient discharge of Chapman independent gate hydrant.
" $C = .974$ for ordinary smooth nozzles.
" $C = .74$ for ordinary ring nozzles.

Now, to find h_2 in terms of h:

$$\frac{h_2}{h} = \frac{V^2}{v^2}, \text{ but evidently } \frac{V}{v} = \frac{c\,d^2}{D^2} \therefore h_2 = \frac{c^2\,d^4}{D^4} \cdot h.$$

Therefore, $h_3 = \dfrac{c^2\,d^4}{D^4}\left(\dfrac{1}{o^2} - 1\right) h.$

Next, the friction in the hose being simply proportional to its length and to the square of the quantity flowing, and calling $p = 2.3095\,h$, we have

$$\frac{h_5}{2.3095\,F} = \frac{L}{100}\,\frac{G^2}{(240.)^2} \therefore h_5 = \frac{L\,F\,G^2}{2494000.}$$

Substituting the value of G from Formula No. 1, we have $h_5 = c^2\,d^4\,\dfrac{L\,F}{6472}\,h.$

Returning to our original equation, and substituting these values,

$$H = \frac{c^2\,d^4}{D}\left(\frac{1}{o^2} - 1\right) h + \frac{c^2\,d^4\,L\,F}{6472}\,h + h.$$

Whence $h = \dfrac{H}{c^2\,d^4\left[\dfrac{1}{D^4}\left(\dfrac{1}{o^2} - 1\right) + \dfrac{L\,F}{6472}\right] + 1.}$

And expressing pressures in pounds instead of feet-head, we get Formula No. 5.

Introducing these values we have the following formulas for obtaining (static) nozzle pressure when hydrant pressure is known:

For smooth nozzles $p = \dfrac{P}{.949\ d^4 \left(\dfrac{1}{D^4} + \dfrac{LF}{6472}\right) + 1}$ (7)

For ring nozzles $p = \dfrac{P}{.548\ d^4 \left(\dfrac{1}{D^4} + \dfrac{LF}{6472}\right) + 1}$ (8)

The following formula will serve for approxima'e purposes. The error involved in its use is small, consisting only of the assumption that O = co-efficient discharge hose nipple = 1.0, or a neglect to include the small waste of energy due to the contraction at entrance to hose. For lengths of hose greater than 200 feet, and nozzles not larger than $1\frac{1}{8}$ inch, this is so small relatively to loss within the hose itself, that the error due the use of the following formula will be of no practical importance.

For smooth nozzles $p = \dfrac{P}{\dfrac{LFd^4}{6822} + 1}$ (9)

For ring nozzles $p = \dfrac{P}{\dfrac{LFd^4}{11819} + 1}$ (10)

Having thus determined the effective pressure p at base of playpipes, the discharge may be readily computed by formula No. 4. The extreme height to which a jet of the given size will rise in still air under this pressure is shown by diagram in Fig. 56, page 390.

The maximum height for practical use as a first-class fire stream, under adverse conditions as to wind, is shown by Fig. 56*b*.

The greatest horizontal distance that a stream of the given size and pressure can attain, is shown by the diagram Fig. 57, page 390. And the greatest horizontal distance as an effective fire stream, by Fig. 57*b*.

The jet diagram, Fig. 63, contains much information on these points condensed into very compact form; or if formulas are desired:

The extreme height to which a jet will rise under this pressure in still air may be found from the formulas given on pages 381 and 392, namely:

$$J = h - .00\,135\,\frac{h^2}{d_1} \qquad (11)$$

In which J = height in feet to which average extreme drops of a vertical jet will rise in still air above level of nozzle.

d_1 = diameter of jet in inches. (For ring nozzles $d_1 = 0.86\,d$.)

h = static head in feet acting upon nozzle.

(Formula No. 10 gives pressure in pounds $= p$.

To reduce this to feet for use in No. 11, $h = 2.3095\,p$.)

Having determined J for extreme drops, the height attained by center of main body at top of jet will be about 90 per cent. of J for jets 100 feet high, or 94 per cent. of J for jets 75 feet high.

Height as a thoroughly first-class fire stream will be about

82	per cent.	of J	for jets where	$J =$ 50	feet.
79	"	"	"	75	"
73	"	"	"	100	"
67	"	"	"	125	"
63	"	"	"	150	"

Discharge of Hose with Open Butts.

Required the hydrant pressure necessary to produce a given discharge. This is given by the following formula:

$$P = G^2 \left(\frac{0.001\ 123}{D^4\ o^2} + 0.000\ 000\ 1736\ L\ F \right) \tag{12}$$

The more common problem is: What will be the discharge under a given hydrant pressure. This is given by the formula:

$$G = \frac{\sqrt{P}}{\sqrt{\frac{0.001\ 123}{D^4\ o^2} + 0.000\ 000\ 1736\ L\ F}} \tag{13}$$

The above are general formulas for any diameter of hose and any degree of contraction at entrance to hydrant nipple.

For the special case of $2\frac{1}{2}$-inch hose and a hydrant like the Chapman Valve Company's Independent Gate Hydrant, whose co-efficient discharge = .71, the equation becomes:

$$G = \frac{\sqrt{P}}{\sqrt{0.000\ 061 + 0.000\ 000\ 1736\ L\ F}} \tag{14}$$

In the above formulas—

G represents discharge in United States gallons per minute.

P represents pressure at hydrant in pounds per inch (measured as shown in Fig. 64).

D represents diameter of bore of hydrant nipple in inches.

o represents co-efficient of discharge of hydrant nipple.*

L represents length of hose in feet.

F represents friction loss for this particular size and kind of hose in pounds per 100 feet in length with 240 gallons flowing. (See pp. 458 and 459.)

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* For a sharp square-cornered entrance..... $o = 0.82$

For a perfectly smooth rounded entrance..... $o = 1.00$

For ordinary hydrants, to allow for disturbances, etc., the value found experimentally for the Chapman Hydrant had best be used, viz..... $o = .707$

DISCUSSION.

Clemens Herschel, M. Am. Soc. C. E.—I think this paper is one distinctly advancing our knowledge of hydraulic facts. Witness the measurement and consequent result of different degrees of roughness on interior of pipes; the beautiful portrayal of the distribution of velocities in jets, skillfully measured, and as skillfully worked up in the results; the tests of different forms of nozzles; the discovery that a nozzle, taken in conjunction with the measurement of the pressure at its base, forms a ready and efficient metering apparatus for certain purposes.

Mansfield Merriman, M. Am. Soc. C. E.—The numerous experiments detailed in this interesting and valuable paper deserve a more extended discussion than I am able to give, for a great deal of study could easily be devoted to the many new facts and conclusions which are presented. I think it was Galileo who said that it was easier for him to understand the laws governing the motion of the distant planets than the laws that control the flow of the water which is everywhere around us. This remark is as true to-day as it was two and a-half centuries ago. In hydraulics experiment rather than theory must be our guide, and if Mr. Freeman has by his careful observations weakened one or more theories previously held, his paper is all the more welcome.

That the tables of Ellis give the discharge from nozzles much too small has been apparent for some time, so that in my book on hydraulics I made no mention of his results, except that the co-efficient of discharge for smooth nozzles appeared to be about 30 per cent. greater than for ring nozzles. Mr. Freeman now shows that the mean co-efficients are about 0.97 and 0.74. The results that he deduces concerning the precision of the smooth nozzle as an apparatus for measuring water seem to me to be a valuable addition to our hydraulic knowledge. I hope to see his suggestion regarding their use in pumping tests carried out, for it is probable that the common method of relying upon the piston displacement often gives the amount of water lifted as too large.

It must certainly be true, as the paper shows, that the form of the smooth nozzle has but a slight influence on the efficiency of the stream as compared with other elements. I was not, however, entirely prepared for the conclusion that the conical nozzle is superior to all others in this slight influence. The "uniform acceleration nozzle," used in the experiments, had its tip straight for one inch in length, and although this probably exercised but a slight difference upon the result, yet it would have been as well to have made the entire profile in strict accordance with the equation of the curve. In this connection it may be noted

that the definition of uniform acceleration given on page 403 is incorrect. Uniform acceleration does not imply that a force is instantly applied and then instantly removed, but that the force acts constantly and with uniform intensity in exactly the same manner as the force of gravity. Hence it might be supposed, if the particles of water in their passage from the play-pipe to the tip of the nozzle are subject to the same uniform force or impulse, that there would be less loss of head due to eddies and cross-currents than when the form allows a variation in this force. It is indeed only for this reason, as I take it, that the form called the "uniform acceleration nozzle" has been supposed likely to give a higher co-efficient of discharge than other kinds.

The piezometer couplings used by Mr. Freeman are designed in accordance with what I believe to be sound hydraulic theory, and are more likely to register the mean static pressure of the water flowing through them than any other form I can imagine. Piezometric measurements have been so often subject to errors due to imperfect kinds of connection that it is to be hoped that this method may become a standard for measurements of flow in pipes as well as in hose.

The formula given by Weisbach for the co-efficient of discharge in cases of imperfect contraction seems unnecessarily complex in form if the true law of variation can be represented by a hyperbola, as the experiments plotted in Fig. 46 of the paper seem to indicate. Assuming 0.61 as the mean co-efficient for perfect contraction, and 1.00 for the case of no contraction, I find for the co-efficient of discharge for imperfect contractions, the formula,

$$c = 0.571 + \frac{0.0429}{1.1 - r}$$

in which r is the ratio of the diameter of the orifice to the diameter of the channel of approach. The degree of agreement between the actual co efficients found by Mr. Freeman for square ring nozzles and those computed from this formula is shown by the following comparison :

For	$r =$	0.500	0.833	0.848	0.886	0.950	1.000
By experiment,	$c =$	0.634	0.736	0.729	0.742	0.866	0.975
By formula,	$c =$	0.643	0.732	0.741	0.771	0.857	1.000
Per cent. difference,		+1.4	—0.6	+1.6	+3.9	—1.0	+2.6

The ingenious method used by Mr. Freeman for determining the distribution of velocities throughout the cross-section of jets deserves especial notice, for it is certainly a matter of much surprise and congratulation that the instrument known as Pitot's tube can be so modified as to be used for this purpose with such satisfactory results. The only criticism I have to offer is that its co-efficient may probably be subject to some variation with the velocity, particularly for such a minute orifice, whereas in the discussion of the experiments it was regarded as the same for all points in the same cross-section. If the

curve for the jet from the $1\frac{1}{8}$ inch pipe, shown in Fig. 61, represents the variation of velocities within the pipe as well as in the stream after leaving it, it is no wonder that formulas for flow based upon the mean velocity have proved unsatisfactory. Probably, however, the relative variation is much less marked for the usual velocities of flow than for the high velocities that were used in the experiments.

Mr. Freeman.—Professor Merriman states that the definition of uniform acceleration on page 403 is incorrect. This was a special definition intended to bring out forcibly the fact that with the so-called uniform acceleration nozzle the whole available accelerating force was instantly applied in its full magnitude instead of being applied first gently and then with gradually increasing intensity, thus producing an acceleration which gradually increases up to the instant of discharge, as is the case with a plain conical nozzle.

I still hold to the correctness of the original statement. If a force acts only with a uniform intensity, then it, of course, does not act with varying intensity, and thus, not gradually increasing, is applied instantly at its full intensity, just as is the force of gravity when one cuts the string suspending a body.

We desired to call attention to this from the fact that there seems to be a sort of popular misapprehension on this very point, and hence a notion that the uniform acceleration nozzle would have special virtues. I shared these notions myself once, but on considering the matter in the light above stated, saw this notion was without reason or analogy. For, to state the case in a popular and not strictly scientific manner, if it were desired to get a train of cars from a state of rest or a slow velocity up to a very high velocity with the least possible shock, the engine driver should open his throttle gradually and not give it full force of steam at a jump. This analogy, of course, is of itself no demonstration of the superiority of a conical nozzle. In fact, as I said in the body of the paper, I consider the question to be one, first, of so shaping the nozzle that there shall be no tendency to form cross-currents or eddies; and, second, of keeping the channel large and the velocity and friction loss consequently low, up to as near the point of discharge as is practicable.

A very fair question is suggested in Professor Merriman's discussion as to whether the co-efficient of the modification of Pitot's tube used in determining the distribution of volocities may not have varied with the different velocities found in different points in the cross section. I am very glad this point is brought up, as it reminds me to say that I took care to investigate this question by experimenting upon the distribution of velocity in other jets from some of the same nozzles at much lower pressures, and found the ratio of the central velocity to the theoretical velocity due the head, and also the proportional distribution of

velocity to be substantially the same as for the very high velocity shown in Fig. 62. As to his further question implied as to whether the distribution of velocity in the jet from pipes of uniform diameter shown in Fig. 61 is the same as exists within the pipe itself, it seems to me there can be little doubt that this is the case, for the velocity of the particles of water was measured in less than a thousandth part of a second after they had left the tube, and their inertia must have continued them for this length of time at about the same rate of speed as before they left the tube.

It may be of interest to note the approximately parabolic form of this curve.

E. Kuichling, M. Am. Soc. C. E.—The description and record of the experiments made by Mr. Freeman constitute an important and valuable contribution to the subject of practical hydraulics; and as the array of facts presented is exceedingly numerous and relates to a variety of distinct investigations, a long period of time would be required to give to each one of these investigations the careful study which legitimate criticism involves. In the absence of opportunity to devote more than a few hours at the present time to a review of the paper, the following remarks, relating only to the subject of the loss of pressure by friction in rubber fire hose, are offered.

To many persons the knowledge of the physical laws which govern the results of a series of experiments is of far more interest than the long array of figures and tables necessarily attendant upon all investigations of the character under consideration. These laws, when expressed in the condensed form of a general equation, are generally far more convenient for an engineer's ordinary use than a long table, which occupies much room in a note-book and requires a long time to copy. By means of the equations, moreover, valuable comparisons with other similar expressions can be instituted with ease and brevity, thereby facilitating the discovery of errors on the one or the other side. To illustrate this point it may be said that the law governing the loss of pressure by the friction of the water in its passage through fire hose can approximately be expressed by an equation of the following general form:

$$(1) \qquad p = a\,Q^2; \quad \text{or} \quad Q = b\sqrt{p}. \qquad (2)$$

where (p) denotes the loss of pressure in pounds per square inch by friction in every length of 100 feet of fire-hose; (Q) denotes the discharge through such hose in gallons per minute, and (a and b) certain empirical constants deduced from the results of the experiments, and dependent upon the diameter and quality of the hose used. In the foregoing expression $b = \sqrt{\frac{1}{a}}$, and the other elements are taken for convenience in the same terms given in the tables. The above formulas express that the friction loss in pressure varies with the square of the

discharge, or that the discharge varies with the square root of the friction loss in pressure, the exact numerical values depending upon the value of (a) found by trial. In this compact form, the results of a vast amount of labor performed by the author can be made practically available.

To find whether the experimental results obtained by the author in the above-named subject are in accordance with the general law just enunciated, we may deduce the value of the co-efficient (a) or (b) by substituting the measured values of (p) and (Q) in the expressions $a = \frac{p}{Q^2}$ or, $b = \frac{Q}{\sqrt{p}}$; and if the deduced value is substantially the same in every instance, it may be concluded that the said law has been established. Taking the case of experiments No. 21 to 31 inclusive, contained in Table No. 5, page 342, which were made with Sample K of $2\frac{1}{2}$-inch woven cotton, rubber-lined mill hose, having an average internal diameter of 2.53 inches with an average water-pressure of 49 pounds per square inch, we will obtain the following results:

TABLE A.

1	2	3	4	5	6
No. of Experiment.	Loss of Pressure per 100 feet of hose. (p) pounds.	Discharge in gallons per minute (Q).	Deduced value of co-efficient (a).	Computed value of (Q) with $a = 0.0004239$.	Computed value of (Q) by Formula 8.
21	1.12	54.4	0.00037846	51.40	48.81
22	2.63	81.0	0.00040083	78.77	80.18
23	5.07	108.7	0.00042909	109.36	
24	6.83	125.4	0.00043433	126.93	132.03
25	6.91	126.8	0.00042977	127.68	132.79
26	11.03	159.1	0.00043575	161.31	
27	14.87	186.3	0.00042843	187.29	
28	18.93	211.3	0.00042399	211.32	215.60
29	22.29	229.8	0.00042210	229.31	232.52
30	24.88	243.6	0.00041927	242.27	244.53
31	28.06	259.9	0.00041541	257.28	258.34
	Average of entire 11 values=		0.00041977		
	Average of last 10 values=		0.00042390		

An examination of these values of (a) in the fourth column of the foregoing table shows a striking uniformity in every instance except the first; and if we consider the latter as being affected by some error of observation, the average of the ten following values of (a) gives: $a = 0.000\,4239$, whence $b = 48.570$. The results of this series of experiments may therefore be expressed by the formulas:

$$(3) \qquad p = 0.0004239\ Q^2, \quad \text{or} \quad Q = 48.57\sqrt{p}, \qquad (4)$$

from the latter of which the values of (Q) contained in the fifth column of the table were computed, for the sake of comparison, with the actually observed values contained in the third column. The agreement between these values of (Q) is so close that we may now regard the law above stated as fairly well demonstrated, and that for the particular kind of fire-hose represented by sample K, the loss of pressure due to friction may accurately be calculated from equation 3.

It may also be of interest to compare the above results with those obtained in 1877 at Springfield, Mass., by Mr. George A. Ellis, C. E., to which the author has referred. The details of these latter experiments are by no means as completely given as in the case of Mr. Freeman's work, and the record of the essential experiments on the loss of pressure in hose, as printed in Table No. 6, page 47, of Mr. Ellis' little manual, gives some evidence of more of less interpolation and modification of the actual observations. However, taking out a number of the data therein given, and following the same course pursued above, we obtain for the 2½-inch rubber hose specified:

TABLE B.

1	2	3	4	5	6
No. of Experiment.	Loss of pressure per 100 feet of hose. (p) pounds.	Discharge in gallons per minute (Q).	Deduced value of co-efficient (a).	Computed value of (Q) with $a = 0.00036414$.	Computed value of (Q) by Formula 7.
1	1.40	50.0	0.00056000	62.01	55.98
2	2.51	80.0	0.00039219	83.02	80.05
3	4.34	110.0	0.00035868	109.17	109.02
4	7.90	150.0	0.00035111	147.29	149.57
5	14.15	200.0	0.00035375	197.13	200.17
6	22.40	250.0	0.00035840	248.02	249.58
7	32.65	300.0	0.00036278	299.44	297 61
8	44.90	350.0	0.00036653	351.15	344.31
9	59.15	400.0	0.00036969	403.04	389.81
		Average of last 8 values = 0.00036414			

By rejecting the first of these data, we obtain an average value for the co-efficient, $a = 0.00036414$, whence $b = 52.404$, so that according to Ellis' figures we should have for said fire-hose:

$$p = 0.00036414\ Q^2; \quad \text{or} \quad Q = 52.404\ \sqrt{p}. \qquad (6)$$

(5)

It will accordingly be seen that for the same discharge (Q), the loss of pressure by friction in pounds per square inch per 100 feet length of hose will be 1.079 times greater by Mr. Freeman's figures than by Mr. Ellis' data, or for the same loss of pressure (p), the discharge will be 1.164 times greater by Mr. Ellis' figures than by Mr. Freeman's.

An attempt was also made to find a formula which would represent

the law governing the loss of pressure by friction in such hose somewhat more accurately than by the expression $Q = b\sqrt{p}$, especially within the range of discharges usually observed in practice. In the case of Mr. Ellis' data, the writer found that the formula

$$Q = 191.88\sqrt[4]{p}\,(1.2332\sqrt[12]{p} - 1) \qquad (7)$$

would give the results exhibited in column 6 of Table B, which agree much more closely with the alleged actual observations in the third column of said table, between the limits $Q = 80$ and $Q = 250$, than the computations obtained from equation 6. A similar process of deduction applied to Mr. Freeman's data in Table A gives

$$Q = 162.98\sqrt[4]{p}\,(1.279\sqrt[12]{p} - 1) \qquad (8)$$

which yields the values contained in the sixth column of said Table A. The agreement between these values and the recorded observations is not as good as when equation 4 is used, and hence we may infer that equation 8 fails to express properly the law governing the loss of pressure due to friction. This inference is further warranted by the failure of a formula similar to equations 7 and 8, to agree with the results of experiments with iron pipes, and the form of expression is alluded to here simply because it fits the Ellis tables much closer than any of the others which the writer tried to apply in the course of a long examination of said data; but as Mr. Freeman's careful experiments now clearly point out that the law relating to the flow of water through fire-hose is the same as that pertaining to flow in other pipes, there is no occasion to resort to such complex formulas as those last mentioned.

With reference to the height of jets, it may be of interest to mention briefly the following few observations made with unusually large vertical streams during the official trial of the pumping machinery of the Rochester, N. Y., Water-works, on February 18th, 1874:

Diameter o orifice of ring nozzle.	Pressure in 16-in main at base of nozzle. Pounds per square inch	Discharge in gallons per minute.	Vertical height of Jet in feet above street surface.	REMARKS.
2 inches.	160	1 215	210.34	Three lines of 2½-inch fire hose from two hydrants to Siamese coupling and play-pipe.
3 "	170	2 778	285.98	Height measured by vertical angles from three different stations with different instruments and observers. Figures are averages.
4 "	165	4 938	294.40	Do. do.
5 "	120	6 463	256.80	Do. do.

The pressures in the second column of the foregoing Table are the averages of frequent observations on a Bourdon gauge, while each experiment was in progress, the gauge being located directly on the street main, and at one end of the 16 × 10-inch branch to which the vertical play-pipe was attached. According to the writer's recollection, the variation of the pressure gauge during the trials was comparatively slight, not exceeding a very few pounds. The discharge was estimated from the number of strokes made per minute by the pumps, as determined by counters, and probably contains only a small allowance, if any, for slip. The play-pipe for the 3, 4 and 5-inch streams was about 7 feet in length, and that for the 2-inch stream was about 5 feet long. All of the nozzles were provided with rings at the mouth. The observed heights refer to the extreme jets of solid water, as the spray was almost invisible through the telescopes. A 4-inch stream was also thrown, under an inclination of about 40 degrees from the horizon, to a distance of 465 feet, measured along the level street. It is greatly regretted that more precautions to insure a higher degree of accuracy for the above data were not taken at the time, and therefore they can only be regarded as approximations so far as pressure and discharge are concerned. For this reason, also, no attempt was made by the writer to compare the observed heights with the results obtained from the various formulas then available.

There is evidently a vast amount of exceedingly valuable material in Mr. Freeman's paper, and it is earnestly hoped that he may be induced to supplement his present work by a digest of the various analyses and mathematical investigations to which he has called attention.

I am highly pleased with the elegant exposure of the fallacy of the superiority of the ring nozzle and the undercut nozzle, as I have never been able to see, either from theory or practice, wherein their alleged superiority lies, yet nearly every mechanic and fireman whom I have met in this country insists that the ring or the undercutting is a vast improvement over the smooth bore. The question of size of hose is also of immense importance and significance, and I rejoice to see another champion enter the lists to battle for 3-inch hose, whose use in communities where the buildings are, with few exceptions, highly inflammable, and where the water pressure available is light, would be sure to effect a great reduction in the annual losses by fire.

Mr. Freeman.—The fact brought out by Mr. Kuichling's Table A, that the friction loss varies almost exactly as the square of the velocity is illustrated in what seems to me a more clear and striking manner by the plottings of Fig. 48, where the full lines are the curves of observations, and the dotted lines, which coincide so closely with them, are mathematical curves of squares.

Referring to the equation with twelfth roots, which Mr. Kuichling

so ingeniously devised to fit the Ellis Experiments, I fully coincide with the view implied by Mr. Kuichling, that the complex form was not due to any hydraulic law so much as to attempting to provide an equation which should fit errors of observation.

As to the experiments on Jets, presented by Mr. Kuichling, with the statements that he regarded them only as somewhat roughly approximate, I have thought it worth while to compare them with the jet formula of page 392 (using $d_1 = 2$ for jets more than 2 inches in diameter) and obtain the following results:

Diameter of orifice of Ring Nozzle. Inches.	Gallons per minute discharged, allowing 5 per cent for slip.	Observed head at water main. Pounds.	Co-efficient of discharge. Supposing full observed head utilized in producing discharge.	Actual diameter of Jet. d_1.	Height of Jet by formula. Feet.	Observed height. Feet.	Error. Feet.
2	1 154	160	.76	1.74	263	210	— 53
3	2 639	170	.75	2.61	289	286	— 3
4	4 692	165	.77	3.48	283	294	— 11
5	6 140	120	.75	4.35	225	257	+ 22

In the first experiment, though description is vague, probably the discrepancy is mainly due to friction loss in the three lines of hose leading to Siamese. In the last three experiments the fact that height of solid water rather than extreme drops was observed tends to offset friction loss between gauge and nozzle, but with such large jets the extreme drops do not rise much higher than the main body of water.

The co-efficient of discharge found (0.76) is about what would be expected from a ring nozzle where the ring was of an ordinary thickness, namely, one-twentieth part of diameter of nozzle (see Fig. 46, page 335). However, since co-efficient of contraction, force of wind, loss of pressure by friction between gauge and nozzle, and elevation of gauge relatively to orifice are not known, we must, as Mr. Kuichling implies, not attach too much weight to these experiments, but can regard them, I think, as confirming, and in no wise contradicting, the formula presented in the paper.

G. F. Swain, Assoc. Am. Soc. C. E.—The paper by Mr. Freeman seems to me to be a very important contribution to the science of hydraulics, particularly as it discusses with great completeness a branch of the subject regarding which experimental data have hitherto been very meagre.

I have been particularly struck with the care that has been taken to state clearly in every case the probable limits of error of the observations. I think this is a matter deserving of imitation, as it is not uncommon for experimenters to deceive themselves as well as their readers by attributing to the results of experiments an accuracy not warranted by the experiments themselves.

The experiments on the discharge of nozzles are very interesting to me, as showing that the co-efficient of discharge and range of jet are practically independent of the particular shape of the surface, but that they will be nearly the same for any smooth, well-made nozzle, either conical or with any curved shape in which the curve is gradual and easy, and such as to avoid as far as possible any losses of head; thus confirming the experimental results which have been found in the testing of turbine wheels, in which the precise shape of the buckets is found to be practically of small importance, as is observed by Mr. Freeman. With a given head at base of play-pipe, the only thing that can affect the co-efficient of discharge is the loss of head between the piezometer and the nozzle orifice, and it is not at once evident just how any difference in the form of the curve will affect this loss. As the author gives no formulas, it may not be out of place to give here the expression for the loss of head between the piezometer and the nozzle orifice, in terms of the co-efficient of discharge.

If v is the mean velocity through the nozzle orifice, v_0 that past the piezometer orifices, A and A_0 the areas of the current at these two points, Θ the loss of head between these points, m the co-efficient of discharge, and h the head of the piezometer, above the level of the nozzle orifice, then we have, as is well known,

$$v = \sqrt{\frac{2g\ (h-\Theta)}{1-\left(\frac{A}{A_0}\right)^2}}$$

The actual discharge is, therefore,

$$Q = Av = A\sqrt{\frac{2g\ (h-\Theta)}{1-\left(\frac{A}{A_0}\right)^2}}$$

What is termed the "theoretical discharge," or that in which Θ is neglected is,

$$Q_1 = A\sqrt{\frac{2\,gh}{1-\left(\frac{A}{A_0}\right)^2}}$$

$$\text{Hence} \quad m = \frac{Q}{Q_1} = \sqrt{1 - \frac{\Theta}{h}}$$

$$\text{or} \quad \Theta = h\,(1 - m^2)$$

Theoretically, therefore, the value of m should vary with the same nozzle unless $\frac{\Theta}{h}$ is constant. It might be of some interest to discuss from these observations the relations between m, Θ and h, for the different nozzles used, but it will not be here attempted. It may be noticed, however, that the data on pages 328 and 329 show that for the nozzle B, for instance (as well as for others), the loss of head Θ is less than for any of the "uniform accelleration" nozzles, since for B the value of m is greater, even for the same or for smaller values of h.

The result arrived at by Mr. Freeman, that extreme accuracy may be obtained in the measurement of water by means of nozzles, strikes me as of great practical importance, and one that should bear rich fruit. If the accuracy obtained by the use of a nozzle is as great as that which can be obtained by the use of a weir, there can be no question that the former method is in very many cases preferable, and it certainly seems, as Mr. Freeman says, to offer an easy means of testing, as often as may be desired, and with very little trouble, the performance of a pumping engine. It is to be hoped that Mr. Freeman may be able to suggest convenient means by which this method of measurement may be easily and accurately used in the case of pumping engines of large capacity.

The experiments of Mr. Freeman upon flow of water through fire hose are of much interest to me, and the ingenious expedient of taking a plaster cast of the interior of the hose is the first attempt that I can now recall to give a precise idea of the degree of roughness in a pipe. It is a matter of some speculative interest, how far it is the degree of roughness alone which affects the flow of water in pipes, and how far, if at all, it is anything in the nature of the material itself, which may affect the flow by influencing the adhesion, or in some other way.

The result of the experiments on curves, namely, that the loss of head was greater with large than with small radii, is anomalous, and a larger number of experiments than were here made would be necessary to establish it conclusively. A careful study of these experiments, which I have not yet had time to make, might indeed suggest some explanation of the anomaly.

The experiments on jets appear to me to be without doubt the best that have ever been made, in this or any other country; and an especially valuable point, it seems to me, is that the distinction is here made, for the first time, between the range of the extreme drops, and the range as a "good fire stream," uncertain though the latter is. The "jet diagram" (Fig. 63) is a very compact and ingenions means of rep-

resenting the results of the experiments, and it is only to be regretted that more data were not obtained, in order that the curves shown might be more accurately defined. As it is, many of them appear to rest on three observations simply. The experiments on the distribution of velocity in jets are of great interest, and the agreement between the theoretical velocity and the measured velocity in the center of the jet appears very close. I may venture, however, to call attention to the fact that there are two circumstances which may alter, perhaps materially, the figures in the column of differences on page 414. One is that in correcting for velocity past piezometer the actual discharge has been used instead of the theoretical, as should strictly be the case; the "theoretical velocity" being thereby decreased, although by a very small amount—an amount, however, which would probably be material compared with the small values in the column of differences. The other is that the co-efficient of the Pitot tube has been assumed as unity, whereas it has commonly been supposed to be somewhat less. There does not appear to be any theoretical reason, however, why there should be any perceptible or considerable loss of head in the center of the jet, within the distance considered, at least with a form of nozzle which would not cause internal disturbances, or throw toward the center the slower-moving water near the edges of the tube; and these experiments (especially the extraordinarily close agreement between the measured and computed co-efficients of discharge, on page 418) may be looked upon, as Mr. Freeman says, as proving very satisfactorily that the co-efficient of the instrument was unity, or very near it. The table on page 414 shows a greater difference between the theoretical velocity and actual velocity in center of jet in the case of the uniform acceleration nozzle than for any other, thus indicating a greater loss of head, and corresponding to the result on page 472 of this discussion. This may perhaps be due to the fact that the shape of the nozzle is such as to tend to throw toward the center the more slowly-moving water near the periphery. The same thing might be considered to be indicated by the form of the curve of distribution of velocity for this nozzle.

Altogether, I think it would be difficult to overrate the importance and interest of the experiments described in this paper.

Mr. Freeman.—With reference to the point raised by Professor Swain that a larger number of experiments is needed to establish that the loss of head due curves of a given total angle of curvature was greater for large radii than for small, I would say that I think there is almost no doubt whatever but that this was a fact within the limits of these experiments; but yet, of course, I believe that as radius of curve became smaller than ten times diameter of pipe, a point would soon be reached when loss would increase sharply with a further diminution of radius of curve. The inexpansible and excellent hose used was much

better than any metallic pipe for the purpose of this delicate measurement, as the surface for the different radii was identical and the change from one radius to another was made with water flowing under full pressure, and required but a very short time.

When I at first obtained the results referred to I could not credit them myself, so about two weeks later I rearranged my apparatus, taking all precautions that could be devised to reduce errors of observation to a minimum. These experiments (see page 363, September 24th) told the same story as before; and though, after considering the possible limits of error at every step, I could see no reason to doubt the figures, yet in the effort to remove all doubts I tried another set of experiments on the next day with still a different kind of hose, and for the third time found that the loss in this 2½-inch pipe was greater with 4 feet radius than with 3 feet, and greater with 3 feet radius than with 2 feet. The loss even for the 2-foot radius is extremely small even with the velocity of 20 feet per second, and this experiment is mainly of theoretical interest in illustrating that above a certain radius the length of a curve becomes a more potent element than the sharpness of the deflection.

I am glad Professor Swain called attention to that question of speculative interest—how far it is the degree of roughness alone which affects the flow, and how far the material of the pipe by its adhesion, etc.—for it seems to me these experiments indicate that it is almost wholly a matter of the roughness of the material.

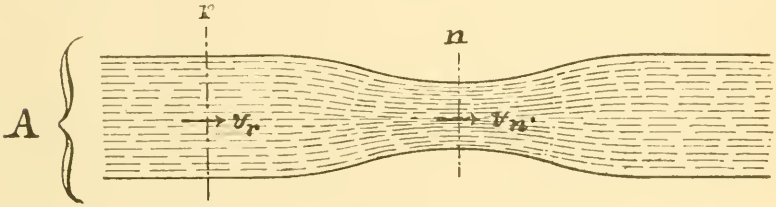
The experiments show a great range of co-efficients of flow with one material—India rubber—in various degrees of roughness, and the smoothest of these surfaces gives results which, as I now recall the comparison, do not differ very materially from those found with pipes of glass and lead, so far as may be judged from the few experiments on record for anywhere near this same diameter and velocity. If there were on record some experiments on pipes of lead and glass of 2½-inch diameter, with velocities of about 15 feet per second, there would be great interest in comparing the results.

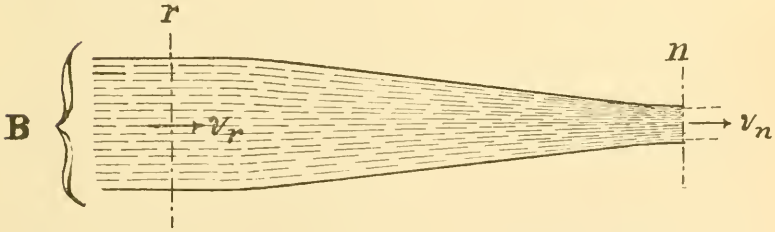
Professor I. P. Church.—It is with considerable diffidence that I venture to send anything in the way of criticism on the Fire Stream paper, from the necessary haste with which I have looked it over. I could not fail to be impressed, however, with the care and thoroughness observed in the conducting of the experiments, and the pains taken to make a definite showing of the degree of roughness of the interior surface of the hose. The Pitot tube results are extremely interesting, and I hope may be succeeded by others in which the form and size of the point receiving the impact will be varied in different ways. Taken all in all, this series of experiments certainly constitutes a very important contribution to

hydraulic science, to say nothing of its practical value to those interested in the actual use of fire streams.

A theoretical point occurs to me, as it did also in perusing the results of Mr. Herschel's paper on the Venturi water-meter,* connected with the computation of the theoretical jet velocity due to pressure at base of play-pipe. It may seem hypercritical to mention it, as its influence on Mr. Freeman's co-efficients for nozzles would be hardly appreciable, but, if I remember rightly, some of the values in one of Mr. Herschel's columns would be sensibly affected.

In the following figures let p and p_n be the observed piezometer pressures at the positions r and n, γ the weight of a unit of volume of water, and b the height of the water barometer. A represents a longitudinal section of the Venturi meter and B that of a play-pipe and nozzle. Of course in B the piezometer pressure at n is zero.





v_r and v_n denoting the velocities at the two positions (at the same level), the principle of Bernoulli tells us that with no source of resistance between r and n we have

$$\frac{p_r}{\gamma}+b+\frac{v_r^2}{2g}=\frac{p_n}{\gamma}+b+\frac{v_n^2}{2g} \text{ for } A; \text{ and } \frac{p_r}{\gamma}+b+\frac{v_r^2}{2g}=0+b+\frac{v_n^2}{2g} \text{ for } B \qquad (1)$$

* Transactions, Vol. XVII, p. 228, November, 1887.

It is required to compute the theoretical value (*i. e.*, the value if there were no loss of head between r and n) of v_n, from the observed pressures. Equation (1) contains not only v_n but v_r. But v_r is connected with v_n by the relation $F_r\, v_r = F_n\, v_n$, (F_n and F_r are sectional areas).

Hence, putting $\frac{F_n}{F_r}\, v_n$ for v_r, we have

$$\left.\begin{aligned} v_n &= \sqrt{\frac{1}{1-\left(\frac{F_n}{F_r}\right)^2}}\sqrt{2g\left[\frac{p_r}{\gamma}-\frac{p_n}{\gamma}\right]} \text{ for } A; \\ \text{and, for } B,\ v_n &= \sqrt{\frac{1}{1-\left(\frac{F_n}{F_r}\right)^2}}\sqrt{2g\,\frac{p_r}{\gamma}} \end{aligned}\right\} \quad (2)$$

It seems to me that a value of v_n thus computed—by equation (2)—is more strictly deserving of the name "*theoretical velocity*" than one based on the relation (used, I think, both by Mr. Freeman and Mr. Herschel):

$$v_n = \sqrt{2g\left[\frac{p_r}{\gamma}+\frac{v_r^{1\,2}}{2g}-\frac{p_n}{\gamma}\right]} \text{ for } A; \text{ and } v_n = \sqrt{2g\left[\frac{p_r}{\gamma}+\frac{v_r^{1\,2}}{2g}\right]} \text{ for } B \quad (3)$$

where v_r^1 is the actual or experimental value of the velocity through section r. For, if v_n^1 = the measured or actual velocity of the jet $= \frac{F_r\, v_r^1}{F_n}$, it follows from (3) that

$$\left.\begin{aligned} v_n &= \sqrt{2g\left[\frac{p_r}{\gamma}+\left(\frac{F_n}{F_r}\right)^2\frac{v_n^{1\,2}}{2g}-\frac{p_n}{\gamma}\right]} \text{ for } A; \\ \text{and } v_n &= \sqrt{2g\left[\frac{p_r}{\gamma}+\left(\frac{F_n}{F_r}\right)^2\frac{v_n^{1\,2}}{2g}\right]} \text{ for } B. \end{aligned}\right\} \quad (4)$$

In other words we are using the measured value of v_n to compute the "theoretical value!"

The point, therefore, is this, it seems to me: Theory is abundantly competent to find the "theoretical" v_n with no measurements save those of pressure and sectional areas. Does it not, therefore, savor somewhat of "begging the question" to employ in its computation the measured value v_n^1 or a quantity having a fixed relation to that measured value and obtained from it?

If it be then represented that the play-pipe takes the velocity as transmitted to it from the adjacent up-stream parts, it must be remembered that the velocity at any section of a pipe in which a steady flow is proceeding depends on all the features of the whole line of pipe from reservoir to, and including, the nozzle, so that a change in the resistance-character of a single part affects the velocities in all.

In the case of a nozzle or Venturi meter of such design as to cause

considerable loss of head between r and n, it seems clear that the value of the co-efficient $\frac{F_n\, v_n^1}{F_n\, v_n}$, or ratio of the actual to the theoretical discharge, with v_n computed from equation (2), would be quite different from that based on v_n as obtained from (3) or (4). In case, then, this co-efficient is to be taken as a measure of the efficiency of the apparatus used, it would seem that the former procedure is to be preferred (2).

As before remarked, this criticism may seem quite out of place as applying to Mr. Freeman's nozzles, most of which offered so little loss of head. It is made simply to bring up the question as to what should properly be termed the "theoretical velocity" of the jet, at n, in such a case.

Mr. Freeman.—Professor Church's point appears well taken, but, as he intimates, the error actually introduced is practically inappreciable. It will be noted, however, that in formula 1 of the appendix the expression is given in its true form.

Edmund B. Weston, M. Am. Soc. C. E.—I have been very much pleased and gratified by reading Mr. Freeman's instructive and valuable paper.

The value of n in the expression $v = n\,(r\,s)^{\frac{1}{2}}$, given in Table No. 7 of this paper, is, as deduced from my general formula * 116.4, which is almost exactly equal to the average of the results given in the same table that were obtained with hose designated by the letters D and E.

Then I find that the average of the co-efficients of discharge given in Table No. 1, which were determined from experiments made with smooth nozzles $\frac{3}{4}$, $\frac{7}{8}$, 1, $1\frac{1}{8}$, $1\frac{1}{4}$ and 1.55 inches in diameter, is 0.976, while the average of those deduced by myself from experiments made with smooth nozzles $\frac{7}{8}$, 1 and $1\frac{1}{8}$ inches in diameter,* is 0.971; also that the average of the co-efficients of discharge given in Table No. 3, which were determined from experiments made with square ring nozzles $1\frac{1}{8}$ and $1\frac{1}{4}$ inches in diameter, is 0.74, while the average of the results that I deduced from experiments made with square ring nozzles $\frac{7}{8}$, 1 and $1\frac{1}{8}$ inches in diameter *, is 0.721.

I have made a few comparisons, which are shown in the following table, of some discharges which I had at hand, that were calculated by my formula † for 1 inch smooth nozzles connected to $2\frac{1}{2}$ inch rubber hose of different lengths, with some of the discharges for 1 inch smooth nozzles connected to the best quality of rubber hose, given in Table A.

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* Transactions American Society of Civil Engineers (Vol. XIII, November, 1884).

† In order to obtain the hydrant pressure, to correspond to my discharges, I added to the h given in my formula, 1.5 times the theoretical head due to the velocity of the water flowing in the $2\frac{1}{2}$ inch gauge coupling, 1.5 being a factor that I deduced from experiments made with a Providence Water Works hydrant, a number of years since.

Hydrant Pressure.†		Length of Hose.	Gallons Discharge per Minute.		
Pounds.	Feet.	Feet.	Weston.	Freeman.	Per Cent.
40.	92.4	50	172.5	174.0	99
98.	231.4	50	267.5	274.0	98
50.	115.5	100	184.0	186.0	99
100.	231.0	100	261.0	263.0	99
43.	99.3	200	158.0	161.0	98
94.	217.1	200	233.0	237.0	98
46.	106.3	400	143.0	147.0	97
92.	212.5	400	202.0	208.0	97
30.	69.3	1 000	88.0	93.0	95
90.	207.9	1 000	152.0	161.0	95

† See foot note previous page.

As will be seen, the average of the results in this table computed by my formula is only 2.5 per cent. less than the average of those of Mr. Freeman.

I consider the close agreement between the results of Mr. Freeman's investigations and my own in all of the preceding comparisons that I have mentioned remarkable, especially as I used hose that had been some time in service, and nozzles that were selected from general stock that were not made by the same manufacturers as those used by Mr. Freeman.

I quite agree with Mr. Freeman's idea, that nozzles on some occasions can be more advantageously used than weirs for accurately measuring small quantities of water. My formulas have been very frequently used since 1877 in the Water Department of the City Engineer's office in Providence for this purpose.

Mr. Freeman, while giving me credit for accuracy, infers that if my general formulas had been more conveniently arranged they could have been more extensively used. The principal object that I had in view when constructing these formulas was accuracy, as I was familiar with one or two simple formulas for the flow of water through rubber hose and nozzles that I consider the reverse. An examination of my formulas will show that they are arranged for obtaining direct results in cases where nozzles are attached to $2\frac{1}{2}$-inch hose, or when the "open butts" consist either of $2\frac{1}{4}$ or $2\frac{1}{2}$-inch couplings, the pressure being obtained in the usual manner by connecting a pressure gauge to the hose near the hydrant or fire engine.

When I presented the results of my researches to this Society in 1884, I simply did so in order to give engineers, or others if they chose to make use of the formulas, an opportunity for obtaining results, that, in my opinion, could be relied upon. I should have been very glad, at the same time, to have been able to have furnished, in addition to formulas, practical tables, but as there were other things of much more

importance that required my attention I felt that I could not devote any more time to the subject.

R. Hering, M. Am. Soc. C. E.—I should like to call attention to some facts presented in the paper of Mr. John R. Freeman on Fire Streams. This shows a thoroughness in getting and arranging results for which the profession will feel greatly indebted, the merit of Mr. Freeman's work being enhanced by the fact that the experiments, conducted in a very careful manner, are an original contribution to the knowledge on fire streams, and one which I think will put the subject on a scientific basis.

While Mr. Freeman felt tempted to devise a formula from the results gained, he did not do so, preferring for the present to substitute tables and curves, partly because, as he well states, they are more convenient for use and less likely to be used far beyond the limits of the experiments from which they were derived.

While much could be said regarding the merits of the paper, I will confine myself to the subject of the effect of the variation of the degree of smoothness in small pipes, as Mr. Freeman's experiments give a valuable contribution to this subject. On page 358 he records the hydraulic elements for each experiment. By letters *A* to *L* he indicates the different kinds of hose used, and in Fig. 47 shows their degree of roughness by photographs of plaster casts.

I have taken the co-efficients of roughness as found by the Ganguillet & Kutter formula from their diagram, and found them to be as follows: (See table on next page.)

Any one comparing the co-efficients in the last column with the photographs of the respective hose will have a clear exhibition of the influence of roughness upon the velocity and consequently upon the pressure necessary to produce it, and also of the numerical value for the co-efficient corresponding to each degree of roughness illustrated.

I think it will require no further argument than a glance at the following Plate (Fig. 65), which is slightly re-arranged from that of Mr. Freeman, to convince any one that a formula for the flow of water in pipes should contain a variable co-efficient for the roughness of the perimeter, and at the same time to convince him of the usefulness of the Kutter formula in this respect, even granting its having a number of defects.

From the illustration it might appear that the fourth sample was rougher than the fifth, yet it gives a smaller co-efficient. This is only apparent, however, because the former has a surface of rubber while the latter has one of unlined linen with its unyielding cords and fibers to retard the water.

Bazin's experiments in a small trough lined with canvas give a higher co-efficient of resistance than in the above case, namely, .0134 to .0166

Sample or kind of hose.	Diameter of hose. Inches.	Square root of mean radius. r.	Sine of slope. s.	Mean velocity in hose. Feet per second.	Co-efficient in $v=c\sqrt{rs}$.	Kutter's co-efficient of roughness.
A	2.65	.235	.1863	12.50	123.3	.0073
	"	"	.2311	13.96	123.6	.0073
	"	"	.3404	17.00	124.0	.0073
	"	"	.4714	20.00	124.0	.0073
C	2.47	.227	.2464	13.40	119.1	.0075
	"	"	.3467	16.07	120.4	.0075
	"	"	.5269	20.00	121.5	.0074
D	2.49	.228	.2427	13.20	117.7	.0076
	"	"	.3351	15.81	120 0	.0075
	"	"	4733	19.00	121.3	.0074
	"	"	.5708	21.00	122.1	.0074
E	2.68	.236	.0809	7.50	111.6	.0080
	"	"	.1440	10.00	111.6	.0080
	"	"	.2611	13.65	112.3	.0079
	"	"	.3931	17.00	114.8	.0078
I	2.69	.237	.2357	11.50	100.1	.0085
	"	"	.3120	13.55	101.7	.0084
	"	"	.4382	16 00	102.2	.0084
	"	"	.5165	18.00	105.8	.0083
M	2.12	.210	.3448	14.00	113.4	.0076
	"	"	.4897	17.00	114.8	.0075
	"	"	.7673	21.81	118.4	.0074
K	2.53	.230	.0261	3.50	94.3	.0088
	"	"	.0582	5.00	90.3	.0091
	"	"	.1384	7.50	87.8	.0093
	"	"	.2450	10.00	88.0	.0093
	"	"	.3425	12.00	89.3	.0092
	"	"	.5570	15.31	89.3	.0092
	"	"	.6748	17.00	90.1	.0091
	"	"	.8264	19.00	91.0	.0090
L	2.60	.233	.0414	3.50	73.9	.0102
	"	"	.0839	5.00	74.1	.0102
	"	"	.1846	7.50	74.9	.0102
	"	"	.3120	10.00	76.9	.0101
	"	"	.6286	14.50	78.5	.0100
	"	"	.8558	17.00	78.9	.0100
	"	"	1.1624	20.00	79.6	.0100

instead of .0100. This difference is probably due partly to the much higher velocity in the latter case, which will be appreciated by observing the variation of the co-efficient of roughness with the slope, and partly to the imperfections still remaining in the Kutter formula, as already mentioned.

Hamilton Smith's experiments in glass pipes give co-efficients of roughness of .0068 to .0073, *i. e.*, less than the smooth rubber-lined hose of Mr. Freeman's sample *C*, as might be expected. The numerous experiments with cast-iron pipes under higher velocity show co-efficients corresponding to Mr. Freeman's samples *I*, *K* and *L*.

I notice that these gaugings give a very satisfactory confirmation of a conclusion I reached some years ago, namely, that Kutter's co-efficient of roughness decreases as the slope or velocity increases, if the pipes

Fig. 65.

PHOTOGRAPHS FROM PLASTER CASTS OF INTERIOR OF FIRE HOSE.

(As per Fig. 47 in paper of Mr. John K. Freeman.)

Together with a description of the character of roughness of interior surface, and the numerical value of the Coefficient of roughness in the Ganguillet and Kutter formula for the mean velocity of water.

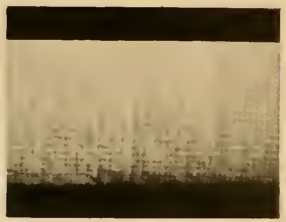
(Discussion by Rudolph Hering, M. Am. Soc. C. E.)

.0074 to .0075 Coeff. of roughness.

SAMPLE C. **WOVEN COTTON, RUBBER-LINED HOSE.**
(Average depth of corrugations, $\frac{1}{1000}$ to $\frac{1}{1000}$ inch.)
14.1 lbs. pressure lost by friction per 100 feet.
Rubber lining slightly thicker than in sample K, and interstices between threads next lining well filled with rubber, so inner surface remained smooth under pressure.

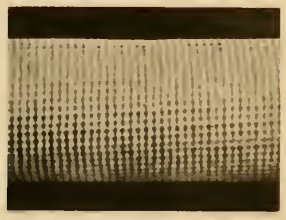
.0074 to .0076 Coeff. of roughness.

SAMPLE D had same kind of rubber lining, and gave loss of 14.2 lbs. per 100 feet.



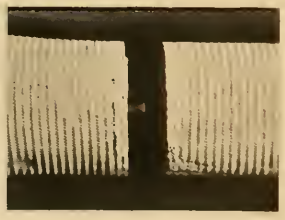
.0078 to .0080 Coeff. of roughness.

SAMPLE E. **KNIT COTTON, RUBBER-LINED HOSE.**
Interstices between threads next lining well filled.
16.0 lbs. pressure lost by friction per 100 feet in length (or 13 per cent more friction loss than in sample C.)



.0083 to .0085 Coeff. of roughness.

SAMPLE I. **KNIT COTTON, RUBBER-LINED HOSE.**
Interstices between threads next lining not well filled.
Average height of roundabout ridges, about $\frac{1}{100}$ inch.
Average height of ridges which run lengthwise, about $\frac{1}{1000}$ inch.
19.4 lbs. pressure lost by friction per 100 feet in length (or 37 per cent more friction loss than in sample C).



.0088 to .0093 Coeff. of roughness.

SAMPLE K. **WOVEN COTTON, RUBBER-LINED "MILL HOSE."**
(Average depth of corrugations, $\frac{1}{1000}$ to $\frac{2}{100}$ inch.)
25.5 lbs. pressure lost by friction per 100 feet in length of hose (or 80 per cent more loss than in sample D or C, shown below).
Rubber lining was rather thin, and crevices between threads next lining not being well filled, the water-pressure caused inner surface to conform to threads of fabric.

.0100 to .0102 Coeff. of roughness.

SAMPLE L. **UNLINED LINEN HOSE.**
33.2 lbs. pressure lost by friction per 100 feet in length (or 2¼ times as much loss as in sample C, shown below).

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are fairly straight, regular and smooth. A revision of Kutter's formula should correct this feature, as the co-efficient of roughness ought to remain the same whatever the velocity is.

Mr. Freeman.—My thanks are due to Mr. Hering for his kind interest as shown by the care which he has taken to compare these experiments with that equation of most remarkable range of application, the Kutter formula.

Most earnestly do I concur in his conclusion that a satisfactory "formula for the flow of water in pipes should contain a variable co-efficient for the roughness of the perimeter;" but while the Kutter formula is useful in this respect as a formula for pipes, and inasmuch as it recognizes this principle is a long step in the right direction, yet I trust and believe that we shall before many years have a much simpler formula, designed for pipes alone, in which the same principle shall be incorporated.

As to the means by which such small roughnesses of surface are able to induce such powerful effects upon the friction loss, may we not consider that, instead of being due mainly to friction of the water upon the pipe, they are most likely due to the ability of these small projections to set the whole stream to its very center into a condition of turmoil and minute eddies, just as a single stone on the bottom of a smooth and deep canal is sometimes seen to produce a "boiling" at the upper surface?

Future experiment must determine the law governing the influence of projections of certain size, shape and arrangement upon the flow in pipes of various size, before this "satisfactory general formula" for flow in pipes can be derived. Other experimenters must give us for larger pipes of iron plastic representations of the exact degree of roughness corresponding to a given co-efficient of flow, or something analogous to the photographs which I have presented in Fig. 47, as pertaining to the more humble subject of fire hose, and then the means for properly designating the "variable co-efficient of roughness" in a general formula for flow in pipes will be at hand.

But yet that the friction loss does vary in relation to the velocity by a law somewhat different from the simple square is shown by the inclination of the plotted lines representing the values of n in Fig. 49, page 356. These variations are so slight, however, that it is clearly shown they may be disregarded in practical computations.

In reference to the demonstration that ring-nozzles have no special virtues, to which Mr. Kuichling refers with approval, I may mention that since, as he also suggests, so many men firmly believe in their merits, I took care to have some competent witnesses to this test, and so one day when Mr. James B. Francis, Mr. Clemens Herschel, Mr. E. B. Weston and fifteen or twenty other gentlemen skilled in these matters gave me the honor of their presence at a demonstration of the main

features of the experiments, we repeated the jet experiments as shown by Figs. 54 and 55, and although the two streams followed each other very closely, yet I am very sure that no one present would claim that either the square ring, under-cut ring or uniform acceleration-nozzles were in any degree better than the plain cone.

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ON THE FRESH WATER ALGÆ AND THEIR RELATION TO THE PURITY OF PUBLIC WATER SUPPLIES.

By GEORGE W. RAFTER, M. Am. Soc. C. E.

WITH DISCUSSION.

It has been my pleasure to devote the most of my leisure for the last few years to studying questions relating to the preservation and improvement of public water supplies. During this time I have pursued, in a desultory way, some lines of investigation which, I hope, may be of interest to the members of the Society, and which I venture at this time to briefly lay before you.

Knowledge of the sanitary significance of many of the minute forms of plant life inhabiting our water supplies is very limited, and it is only when some special reason exists that students direct their attention to the sanitary side of the question. This is the more remarkable when we consider the amount of energy that has been expended in study from the purely botanical point of view.

Take, for instance, the fresh water algæ, to which the present paper is particularly devoted. Including the desmids, but not the diatoms,

about fifteen hundred species have been illustrated and described as peculiar to this country, while in Europe, where more complete studies have been made, a much larger number are known to cryptogamic botanists. Moreover, this number is being added to from year to year, so that he who studies the fresh water algæ can hardly fail to have new species to determine, whenever he sees fit to go forth into the fields and search diligently for them.

Much misconception has existed as to the cause of the various bad tastes and odors, known as "fishy," "pig-pen," "cucumber," "musty," "woody," and others, with which different water supplies have been affected. The impression is quite general, not only that the plants producing these different tastes and odors are limited in number, but that the grass-green varieties of algæ are entirely innocent. Both views are undoubtedly erroneous, as it is my experience that very many of the algæ will in an experimental way produce objectionable tastes and odors, and that some of these are especially virulent in the case of certain of the grass-green varieties. Both of these points have been tested by experimenting on at least two hundred species, representing not only my own collections for three years, but a large number of samples received from correspondents.

Before proceeding to discuss some of the more interesting results of the study, I will call attention to a few peculiarities of the fresh water algæ in relation to habitat.

The algæ are in the fullest sense hardy plants, many of them growing vigorously in the middle of winter. This I have had occasion to notice, particularly during the past winter, when a form resembling *Microcoleus*,* an alga genetically related to *Nostoc*, has been exceedingly plentiful in the domestic water supply of the city of Rochester, without, however, so far as known, being the cause of any trouble. This development was at its height in the latter part of the winter (February 20th), when Hemlock lake was covered with an unbroken sheet of ice from twelve to fifteen inches in thickness.

This abundant vitality was even more markedly forced upon my attention about March 1st of the present season (1889), while doing field work in Livingston county, N. Y., where I observed the ground not only partly covered with melting snow, but, for a considerable area, with a vigorously germinating alga. In the absence of a microscope its species

* Considered to be the agglutinated form of *anabæna*.

could not be determined, and I am able therefore only to record the fact of having seen, while the snow still largely covered the ground, the visible signs of such a development.

The observation had to me greater interest than would usually attach, from the fact that only a few days before seeing it the temperature had fallen several degrees (8° to 12°) below the zero of the Fahrenheit scale.

It has been well known for some time that numerous species of algæ flourish in the arctic regions of perpetual snow, and it is of interest in this connection to cite that a study a few years since of the algæ of Nova Zembla shows the existence of one hundred and seventy-two fresh water species belonging to fifty-seven genera.*

Chlamydococcus pluvialis, the well-known red snow† of the arctic region, has an extended range of habitat, being found plentifully in even New York and Pennsylvania. It must be admitted, therefore, that low temperature does not necessarily imply cessation of growth of all of the algæ, and we must extend our limit of vision in this direction also. Indeed, high temperature will undoubtedly be as fatal to some species as low temperature would be to those which are peculiar to thermal springs.

While it is thus true that many species are hardy, it is equally sure that some of them are at certain stages of development extremely sensitive to changes in the environment—a fall of temperature of a few degrees, a slight change in the direction of the wind, humidity of the atmosphere, or variations in the chemical constituents of the waters they inhabit, frequently sufficing to cause their immediate disappearance from localities the most favorite.

Again, certain species only live in waters containing some particular substance in solution, as, for instance, *Beggiatoa* in waters containing sulphur in some form, *Batrachospermum moniliforme* in rapid running limestone waters, while some species of *Inactis* are at home only in sandstone waters. *Cladophora* and *Vaucheria* are primarily marine forms, and it is doubtful if the fresh water varieties will continue to exist in a water absolutely free of sodium chloride in solution.

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* Jour. Roy. Micr. Soc., 1880, page 679.

† Cooke's British Fresh Water Algæ and Wolle's Fresh Water Algæ of the United States contain full description. Both agree that *C. pluvialis* and *C. nivalis* are probably one and the same alga.

Others affect sluggish streams and stagnant ponds, some modestly hiding under shaded banks and beneath water-logged trees, while others again prefer the full light of noonday, and boldly seek open spaces in the purest waters. They are found at all depths, from nothing to the bottom of the deepest lakes. In the somewhat crude experiments which I have made I have endeavored to reproduce, so far as I could, in my workroom, with an assortment of bottles of various sizes, beaker glasses and fruit jars, the natural conditions of growth of each species. In many cases I have had only failure as the result; they utterly refused to grow, but in their death I gained information as to the odor-producing capacity to be gained in no other manner.

Pursuing the line above indicated relative to the grass-green varieties, it may be generally stated that the recent studies indicate the ability which these minute cryptogams possess of causing various bad tastes and odors to be entirely independent of color. This is emphasized by considering that *Beggiatoa* and *Crenothrix* are in effect colorless, *Cladophora* and *Volvox* grass-green, while *Clathrocystis* and *Nostoc* are bluish green. It seems equally certain that, so far as present systems of classification are concerned, we cannot refer the power to produce these difficulties to any particular class or order of cryptogams. We must look farther for the final cause. This is usually found in the jelly, starch, oil or sulphur-producing capacity of these plants, and generally the question of whether or not any particular species is likely, under favorable conditions, to produce trouble, will be settled by observing whether it develops at any stage of its life history an albuminous gelatine, either as an essential part of cell structure, or as an enveloping matrix, or whether it secretes starch, oil or sulphur at some period of its existence. An observation as to which of these is the predominant characteristic of any given alga will also indicate the probable quality of any tastes or odors of which it may be the cause. The intensity of the resulting tastes and odors will also be indicated by the vigor of development and the quantity present in any given case.

So far as I am aware, no one has yet advanced a theory accounting fully for these various bad tastes and odors, and it is with considerable diffidence that I venture to offer the foregoing, which can only be called a working theory, subject, I have no doubt, to modification, as more complete studies are made of this interesting and very practical question.

It is well known that certain algæ contain starch grains in more or less quantities in connection with the chlorophyl granules.

Cladophora in particular is distinguished by the possession of a large number of such grains.* The presence of these starch grains may be demonstrated by the application of the iodine test,† though in many cases, when one knows just what to look for, the starch can be distinguished without the application of any special tests.‡

About two years ago I found in one of my gatherings a specimen of *Cladophora* containing an unusually large amount of starch throughout the chlorophyl, so much so that I began to make an extended study of the specimen. An interruption of a few days occurred, and when I resumed the study I found the specimen well advanced in decay and producing a nearly unbearable stench. I thereupon experimented with a solution of starch and water, and found it to give off, after standing for a few days, a quite similar stench.‖ With this as a basis I began to gather all the species of algæ containing starch that I could find, and by experiment under various conditions endeavored to ascertain whether any other species containing starch gave objectionable odors on decay, and, while I have by no means completed the study, the result thus far seems to justify saying, in a general way, that all the chlorophylaceous algæ, which contain either starch grains or amylaceous granules in the chlorophyl, are likely, if they develop in quantity, to produce at some stage an unpleasant odor in decay.

Again, many of the algæ secrete globules of oil in connection with the chlorophyl. *Vaucheria* in particular, with its green, robust filaments, has its chlorophyl rather evenly distributed on the inside of the walls of the tubes, with green granules and minute oil drops imbedded.§ *Vaucheria sessilis* has this development of oil drops especially well marked, though this alga will not usually be the cause of trouble with water

* See description of *Cladophora* in Wolle, *loc. cit.*

† For detail of iodine test for starch in chlorophyl grains, see Behrens' Guide to the Microscope in Botany, Hervey's translation, page 364.

‡ Two forms of grain or granule may be distinguished, the starch grain pure and simple, and the amylaceous granule, which resembles starch, and which may or may not actually become starch, depending upon phases of development. The following is given as a partial list of fresh water algæ which exhibit one or the other of these forms: *Chætomorpha, Cladophora, Conferva, Cylindrocapsa, Microthamnion, Spirogyra, Ulothrix, Vaucheria, Volvox, Zygogonium, Zygnema.*

‖ The water from starch factories becomes particularly offensive. See paper by the late Professor William Ripley Nichols, Some Remarks on the Tastes and Odors of Surface Waters, in the Journal of the Association of Eng. Societies, for January, 1882.

§ So described by Mr. Wolle, *loc. cit.*

supplies, as its usual habitat is wet ground, except that it is occasionally found in ponds and ditches; but what is true of one species is likely to be true of others, and I cite *Vaucheria* in this connection because it is widely distributed and easily obtained for purposes of study. There are a number of species of *Vaucheria*, and one of these, *Vaucheria dichotoma*, is only found in marshy places in the vicinity of limestone springs.

The late Professor Nichols records a statement of Mr. Fteley* that the addition of common salt to water possessing the cucumber taste will sometimes develop a decided oily flavor. I have no doubt but that this observation is correct, and that some of the oil-secreting algæ are responsible for the so-called cucumber tastes, which possibly are modified by the addition of the common salt, while at the same time that of the oil is accented enough to bring it markedly within the range of the sense of taste.

There are, however, a large number of species of algæ in which the starch grain and oil globule are not the most striking features of development, and these are the species which are generally credited with causing all the trouble. A number of them belong in the class *Cyanophyceæ* and the order *Schizosporeæ*, including among such *Nostoc*, *Clathrocystis*, *Cœlosphærium*, *Lyngbya* and others. A number of the cryptogams of this class and order are inclosed in a maternal jelly, a typical form of which is well illustrated by *Nostoc*. We have, however, *Batrachospermum*, in which the jelly mass is an integral part of the development, in the class *Rhodophyceæ*, while among the class *Chlorophyceæ* and in the order *Confervoideæ* may be found *Draparnaldia* and *Chætophora*. Also in the order *Protococcoideæ* we find *Volvox*, *Eudorina*, *Pandorina*, *Hydrodictyon* and *Palmela*. All of these are likely by reason either of the jelly matrix or the gelatinous structure, to give rise, when they develop in quantity, to objectionable tastes and odors in water supplies.

In order to complete this portion of our working theory it is desirable to indicate the relations which subsist between the production of the jelly mass, as an integral part of the life history of an alga, and the processes of assimilation and nutrition. This will lead us to consider, briefly, not only the formation of the chlorophyl, but also the vegetable jelly or mucilage. The fundamental substance of chlorophyl is nitrogenous matter, and it is according to Sachs† a protoplasmic form.

* In Some Remarks on the Tastes and Odors of Surface Waters, *loc. cit.*

† Text Book of Botany

In the process of development there are stored up assimilated substances, called reserve material. This at the proper time is drawn upon, and through the process of nutrition finally becomes part chlorophyl, part cell membrane and part refuse matter, this refuse matter, which is rejected as waste material, taking the form of jelly.* Such in a few words is probably the origin of the vegetable mucilage or enveloping jelly of many of the algæ. In any case it is essentially a form of protoplasm, nitrogenous in its chemical constitution and liable in decay to produce offensive odors, as indeed it is well known to do from the observed facts.

A few of the algæ of the class under consideration, however, do not secrete any exterior mucilaginous or gelatinous matrix, as, for instance, *Volvox globator*, which has usually been considered a hollow sphere, but is now known to be exactly the contrary, the sphere being entirely filled with a comparatively solid mucilaginous jelly.† In this the young *Volvoces* develop very much as *Nostoc* develops in its enveloping matrix of albuminous gelatine.

Hydrodictyon utriculatum grows abundantly on the limestone rocks forming the bed of the Genesee river, in the city of Rochester. This stream has so low a summer flow that, occasionally, its bed between Main street and the Johnson and Seymour dam is nearly dry for several days at a time, by reason of the mill races at either side taking the entire flow of the river. On such occasions there frequently arises from the river bed an odor which is an offence to every one in the vicinity. On one occasion, when the odor was especially rank, I ventured into the river bed, and securing specimens of the abundant growth of *Hydrodictyon*, easily satisfied myself as to the important part which this alga played in its production. At this time the growth of *Hydrodictyon* was so abundant that several wagon loads could have been easily gathered from a few hundred feet of river bed. The common name of *Hydrodictyon* is "water net," by reason of its growing in a net-like series of reticulations, frequently to the length of from ten to twelve inches. I have never seen it in any waters except those containing considerable organic impurity.‡

* The basis of this view may be found in Sachs' Text Book of Botany, Chapter on Chemical Processes in the Plant. The view there advanced by Professor Sachs is a perfectly general one, applying quite as well to cryptogamous as to phenogamous plants.

† On *Volvox globator*. Is it a Hollow Sphere? Jour. Roy. Micr. Soc., 1883, page 889.

‡ See Wood's American Fresh Water Algæ for complete account of development of *Hydrodictyon*.

The ground taken by some of the earlier students of this subject, that the diatoms never cause trouble, must, I think, be modified in the light of recent experience. These little cryptogams, while possessing an incombustible, silicious frustule, have in addition not only a thick, gelatinous enveloping membrane, which, when they multiply in quantity, will undoubtedly produce the tastes and odors referred to, but they also produce starch in the chlorophyl. During last summer I made, on several occasions, gatherings, about the margins of Hemlock lake, the source of the domestic water supply of the city of Rochester, which, even when freshly gathered, gave a vile pig-pen odor, and which on examination with the microscope proved to be composed almost entirely of diatoms; and I have been informed by J. Nelson Tubbs, M. Am. Soc. C. E., Chief Engineer Rochester Water Works, that, on one occasion several years ago, a similar deposit was so extensive as to cause an unpleasant odor for several hundred feet from the lake. At this time the water itself was entirely unaffected, the odor proceeding, evidently, from the decay of the protoplasmic part of the diatoms scattered along the beach.

J. D. Hyatt has described an extraordinary development of the common diatom, *Meridion circulare.** This diatom has a thick, gelatinous envelope, and, early in the spring of 1881, developed to such an extent in the headwaters of the Croton river as to cover for many miles every submerged object in the streams to the depth of nearly a quarter of an inch. In about twenty days after its first appearance this growth began to break loose and in a week it had entirely disappeared, but following the disappearance of this sporadic growth of *Meridion* in the upper Croton, the water as delivered in New York began to be pervaded by an unpleasant odor, which it is fair to assume was due to the decomposition of the gelatinous envelope of *Meridion.*† This continued for about two weeks, and Mr. Hyatt states it to have been in all respects exactly similar to that of the odor from the decomposing specimens contained in his collecting bottles; and he further remarks that on experimenting it was quite surprising to observe how small a quantity of the fetid gelatinous matter was sufficient to taint a large volume of water.

* On Sporadic Growth of Certain Diatoms, by J. D. Hyatt, in the Transactions of the Ameri n Society Microscopists, 1882, page 197.

† .e trouble was so serious as to lead to the employment of experts to examine the Croton water. Such examination made from samples taken in the city resulted in a report that Croton water contained no unusual amount of organic matter, and nothing in any way deleterious to health.

So far as known the desmids have never been the cause of any trouble in public water supplies. This I apprehend is chiefly because they are less liable to sporadic growth than the diatoms and filamentous algæ, for they certainly possess characteristics which would lead to the production of tastes and odors if they developed in quantity. They are all more or less gelatinous and some species have a distinct, wide, colorless envelope.*

Crystals of sulphate of lime are found in the terminal vesicles of *Closterium* and other desmids, and the presence of these crystals suggests the agency of the desmids in the reduction of foreign matter in a manner similar to the production of oxalate of lime in *Spirogyra* and other species of filamentous algæ.†

A. Fteley, M. Am. Soc. C. E., records in the Supplement to the Report of the Massachusetts State Board of Health for 1879‡ the results of a number of observations on the development of algæ in some of the basins of the Boston Water Works. Among many other interesting facts he states that sulphureted hydrogen was present in basin No. 3 in such quantity as to be plainly perceptible to the sense of smell while the water was passing through the sluice gates. It was also observed, not only that algæ were numerous in the basin at this time, but that the temperature was uniform throughout the whole body of water, even to the depth of twenty feet. Mr. Fteley suggests that this uniformity of temperature was produced by chemical action, without, however, giving any clew as to the process by which the temperature was kept uniform. It appears, however, that the algæ were probably concerned in the matter in about the following way. It has been already mentioned that *Beggiatoa* only exists in waters containing sulphur in some form; and the further fact is now advanced that it and certain other algæ possess the power of decomposing sulphates in waters containing them in solution, a portion of the sulphur being absorbed in an uncrystallized state into the protoplasmic mass of the cell structure, while the balance goes to form sulphureted hydrogen as a free product. Waters containing either sulphates of lime or soda in solution are liable to this trouble.

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* Wolle's Desmids of the United States, page 15.

† On the Occurrence of Crystals of Gypsum in the Desmidieæ. Jour. Roy. Micr. Soc., 1884, page 108.

‡ Algæ Observed in Storage Basin No. 3 of the Boston Water Supply in 1879, by A. Fteley, C. E., Res. Eng.

A. Etard and L. Olivier have studied this question broadly,* and they conclude, in addition to *Beggiatoa*, which is nearly always found abundantly in sulphurous waters, that *Oscillaria*, in certain stages which they detail, and *Ulothrix*, present similar phenomena, and it is probable from some incomplete observations by the present writer that several other species possess this property in a more or less limited degree.

The observations of Etard and Olivier, however, extended only to the power of these algæ to decompose the mineral sulphates. Nevertheless, it is probable that *Beggiatoa* at any rate has the power not only of extracting sulphur from the mineral sulphates, but it can also extract it from decomposing organic matter, when containing it, so that the presence of sulphureted hydrogen in any given water supply may or may not be a danger signal, according to circumstances. It appears easy, therefore, to assent to Mr. Fteley's observation as to the nature of the action producing the same temperature throughout a large body of water twenty feet in depth.

The late Professor William Ripley Nichols explained the presence of sulphureted hydrogen in basin No. 3, referred to by Mr. Fteley, in a manner somewhat different from the foregoing.† According to Professor Nichols, the flooding of the basin started the decay of a large quantity of organic matter; this taking place in the presence of the sulphates contained in the water, changed them into sulphides, and from these sulphides thus formed sulphureted hydrogen was liberated by the acid products of decay. It appears to me, however, that if this is the whole state of the case, the chemical action must have been confined almost entirely to the sides and bottom of the basin, in consequence of which currents must have been produced, leading to varying temperatures in different parts. The fact, however, that the temperature was not only high, but uniform at all depths and localities, would indicate a distribution of the chemical action. The presence of large quantities of algæ, which in the absence of any evidence to the contrary we may assume as acting at least as assistants, would furnish a rational explanation without doing violence to any of the observed phenomena.

This property of decomposing the sulphates renders *Beggiatoa* of considerable interest to sanitarians, and although it has received a large

* On the Reduction of Sulphates by Algæ, in Jour. Roy. Micr. Soc., 1883, page 259, from *Comptes Rendus* XCV (1882), pages 816 to 849.

† Some Remarks on the Tastes and Odors of Surface Waters, *loc. cit.*

amount of attention,* its position in a systematic classification is not yet well defined. Mr. Wolle, writing as an algologist purely, is disposed to reject the whole genus, referring it rather to *Leptothrix*. The bacteriologists, however, retain the genus and place it among the *Schizophytes*. Cohn's classification, which is the best known, places *Leptothrix*, *Crenothrix*, *Beggiatoa*, *Hypheothrix* and *Oscillaria* in close relation in the second section of the *Schizophytes*, or among the *Nemutogenes*.† It is probable that *Beggiatoa* has bacillus, coccus and spirillum phases of development, and this, if true, would justify its classification with the bacterial forms.‡

It has been assumed by the promoters of ground water systems of water supply that the water so obtained would be entirely free from all the various troubles which, in a general way, may be said to be due to the presence of organized life; especially has it been claimed that such supplies could by no possibility contain the algæ, and, in consequence, immunity from the various tastes, odors and colors characterizing surface waters has been confidently predicted. This claim, however, is fallacious, as even ground waters are found to have difficulties of this sort also, and as certain ground water supplies of this country have recently passed through such trouble, it is considered that a general account of a similar trouble which has been very thoroughly studied abroad will be of interest in the present connection.

The trouble referred to was at Berlin, where, since 1877, a part of the supply has been drawn from a series of covered wells in the vicinity of the Tegel lake. The water is taken from the wells and pumped to covered reservoirs, from whence it passes into the city distribution without exposure to light or open air at any point of its passage. Soon after the introduction of this water it was found to be offensive, not only to taste and smell, but to the sense of sight as well. When first drawn from the wells it appeared tasteless and colorless, but, on standing, became tur-

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* See Fungi found in Sewage Effluents, paper by A. W. Bennett, in Trans. Am. Soc. Micr., 1884, page 90.

† For Cohn's Classification see The Bacteria, by Dr. George M. Strenberg.

‡ The following will give full reference to *Beggiatoa:*

(*a*) Micro-organisms and Disease, by E. Klein, 3d Ed. (1886), page 113.

(*b*) A. W. Bennett, *loc. cit.* Also note on Sewage Fungus, in Jour. Roy. Micr. Soc., 1884, page 927, from Proc. of Brit. Assoc. Adv. Sci. (Montreal), 1884.

(*c*) Manual of Bacteria, by Edgar M. Crookshank, 2d Ed. (1887), page 324.

(*d*) Lectures on Bacteria, by A. De Barry, 2d Ed., translated by Henry E. F. Garnsey. Numerous references.

(*e*) Wolle, Fresh Water Algæ of United States.

bid, and deposited a yellow sediment, consisting of oxide of iron, amorphous matter, and what on microscopical examination was found to be an alga with numerous specimens, both living and dead. This alga was *Crenothrix Kühniana* or *Crenothrix polyspora*, Cohn. Rabenhorst* gives it *Leptothrix Kühniana* and *Hypheothrix Kühniana.* Professor Kühn originally discovered *Crenothrix* in Silesian farm drains, where its development was so rapid as to completely choke the drains in a few days. It was also studied by Professor Cohn, and finally at Berlin by Dr. Zopf, whose paper† is a complete account of all stages of its life-history.

In decay this plant gives rise to the most offensive tastes and odors, so that Dr. Zopf's title, *Crenothrix polyspora* die Ursache der Berlin *Wassercalamität*, can be taken as expressing the consternation of the people of Berlin at the apparently serious calamity which had fallen upon their water supply.

On investigation it was found that *Crenothrix* was present in the wells, reservoirs, force mains and distribution pipes, in various stages of development and decay. The spores (gonidia) are from $\frac{1}{25000}$ to $\frac{1}{100000}$ inches in diameter, and after passing through a series of developments, which Dr. Zopf has thoroughly illustrated in the three elegant plates accompanying his paper, finally attain to the condition of long filaments, which under the microscope are found to be a series of individual cells enclosed in a gelatinous sheath. Figure 1, Plate IV, illustrates the appearance of a typical mass of these filaments in an early stage of development.

In the beginning these filaments are transparent and colorless, but they finally become olive or brown by absorption of iron. Frequently they become so incrusted with iron as to render their structure invisible (see Figure 1, Plate V), but the application of dilute muriatic acid will dissolve the iron, again revealing the structure.

The investigation also showed that *Crenothrix* developed and passed through the various stages of its life-history in the ground itself, and consequently without access to light. This was proven by sinking a number of driven wells to various depths in the vicinity of the supply wells and the taking of water therefrom for examination under such con-

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* In *Flora Europæa Algarum Aquæ dulcis et submarinæ*, by L. Rabenhorst.

† *Entwickleungsgeschlichtliche Untersuchung uber Crenothrix Polyspora die Ursache der Berlin Wassercalamitat.* By Dr. W. Zopf.

ditions as precluded the possibility of error. In some cases the plant was found more than 75 feet below the surface.

It also appeared from Dr. Zopf's study that iron in solution is necessary for the existence of *Crenothrix*, as even in the early stages of development, when the filaments are quite colorless, the application of potassium ferrocyanide will turn them blue, thereby indicating the presence of iron, while, as already stated, in the later stages the reducing or transforming action becomes so energetic as to completely incrust the filaments to the extent of concealing the structure. Lime, however, it is said, destroys it. At Halle, where *Crenothrix* also gave trouble similar to that at Berlin, the admixture of water from another source is claimed to have brought about its extermination.

Dr. Zopf farther found that *Crenothrix* endured a temperature of 11 degrees Fahr., and when thawed out after several weeks' freeze, revived and developed new plants.*

In the United States *Crenothrix* has been found in moderate quantities in ground water supplies at several places in the State of Massachusetts,† and in larger quantities in the water supply of the city of Jamestown, N. Y.

Early in the summer of 1886‡ the water furnished by the Jamestown Water Supply Company became the source of such serious complaint that the Mayor of Jamestown addressed a communication to Governor Hill, requesting him, under authority of Section 8 of Chapter 322 of the Laws of 1880, to require the State Board of Health to investigate the difficulty. The Mayor's letter states "that the water which the Company furnishes is filled with dead and decayed fish, or portions of fish, decayed vegetable matter, and is thick and muddy, and has an offensive smell." The Water Company also wrote the Governor setting forth that they were the victims of "a long continued and malicious attack upon the quality of the water furnished through the

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*A more complete account of *Crenothrix* can be found in a paper, Natural Filtration, at Berlin, by Professor William Ripley Nichols, in Jour. Frank. Inst., Vol. CXIII (1882), pages 209 to 216. Professor Nichols has cited a number of references to the literature.

An abridged account can also be found in Professor Nichols' Water Supply, Chemical and Sanitary, pages 125 to 128.

† See Report of the biologist, G. H. Parker, in the Nineteenth Annual Report of the Massachusetts State Board of Health (1887), pages 89 to 94. *Crenothrix* is given as *Hypheothrix*.

‡ See Investigation of Jamestown Water Supply, in the Seventh Annual Report of the New York State Board of Health, 1886, pages 190 to 334. The various letters and reports referred to in the discussion may be found in the account of the proceedings in the Jamestown case there given.

mains by this Company," and heartily joining in the request for an investigation by the State Board of Health. The Company also recite in their letter that analyses have already been made by a number of eminent chemists (five in all), whose reports were at the service of any person who might be detailed for the investigation. On these requests the Governor referred the matter to the State Board of Health for investigation, which Board thereupon employed E. Kuichling, M. Am. Soc. C. E., for this purpose. The source of the water supply of Jamestown, as stated by him in his report, is as follows: First, from a series of driven wells from eighty to one hundred feet deep; Second, from the foot of Chautauqua lake, by means of a conduit, part wood and part iron, about two miles long. This conduit leads through a swamp and receives the water from the lake about two hundred feet from shore in a timber crib, with wire screens placed opposite the mouth of the conduit to prevent small fish getting in. The third source of supply is from the outlet of Chautauqua lake, near the pumping station. The original design of the Company was to obtain substantially its full supply from the driven wells, taking water from the Chautauqua outlet only on those rare occasions when the suppression of fires required the use of large amounts for a short time. As the use of water increased it was found that the yield of the wells was diminishing, so that another source of supply had to be secured, and with this in view the Company laid the conduit to the foot of the Chautauqua lake, as referred to above, but this conduit was not used until about the middle of September, 1886.

It appears from the reports that for two or three years previous to the formal complaint to the Governor in 1886, the Jamestown water had been a number of times the source of serious complaint on the part of the consumers, the trouble disappearing in winter and returning in spring and summer, and it was generally held by the citizens of Jamestown that the offensive tastes and odors were due to decomposing matter from the impure water of the Chautauqua outlet. The officers of the Company, however, denied that any considerable amount of water was pumped from the outlet, and maintained that only on the occasion of two large fires had it been used at all after June, 1885. In the meantime the most serious trouble had been experienced during the spring and early summer of 1886, becoming so serious as to lead to the request to the Governor by the Mayor on June 30th, 1886.

The investigation can be found at length in the place cited, and it is unnecessary to go into the detail here, except in a general way it may be said that very little definite light is thrown upon the subject. The chemical examinations show the water from the driven wells to be of excellent quality, as was also that drawn directly from the mains (see the chemical analyses in detail, pages 219 to 224, *loc. cit.*). None of the chemists report any bad tastes and odors, except Professor Leeds, in water taken from a dead end. Professor Leeds also attributes the trouble to algæ derived from the wells, but gives no intimation of the presence of *Crenothrix*. In the latter part of the month of December, 1886, after all the reports of the State Board of Health Investigation had been made, some samples of the Jamestown water were placed in my hands by Mr. Kuichling, with the request that I make an examination thereof. This was done with the result of easily making the identification of *Crenothrix*, and the photographs which accompany this paper (Plates IV and V) are from the samples furnished at the time. The matter was subsequently referred to Mr. Wolle, who verified the identification, and thereupon placed *Crenothrix* in his work on the Fresh Water Algæ of the United States, at that time just going to press.*

The suggestion that *Crenothrix* was possibly present in the Jamestown water was first made, so far as I am aware, by Mr. Kuichling. Owing, however, to lack of high power objectives, he was unable to verify his inference.† Nevertheless, considering the lack of information among water works engineers about cryptogamic botany, the making of the inference at all is an illustration of his broad attainments and may be justly cited as adding to his fame as an engineer.

In concluding this part of the paper, it remains to be stated that there is no absolute proof that the *Crenothrix* was actually derived from the driven wells. The company began to take water from its conduit to Chautauqua lake in September, 1886, while the samples containing *Crenothrix* only came to my hands in December, and the plant may have been derived from the swamp through which the wooden conduit passes from the pumping station to the lake. This wooden conduit is

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* See Wolle, *loc. cit.*, pages 294 of text, and Plate CCIX.

† *Crenothrix* can be seen, and, when one is familiar with it, readily recognized with a fair quality, one-half inch objective; but its complete study requires a good, recent, one-eighth, homogeneous immersion objective.

as I understand, loosely constructed, so that the possibility of such derivation probably exists. On the other hand, the persistency of the trouble when it had once appeared, and the fact of its only appearing after the material lowering of the ground water, would be a strong indication of its origin in the deep wells. Failure to find it at an early stage of the investigation can be accounted for by considering that no one was looking for *Crenothrix*. In any case, the whole matter is a good illustration of the importance of broad study in biology as an imperative requirement for success in such investigations.

Volvox globator, a unicellular form containing not only a mucilaginous jelly, but also starch grains in the chlorophyl (grass-green), and belonging to the order *Protococcoideæ*, is very widely distributed, being found in nearly all parts of the world. Its presence in water supplies has been noted,* but the fact that it will cause the fishy taste and odor has, I think, been generally overlooked. The water supply of the city of Rochester had, however, an infliction of this sort in the fall of 1888, the particulars of which are briefly as follows: In the early part of September, the Water Department began to receive complaints from various localities in the city that the water was permeated with an exceedingly strong fishy taste and odor, and the opinion was quite strongly held by many citizens that a large number of fish had in some way gained access to the mains, and that the difficulty was caused by their death and decay therein. It appeared, however, that the odor in question was precisely that of living fish rather than dead, and only a moderate amount of experiment is required to show that the odor from decaying fish bears no relation whatever to that which they exhale when alive. A daily study with the microscope showed that only the usual amount of animal and vegetable matter was present, with the single exception of an evidently sporadic development of *Volvox*. This alga was present in the distributing mains, and in both reservoirs† in vast quantities, the filtering of so small an amount as three hundred gallons showing many hundred thousands; while single dippings from the reservoirs without any con-

* See Report of the biologist, G. H. Parker, in Nineteenth Annual Report of the Massachusetts State Board of Health, *loc. cit.*, where the presence of *Volvox* in a number of water supplies in Massachusetts is indicated.

† For detailed account of the relation of the storage and distributing reservoirs to Hemlock lake, the source of the supply, see the various reports of the Chief Engineer, especially those of 1872, 1874 and 1877.

centration by filtration showed the presence of the little globes in great abundance. This was the more remarkable from the fact that an almost daily study for nearly two years, and a frequent study for more than a year before that, had utterly failed to develop the slightest indication of *Volvox* in this water. So far as any record goes previous to last summer, *Volvox globator* had never been seen in the domestic water supply of the city of Rochester. No study of the matter was made at Hemlock lake itself until about October 20th, at which date only a few *Volvoces* were found, while at the same time both reservoirs teemed with them, and continued to so teem for two or three weeks thereafter. On the approach of cold weather the quantity gradually lessened, until in January it was only rarely that a single individual could be seen. In the meantime, the fishy taste and odor diminished as the *Volvox* lessened in quantity, and finally ceased to be perceptible to the senses as soon as the quantity of *Volvox* became small.

A satisfactory demonstration that *Volvox* was the real cause of the fishy taste and odor was made in the following manner.

Filterings from two or three hundred gallons of water were obtained by allowing the water to flow freely through a bag of plain, unbleached muslin attached to an ordinary kitchen cock. This concentrated everything contained in the quantity filtered into the volume of a few ounces. The concentrated filtrate was then passed through chemical filter paper with the result of leaving the contained *Volvox* behind on the paper, while clear water had flowed through into a clean Florence flask beneath. The *Volvox* was now washed off the filter paper with distilled water and the washing also introduced into a clean Florence flask. Gentle heat was then applied to both flasks, with the result of developing the fishy taste and odor with an intensity dependent upon the amount of *Volvox* present in the original filtering. After gentle boiling the contents of both flasks were emptied into open vessels and allowed to become entirely cold. On again pouring into the flasks and reheating, it was found that the contents of the first flask developed no farther fishy taste or odor, while the contents of the second flask again showed the taste and odor plainly perceptible. The significance of these tests is that in the case of the first sample, from which *Volvox* itself has been entirely removed by filtration through the filter paper, the application of heat the first time had driven off all the volatile matter causing the fishy taste and odor, so that after cooling nothing remained to be developed

by reheating; while in the case of the second sample, which still contained the original portion of *Volvox*, reheating again plainly developed the fishy taste and odor, but with less intensity than after the first heating. A repetition of these tests under varying conditions left no doubt as to the agency of *Volvox* in producing the fishy taste and odor.

The filterings contained in addition to living and decomposing *Volvox* a large amount of brown-colored amorphous matter, and that this was produced by the decomposition of *Volvox* was easily shown by allowing a concentrated filtering to stand for a short time, when, after the amorphous matter had settled to the bottom, it was easy to syphon off the supernatant fluid, containing only living *Volvox*. On heating the fluid so obtained and allowing it to stand for forty-eight hours, it was found on examination to contain an amorphous sediment precisely similar to that in the original filtering.

The considerable increase in the amount of animal life which followed this development of *Volvox* may also be noted as of interest. This was especially noticeable in the case of a number of the Rotifera, as, for instance, *Anuræa* and *Synchæta*. These transparent little creatures fed abundantly upon *Volvox*, and their structure and organs of assimilation are so clearly revealed under an ordinarily good objective as to exhibit the whole process of nutrition. The green-pointed spheres of *Volvox* could be easily seen rolling along through the æsophagus of the Rotifer on their way to the digestive cavity, where their gradual reduction to food and the ultimate rejection of the dross could be all clearly seen.

As to the reason for the sporadic development of *Volvox*, it appeared that some special cause had operated to produce a much greater development in both the reservoirs than in Hemlock lake, and a discussion of this point will lead us to consider briefly the salient points of reproduction and development.

Volvox may be described as a transparent sphere from one-fiftieth to one-eightieth of an inch in diameter, the inner surface of which is studded with groups of dark green points regularly disposed, and so arranged that each group of points is the center of a symmetrical group of six others. These green points are called gonidia, and each one is probably capable of becoming a perfect *Volvox*, though in fact only a small number of the many hundred with which every perfect specimen is endowed do actually so develop. If we consider, however, that under favorable conditions their growth is comparatively rapid, a ma-

ture individual developing from the embryo in a very few hours, and in its turn giving birth to a new colony, each of which sends forth its progeny, we are led to understand how a large body of water apparently entirely destitute of *Volvox* may in a few days be made to teem with it.

Extended observations of the alternations of the generations of *Volvox* show that its reproduction may take place in two ways, either sexual or a-sexual, and that the development from year to year is an alternation of these two methods, the usual course being development by one process in the spring, and by the other in the fall.* This alternation has for its climax the production of resting spores, which pass into a resting state, disappearing from view, and ultimately revivifying and producing the mature plant.

The *rationale* of the process of revivification is as yet unknown. Professor Cohn held that after passing into the resting state the spores must be desiccated before they could revive and produce living plants. This has, however, been contradicted by later observers; but the consideration that both of the reservoirs were entirely destitute of water during the summer of 1888 at any rate lends interest to this view in the present connection. Following this line it would appear that the extraordinary development in the reservoirs was from the revivification of spores which had possibly been dormant for several years, and which only developed after the absence of water from both reservoirs for the first time since their construction, had, by desiccation, produced the conditions necessary for revivification.

Moreover, the season of 1888 was one of unusually prolific development of *Volvox* throughout western New York, proven by finding large quantities in all the usual haunts. Hemlock lake, therefore, contained the moderate quantity which any such body of water would naturally contain in a season of unusually prolific development; while the reservoirs of the Rochester Water Works, by reason of having been entirely dry, presented the conditions for sporadic development. In any case the question is one of the greatest interest, but as this paper has already exceeded the limits originally set, it cannot be pursued at greater length here†.

* See A. W. Willis on the Structure and Life-history of *Volvox*, in the Midland Naturalist, September, October, 1880. Also summary of same, in Cooke's British Fresh Water Algæ, pages 56 to 63.

† This account of *Volvox globator* at Rochester is abridged from two reports made at the request of the Executive Board by M. L Mallory, J. Edward Line and the present writer, Members of the Microscopical Section of the Rochester Academy of Science. For these reports, giving a more complete account of the matter, see the forthcoming Annual Report of the Executive Board of the City of Rochester for the year ending April 1st, 1889.

It appears to the present writer that considerable advantage would accrue in the study of the important sanitary questions growing out of the relations of the algæ to public water supplies if we had a system of classification more in accordance with their real affinities in relation to the development of the troubles which we are now considering. Mr. A. W. Bennett has for another purpose proposed essentially such a system of classification, in which he places the colorless algæ in one class, those without distinct chlorophyl or starch grains in another, and those possessing pure chlorophyl and starch grains in a third. The use of this will assist by giving something on which we can systematically string our knowledge as it is slowly attained.*

A study of the kind here attempted would be incomplete without some indication of the real purpose of the algæ in the final economy of things. The late Professor William Ripley Nichols, who easily stood first as an accurate observer, remarked very justly that they cannot be regarded as an unmitigated evil; that they are purifying agents, changing at least a part of the dissolved organic matter into insoluble substances which settle to the bottom.† This is a fair statement of the case, rendered more apparent by the consideration that nitrogen is an element largely needed in their growth, and that this is obtained from the nitrates and ammonia contained in the water. In adopting this view, however, we are forced to the conclusion, which is indeed sufficiently indicated in the foregoing, that the presence of certain algæ, in any given locality, may be a danger signal whose warning ought not to be lightly disregarded, while, in other cases, their presence is equally an indication that, impurities being present, nature is doing her very best to get rid of them. In any case, much remains to be done before the subject can be considered as strictly scientific in the sense of having a large stock of classified knowledge.

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* In Jour. Linn. Soc., Lond., Bot., XXIV (1887), pages 49 to 61. Also abst. in Jour. Roy. Micr. Soc., 1887, page 786.

† Sand Filtration at Berlin, Jour. Frank. Inst., Vol. CXII (1881), pages 436 to 440.

DESCRIPTION OF PLATES.

Plate I (LXXVII).

Fig. 1 $\frac{90}{1}$. *Draparnaldia*, showing the penicillate, fasciculate branchlets. The broad chlorophyl band or main filament not shown.

Fig. 2 $\frac{100}{1}$. *Cladophora*, a series of articulate filaments, with the parietal cell contents shrunken, showing the thick, lamellose, gelatinous cell-membrane. (The shrunken matter is the chlorophyl mass containing the starch grains.)

Plate II (LXXVIII).

Fig. 1 $\frac{90}{1}$. Two filaments of sterile *Zygnema* showing the usual form in which *Zygnema* will be found.

Fig. 2 $\frac{90}{1}$. *Spirogyra nitida* in conjugation. The filament on the left is sterile and represents the ordinary form. (This photograph is from a permanent mount and the chlorophyl bands in the sterile filament have shrunken by reason of the preservative medium used.)

Fig. 3 $\frac{90}{1}$. *Desmidium*, a single filament in the vegetative condition. The dark central portions are the chlorophyl masses of each cell.

Plate III (LXXIX).

Figs. 1 to 4 $\frac{200}{1}$. A growing condition of *Ulothrix* which gave a strong pig-pen odor on decay. The chlorophyl masses, containing the starch (amylaceous) granule and the thin cell membrane, clearly apparent in all the figures.

Plate IV (LXXX).

Crenothrix polyspora, Cohn.

Fig. 1 $\frac{560}{1}$. The central filament in process of development, with the exterior sheath clearly shown. The terminal members exceeding in length those towards the middle. (This is a typical development, according to Zopf.)

The curved filament at the right is another stage of development in the process of formation of gonidia.

Fig. 2 $\frac{200}{1}$. A typical mass of *Crenothrix* showing a number of fruiting filaments (sporangia). The irregular masses are points where gonidia have broken through the sheath and colonies of filaments are developing.

Fig. 3 $\frac{850}{1}$. The upper curved filament another typical form of development, according to Zopf.

Plate V (LXXXI).

Crenothrix polyspora, Cohn.

Fig. 1 $\frac{200}{1}$. An irregular mass of *Crenothrix* filaments, with structure obscured by a deposit of oxide of iron.

Fig. 2 $\frac{500}{1}$. A typical development similar to Plate IV, Fig. 1.

Fig. 3 $\frac{500}{1}$. A fruiting filament (sporangium), with fully developed gonidia. This filament shows the completion of the process faintly foreshadowed in the curved filament to right in Fig. 2, Plate IV.

Plate VI (LXXXII).

Fig. 1 $\frac{65}{1}$. *Hydrodictyon utriculatum.* A young net pressed flat and showing the reticulations of the saccate cœnobium.

Fig. 2 $\frac{65}{1}$. A number of reticulations in a more advanced state than in Fig. 1.

Plate VII (LXXXIII).

Fig. 1 $\frac{15}{1}$. *Hydrodictyon utriculatum.* A fully developed single reticulation, with the endochrome of the individual cells shrunken, showing the gelatinous exterior cell membrane.

Plate VIII (LXXXIV.)

Volvox globator.

Fig. 1 $\frac{100}{1}$. A fully developed parent cœnobium with two generations of young cœnobia within the parent. The first generation just ready to destroy the parent by breaking forth.

Fig. 2 $\frac{150}{1}$. Same as Fig. 1, but showing the peripheral cells, clearly, at the edge of the parent, and also on such of the first generation from the parent as are in focus. The beginning of the disintegration of the parent cœnobium is more clearly shown than in Fig. 1.

Fig. 3 $\frac{150}{1}$. A young cœnobium just after breaking forth from the parent.

Plate IX (LXXXV).

Volvox globator.

Figs. 1 and 2 $\frac{100}{1}$. Mature individuals illustrating stages of development somewhat different from any illustrated by the previous figures.

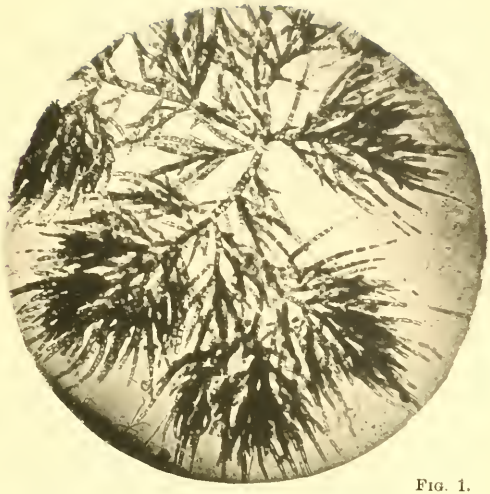


FIG. 1.

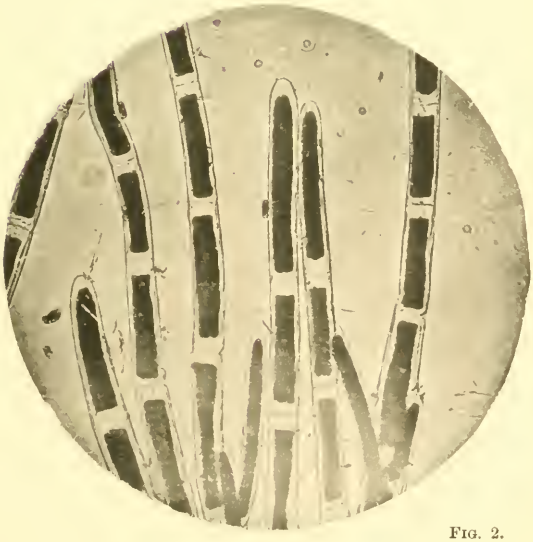


FIG. 2.

Photo-Micrograph,
by GEORGE W. RAFTER.

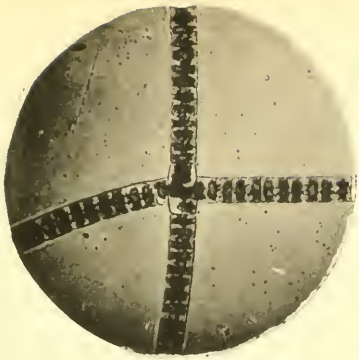


FIG. 1.

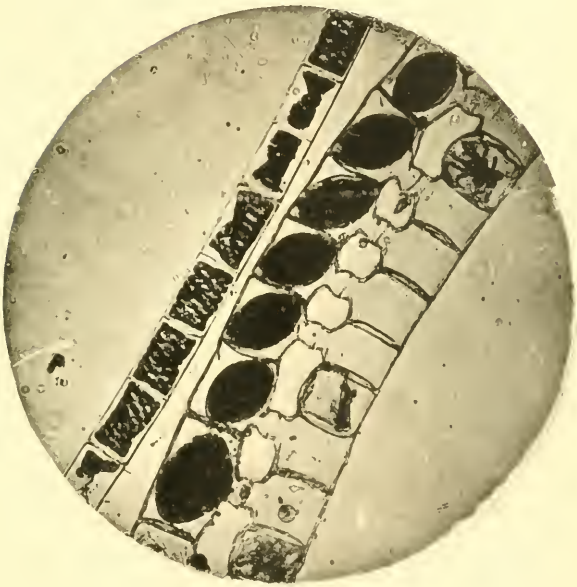


FIG. 2.

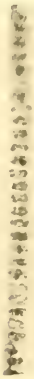


FIG 3

Photo-Micrograph,
by GEORGE W. RAFTER.

FIG. 1.

FIG. 2.

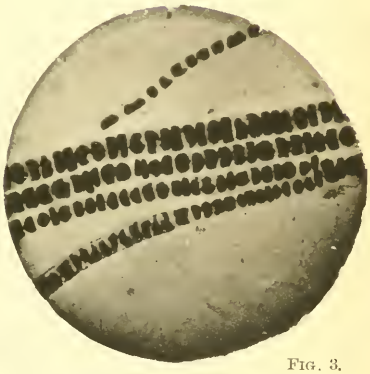
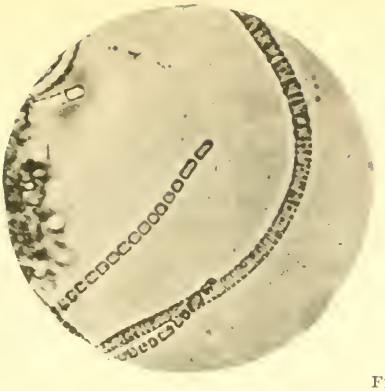


FIG. 3.

FIG. 4.

Photo-Micrograph,
by GEORGE W. RAFTER.



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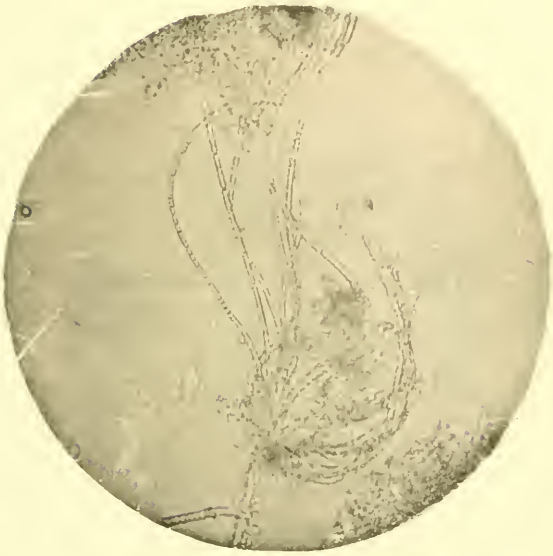

FIG. 2.

FIG. 3.

Photo-Micrograph,
by GEORGE W. RAFTER.

FIG. 1.

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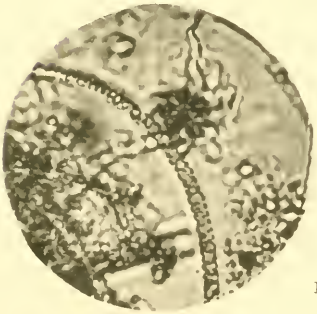

FIG. 3.

Photo-Micrograph,
by GEORGE W. RAFTER.

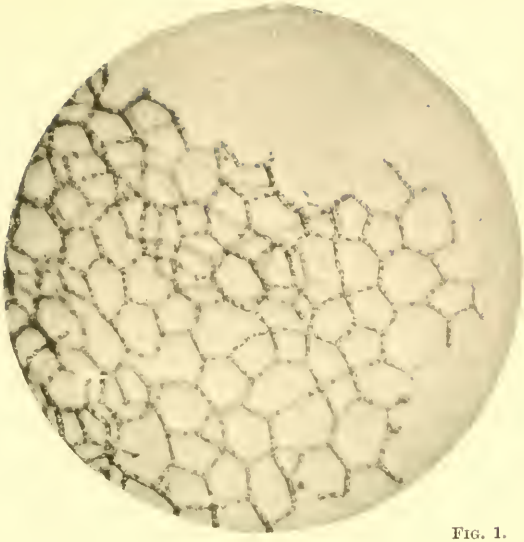

FIG. 1.

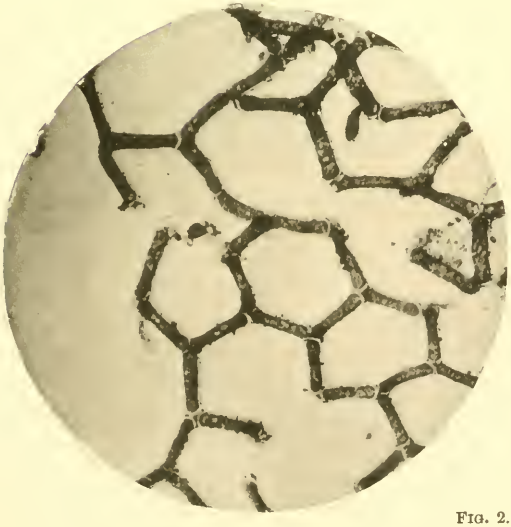

FIG. 2.

Photo-Micrograph,
by GEORGE W. RAFTER.

FIG. 1.

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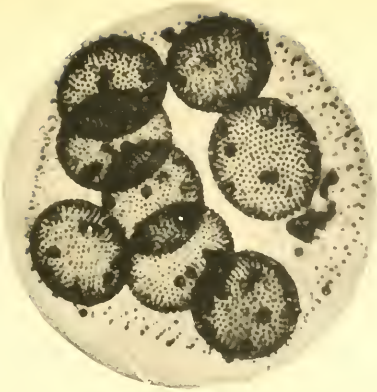
by GEORGE W. RAFTER.



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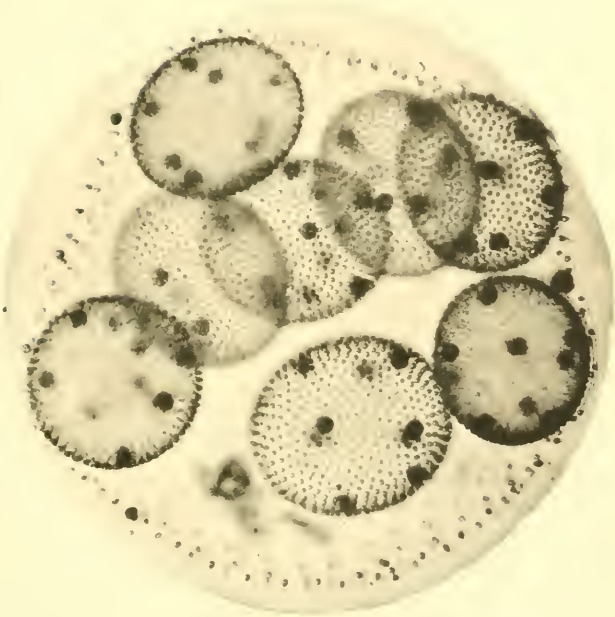

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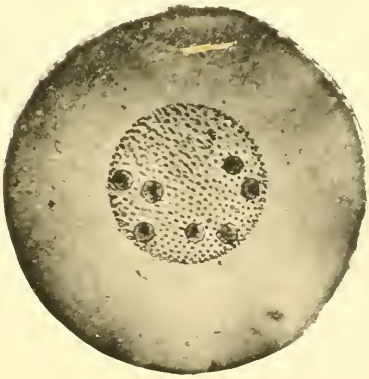
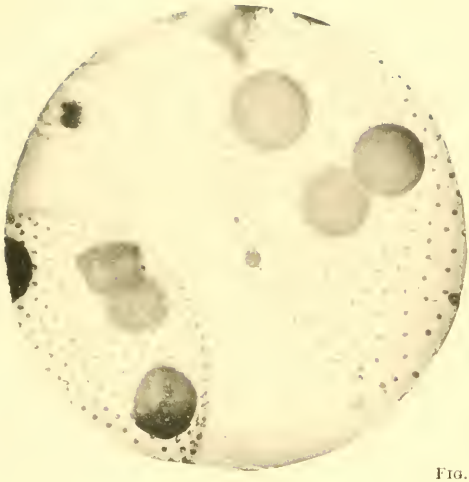


FIG. 3.

Photo-Micrograph,
by GEORGE W. RAFTER.



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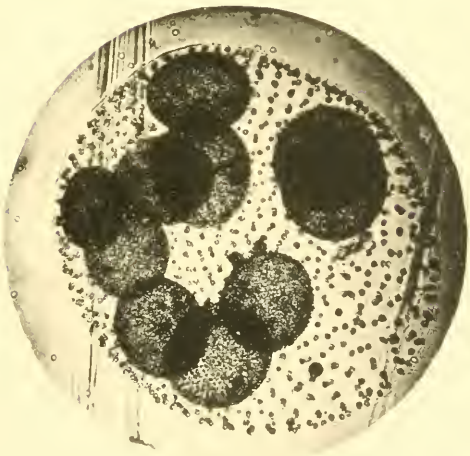


FIG. 2.

Photo-Micrograph,
by DR. J. EDWARD LINE.

DISCUSSION.

Professor ALBERT R. LEEDS.—I am hardly prepared at so short a notice to discuss the elaborate paper of Mr. Rafter in the manner which it deserves, and I can only bring to your notice a few observations in connection therewith, which I trust may be worthy of your attention.

In the first place I think it is of considerable interest, and especially to engineers of the City of New York, that one of the earliest inquiries into this subject was made by Dr. Torrey so long ago as the summer of 1859. At that season the water supply of the City of New York was extremely offensive. Dr. Torrey examined the water at Croton dam and found that much of it was of a bright green color, which color proved to be due to the accumulation of algæ. He took the water from the city hydrants and concentrated it by evaporation, when it developed a smell similar to that of the water taken at Croton dam. He took some of the water containing the algæ and distilled it. The distillate contained a volatile, oily material in which was concentrated a taste and odor similar to that affecting the whole water supply. He attributed this offensiveness to the growth of algæ in the water. He gave it as his opinion (one deserving of much attention as coming from so eminent a biologist, chemist and botanist) that while the water was so offensive, yet it was wholly without deleterious influence on health.

I notice that Mr. Rafter speaks of the development of the offensive taste and smell in connection with certain varieties of algæ containing starch. I note likewise that he states as the result of an experiment upon a solution of starch in water, that this solution after standing for several days gave a similar stench. In this connection I would suggest that the starch that he experimented upon was probably not chemically pure. The ordinary commercial starch is not, containing in addition to starch, which is a carbo-hydrate, some albuminous matter. The offensive taste and smell were probably associated with the albuminoid impurity.

Now in regard to the taste and odor affecting the water supply of the City of Jamestown. As consulting chemist of the Jamestown Water Company, I think it would not be right to allow this occasion to pass without protesting against what I think is an erroneous impression inadvertently conveyed by Mr. Rafter's paper. The Jamestown Water Company originally derived its water from a series of driven wells, sunk to a depth of 80 or 100 feet, believing that these wells were their purest obtainable source of water supply. To the best of my knowledge, which is derived from personal inquiries upon the spot, the company never resorted to the water taken from the outlet of Chautauqua lake except in case of fires. When this great trouble, this offensive taste and smell had been noticed, which was long prior to the use of the Chautauqua lake

water, they constructed a conduit to the foot of Chautauqua lake. They did not make use of the water from that source, however, until I urged them to do so. I made this recommendation because analysis had shown that the water taken from Chautauqua lake was of good quality, in fact not inferior in point of excellence (except in regard to its containing a higher percentage of carbon) to the water taken from the driven wells. I ascribed the presence of the algæ to the water from the driven wells, and connected with the growth of these algæ the presence of the large amount of oxide of iron in the mains. Unfortunately I did not make the identification of the algæ as *Crenothrix*. I am delighted to hear that that identification has been successfully made.

Mr. Rafter has quoted with approbation the monograph of Dr. Zopf, entitled "*Crenothrix*, the water calamity of Berlin." One could with equal truth say, "*Crenothrix*, the water calamity of the Jamestown Water Supply Company," because that company owed to the *Crenothrix* a persecution which for violence and injustice has rarely been exceeded.

A few words in regard to the development of diatoms. In the summer of 1886, the water in the Greenwood Cemetery reservoirs became extremely offensive, and Mr. Charles B. Brush, M. Am. Soc. C. E., submitted that water to me for examination. The supply was from driven wells. This water, filtered as it is through the surface strata of sand, in its original condition, is of great purity. But after being pumped up into the reservoirs it showed an abnormal development of diatoms. I attributed the bad odor and taste to the presence of these diatoms, or rather to the growth and decay of that gelatinous jelly which envelops them as a matrix. The analyses of the reservoir water as compared with those of the original water in the driven wells exhibited a great increase of carbonic acid in solution and of ammonia, and a great decrease of dissolved oxygen.

The chemical phenomena in the Greenwood Cemetery reservoir were similar in their character to those in the water of the Hoboken reservoir in the summer of 1884, although in the latter case the growths were not diatomaceous, but were due to the development of algæ belonging to the genus *Oscillaria*.

I have omitted saying anything as to the suggested remedy. When the Jamestown Water Company requested a remedy, I suggested two—either to exclude light from their reservoir or to aerate the water. They elected the former and covered the reservoir. The exclusion of light had not the slightest influence in preventing the growth of the algæ and the development of the offensive taste and smell.

Professor Elwyn Waller.—It is certainly an encouraging sign of the times that the growing interest in sanitary science has induced, among other things, a more careful study of the phenomena connected with odors in water supplies during the last fifteen years or so. I remember that in 1876, when, as chemist to the New York Board of Health, I was

called upon to investigate the subject in connection with the appearance of odors in the Croton water, the record of observations on the subject was exceedingly meager and unsatisfactory. Our knowledge, however, is not yet in such a form that in any given case we can immediately detect the cause and apply a remedy, but a considerable advance has been made.

In the investigation referred to I found that out of say one hundred and fifty public water supplies in various sections of the country, about half of them had had some experience of the kind. Of those places where, within the knowledge or recollection of those then having charge of the water supply, no experience of odors in the supply had been had, several have since had manifestations of the phenomena. In the replies which I received to a circular-letter of inquiry the data furnished were often insufficient, especially where no odor was reported; but, so far as I could learn, of those reporting no odor, nine or ten at least were from artificial reservoirs, and four were on limestone formations. Of those where odors had appeared, at least twenty-six drew their supplies from artificial and eight from natural lakes. Naturally fuller data were given where odors had been perceived, and it may be erroneous to generalize from these figures.

When the matter first attracted general attention, and the algæ were said to be the cause, many doubts were thrown on this view of the case. I remember, in 1876 or 1877, receiving a letter from Professor W. R. Nichols on the subject, in which he remarked, "There is too much of the *Post hoc, ergo propter hoc*" about this. In an annoying case of the occurrence of the "cucumber odor" in the "Bradlee basin" at Boston in 1876, report was made by Professors Nichols, Farlow and Burgess that the water contained apparently no more chemical substances or vegetable or animal life than usual. Later on, with a recurrence of the trouble, possibly from the same source, Professor Remsen found the cause in the decomposition of the *Spongilla fluviatilis*, which, having been attached to the sides of the reservoir and not floating in the water, may have perhaps escaped notice in 1876.*

The fishy character of the odor in some cases led many to suspect that the presence of considerable numbers of fish, or the prevalence of some disease among them was the cause. At several places reservoirs were emptied at considerable expense, only to find comparatively few fish, and those few in a healthy condition. The thought has occurred to me that in some cases more fish instead of fewer might be a remedy,

* Note by G. W. R.—The sponge which Professor Remsen found at Farm pond, and figured in his Report on a Peculiar Condition of the Water at Boston, in November, 1881, is properly, by the recent work of Mr. Potts, *Meyenia fluviatilis* instead of *Spongilla fluviatilis*, Auct, as given by Professor Remsen on the authority of Mr. Hyatt. For modern classification of fresh water sponges see Contributions towards a Synopsis of the American Forms of Fresh Water Sponges, etc., by Edward Potts, in the Proceedings of the Academy of Natural Sciences of Philadelphia, April-August, 1887.

by serving to preserve the proper balance between the animal and vegetable forms of life. In some cases, however, a fungoid growth on the bodies of the fish has accompanied the presence of odors in a water (Haverhill, Mass.). Professor Nichols, in his book on Water Supply, suggests that when the algæ form scums on the surface of the water it may cut off the supply of air necessary for the fish.

The theory of the production of innumerable bacteria, when there is nothing for them to feed upon, seems unlikely. Other theories were advanced, some more or less absurd. I recall one, that the action of the water upon vegetable matter produced crenic and apocrenic acids which attacked the iron pipes, and thus gave off those ill-smelling compounds which result from the solution of cast-iron in an acid. This is untenable, since the odors are not the same, and moreover frequently have appeared where the water is not and has not been in contact with iron pipes.

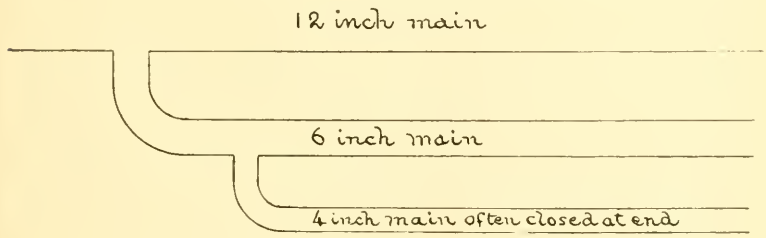
It seems now that we can accept the view that frequently, though not necessarily always, the odors in water supplies are attributable to algæ, or some other vegetable growth. Many direct experiments, such as those quoted by Mr. Rafter as having been made with the *Volvox*, constitute strong arguments in this direction. In general the presence of large quantities of certain forms of algæ just previous to or during the prevalence of odors is another. As to the counter-argument that the odors have appeared sometimes in cold weather, when the conditions are apparently less favorable for vegetable development, we seem to have found a reply in the point made by Mr. Rafter, that the algæ are extremely hardy, and not only survive but may flourish at comparatively low temperatures. With larger plants some will grow during winter, though the warmth of summer favors the most of them, and as to the algæ as a class, some representatives flourish on the snow, while others again have their habitat in hot springs. As to odors in water, they occur most frequently in summer, though they sometimes appear in winter.

It seems probable that a fair uniformity of temperature has a considerable influence; that the algæ observed by Mr. Rafter in March, when the snow had scarcely gone, would have been destroyed by a sudden rise in temperature, though they might, perhaps, endure and flourish if the temperature were gradually raised to that of summer. A sudden fall of temperature was noted to be fatal to certain of the *Nostocs* (Albany), and a high temperature is recorded as having killed multitudes of the *Clathrocystis* (Horn Pond, Mass.).

In this connection it may be well to recall the observation of Mr. T. W. Davis, at Poughkeepsie, that when the temperature of the water in the filter beds passed 70 degrees Fahr., the odors in the water began to cause complaint. From this we may only legitimately conclude that the temperature of 70 degrees and over will favor the development of odors from certain algæ.

This serves to emphasize one point of importance, that reservoirs for water should not be too shallow.

Some other points which Mr. Rafter did not mention may be here alluded to as apparently having influence on the development of the offending algæ. One, that in some cases the establishment of a circulation of the water in the reservoir has mitigated or removed the trouble (New Haven). The effect may have been due to aeration, to prevention of stagnation, or to both. In that connection it may be said that the testimony as to the prevalence of the odor in dead ends of a supply system is very conflicting. Observers as to this point may sometimes be deceived, at least in some of our up-town streets in New York, where, as I am told, in many cases, though a large main in which there is plenty of flow runs through the center of the streets, the houses on the side of the streets draw from a second or third side main parallel to the chief one, and are in reality drawing from dead ends. The arrangement as described to me is something like this:



Another point is, that the filling up of a basin without previously cleaning it is likely to leave to decay in the water numerous trees and other forms of vegetable life, which furnish food and shelter for an immense variety of algæ and other forms of life which may produce odors. I anticipate that if the Quaker bridge dam is built, and (as I understand is the plan) the ground is not previously cleared before the water is let in, we shall have, on a large scale, a repetition of the conditions produced at New Britain, Conn. There the town could not afford the expense of cleaning the reservoir site, and they had an appearance of odor every summer, which was less and less every year for about twelve years. In 1874, the thirteenth year, I believe, they had a short and sharp attack, lasting about two weeks, during which time the President said it had been "never worse."

Among the conditions favoring the development of algæ, or upon which some of them are dependent, Mr. Rafter mentions the presence of certain mineral compounds, as sulphates for *Beggiatoa*, etc.; lime for *Batrachospermum*, etc.; iron for *Crenothrix*; silica for *Inactis;* and sodium chloride for marine forms, and possibly in smaller amounts for

most fresh-water forms as well. If he could add to the list potassium and magnesium compounds, we would have the full quota of mineral substances which are usually found in all natural waters. These observations are of great interest, but will be of limited service in measures of prevention or cure.

As to the more immediate cause of the odors, Mr. Rafter states that they may be caused (1) by volatile oils; (2) by decaying starch; or (3) by decaying nitrogenous jelly secreted by certain forms. The latter, in his estimation, is probably the usual one. This brings up the rather important question as to whether the odoriferous principle is developed during the life or after the death of the algæ. From the evidence which has been collected, it seems reasonable to conclude that in some cases it is the one; in other cases, the other. It seems probable, or at least possible, that the formation of a volatile oil may be coincident with the fructification of some algæ, and that this is immediately followed by the death of the parent, and under such conditions it would be a difficult matter to decide the point.

The presence of any volatile oil in *Nostocs* and many other odor-producing algæ is denied by Mr. Rafter. In that connection I may mention the extraction of a fatty substance, soluble in ether, called by Dr. Torrey "vegetable wax," from the residues of water teeming with *Nostoc*. It was without apparent odor.

The observation as to the nitrogenous character of the jelly was illustrated by an examination which I had occasion to make on the Croton water in 1881, probably at the time referred to by Mr. Rafter as mentioned by Mr. Hyatt.

Of the worst samples the filtered water contained but little ammonia, chiefly albuminoid, but the unfiltered water showed considerable amounts of both kinds of ammonia.

Mr. Rafter further remarks upon certain forms of life which have hitherto been regarded as innocent of the production of odors, and makes out a strong case against *Volvox* and some diatoms. The remark as to the appearance of a rotifer as the enemy of *Volvox* suggests a similar observation of Dr. C. T. Jackson, in connection with the Cochituate water, that *Cyclops* was an enemy of certain odor-causing substances or organisms. Moreover, Professor Lupton reported upon the examination of the water of Nashville, Tenn., when it was affected, that *Cyclops* was very abundant, and in one case of the trouble in the water supply of Trenton, N. J., Dr. Henry Wurz attributed it to the presence of *Cyclops*. We have here either another supposed innocent organism to be added to the list, or a suggestion of a possible destroyer of certain objectionable forms of vegetable life, at least, objectionable when appearing in such overwhelming numbers.

In the light of these facts may it not be the case that any one of the lower forms of life dwelling in fresh water may at times develop to an

abnormal extent, and cause odors in water by the death and decay of myriads of individuals, which are destroyed in the struggle for existence with one another? Taking that view, it is useless to designate one as an odor-producing species and another as innocent, since any one form, whether animalcule or alga, may be the cause in any particular case, and the question would bring itself down to the proper preservation of the balance between the various forms of life. Our superintendents of water works would then have to be students of aquaria.

It is no doubt true that the superintendent who regards the reservoir under his charge more in the light of an aquarium than of an exaggerated water tank, will be the more successful in his management.

Finally, Mr. Rafter suggests that the presence of these odoriferous algæ may be a danger signal. As to that, we hardly know enough on the subject to venture upon even a very guarded statement. Certainly so far as the evidence goes, there is no particular danger to health in swallowing considerable numbers of various species of algæ which directly or indirectly cause the odor. So far as I know, but one case of a poisonous alga has been put on record. *

This was in Lake Alexandria, Australia. The plant was said to be a *Nodularia*, and developed vigorously when the temperature of the water was 74 degrees Fahr. Domestic animals which drank the water containing it were seized first with stupor, then with spasms, which were followed by convulsions and death. It no doubt would also have been fatal to human beings.†

As to preventive methods which have been found efficient in some cases, I have already alluded to the attention to the question of temperature, of circulation, and also the removal of vegetable débris from the reservoir site. Another suggestion may be found in the experience of Mr. McFadden at Philadelphia, where the presence of odors was prevented by the simple expedient of taking the supply from below the surface. Any one or all of these means may, and indeed have, frequently proved ineffectual. We evidently need much more information on the subject, and consequently such information as has been contributed by Mr. Rafter is most welcome.

One of Mr. Rafter's closing remarks, though not perhaps based on a misconception, might prove misleading. He seems to imply that algæ may be purifying agents because nitrites and ammonia form a part of their sustenance. To this I feel constrained to remark that the nitrites and ammonia, even in the most contaminated waters, are absolutely

* Nature, XVIII (1878), page 11.

† Note by G. W. R.—A similar case of a poisonous alga is reported from Minnesota. J. C. Arthur has given an account of it in the "Bulletin" of the Minnesota Academy of Natural Sciences, Vol. II, Bul. IV, and Vol. III, Bul. I. The investigation, however, was only made after the fact, and some doubts remain as to the alga being the cause of the trouble. Mr. Arthur claims in his report to have finally identified the offending form as *Glœotrichia Pisum*.

harmless in the proportions in which they there exist, but that their presence in comparatively large proportion is an indication of the presence of nitrogenous organic matter in a state of decomposition which is probably therefore dangerous. If, then, the nitrites and ammonia are removed from a water, it may still be dangerous, but in the present state of our knowledge, a chemist would be likely to pronounce it safe.

A. Fteley, M. Am. Soc. C. E.—Mr. President, my excuse for rising to discuss this subject is that, having been engaged in a number of works where a great deal of annoyance was caused by the presence of algæ, I have been obliged, out of necessity, to study the question as much as it was in my power to do.

Those who have had occasion to study questions of drainage and water supply, involving the welfare of large communities, will agree with Professor Leeds that their solution is very often embarrassed and retarded by the introduction of subjects entirely foreign to the scientific character of the matter in hand, based mostly on local prejudices, and consequently introducing in the debate an amount of bitterness which is very detrimental, if not absolutely fatal, to the proper consideration of technical points.

To that cause, I believe, must be attributed the doubtful result of investigations which, if not interfered with, would have probably increased our field of knowledge.

As an illustration of the remarks just made, I remember that during one of the periods of "bad water" in Boston, at a time when excitement ran high on that account, outsiders, without sufficient knowledge of the resources and local conditions of the water supply, aggravated matters by publishing descriptions and analyses of water collected at places partially disconnected from the sources of supply and supplied in small quantities at great distances from the point of taking. The facts were most probably true, but, without proper explanation, they were misleading.

There is, most probably, a great diversity of causes for the bad tastes and odors in public water supplies; they may be often traced to the presence of microscopic vegetable growths in the reservoirs where the water is kept stored in large volume, or at any rate these growths are contemporaneous with them; but, at times, the same trouble may occur without their presence. Such has been my experience in regard to three reservoirs containing from 200 to 1 100 million gallons, which I closely observed for several seasons.

It is possible that my want of skill as a microscopist may have prevented me from finding in the water spores of algæ or other minute bodies which might have been the cause of the trouble, but at times I have failed to find, with a glass of high power, any algæ or other microscopic bodies, while at other times I could readily determine the various species which were generally found in those waters.

Professor Waller has referred to Professor Nichol's experience in regard to Bradlee basin, in Boston. As the occurrence is familiar to me, I may be permitted to add a few details which apparently confirm the statement just made.

Bradlee basin is one of two twin reservoirs from which the water received from the storage grounds of the system is mainly distributed into the city; they are separated from one another by an artificial embankment—one, the Bradlee basin, having been excavated to the rock bottom, while the bottom of the other is formed by the original meadow soil; but the water in the Bradlee basin, with a rock bottom, had such a disagreeable taste that its use had to be discontinued, while in the other division with the meadow bottom it was of sufficiently good quality to be distributed to the consumers. A few weeks after the shutting off of the contaminated basin the taste disappeared, without apparent cause, and the water was again used without complaint.

The irregularity which seems to accompany the symptoms just described is found also in the distribution of the taste among the various places of consumption. At times the bad taste seemed to be limited to certain parts of the city; at other times, although general, it was not of equal intensity in different places, or it even disappeared at irregular intervals. A singular fact, reported several times to me, was that while the taste was very pronounced on the ground floor of the house, it would be almost imperceptible at the top.

As to the habits and appearance of algæ in the reservoirs that I happened to examine with some care, I may be permitted to refer to a description published in one of the reports of the Massachusetts Board of Health; the facts therein noted may be found of interest:

"The causes of the alterations observed, especially during the summer, in the quality of the water of our public supplies, remain very often unknown, and local observations are generally found to be insufficient. A complete system of observations extending over several years, covering an extensive territory, and embracing various descriptions of water supplies, would be needed to throw any light on the question.

"The following observations have been limited to some of the ponds connected with the water supply of Boston. They cover too short a time to be conclusive, but they have furnished a few facts which may suggest some new and more complete researches.

"Of the three sources of water supplies of Boston, one, the Cochituate, has presented no unusual feature this year; the new supply from Sudbury river was in a peculiar condition. Although its water has been furnished in a large proportion to the city for the last five years, the new reservoirs were filled this year for the first time. Notwithstanding these unfavorable conditions, the water has been unobjectionable in quality; but it has been observed that sulphureted hydrogen was found in the reservoirs during July and August, and could be plainly per-

ceived when the water was flowing through the sluice-gates. The smell of the gas disappeared after a short exposure of the water to the air, and none of it was ever detected in the conduit.

"The temperature of the water in the reservoirs at the time that the presence of sulphureted hydrogen was detected in it was remarkably high, as shown by the accompanying diagram; and it was identical at top and bottom, although the depth at the point of observation was more than 20 feet. A number of readings taken in June, July and August, and not noted at the time, showed generally a similar result, which may suggest the idea that the temperature of the water was kept uniformly high by chemical action. The disengagement of sulphureted hydrogen ceased apparently toward the end of August; but it may be seen by the diagram that, since that time, the temperatures at top and bottom of Reservoir No. 3 have continued to be singularly near one another.

"The principal trouble experienced this summer about the water supply of Boston was due to the presence of an abundant formation of algæ in some of the ponds of the Mystic supply, and also, to a less extent, in one of the new reservoirs on Sudbury river (Basin No. 3).

"These minute plants, which appear to be uniformly distributed throughout the water, flow with it, and are of such a small bulk that they cannot be separated by screens. The wind has a noticeable effect on them, and often blows them toward the lee shore, where they accumulate and form a solid scum of a sharp green color. When in the fresh state they emit a peculiar musty odor; if stranded by the action of the wind, they soon decay and form a bluish-green mass which develops a smell as of organic matter in process of decomposition; when in the water the algæ remain suspended for some time, and, after a while, sink to the bottom. This result, which it would be difficult to observe directly in a large mass of water, is indicated by the fact that the large quantities of algæ which have been found in the reservoir, at a time when there was no outflow from it, have diminished several times, and ultimately disappeared, without leaving at the surface or on the shores any indication of their presence. The same result has been obtained several times by putting in a closed bottle water containing some of the green scum collected in Reservoir No. 3; after a short time, one or two days, a portion of the green matter, of a light shade, sinks to the bottom; examined under the microscope it proves to be composed of one kind only of algæ, presenting the appearance of strings of single cells; the rest, of a darker green, containing plants of a rounded shape, floats on the surface for a few days, and then sinks; before the end of one week all the vegetable matter is at the bottom, where its color changes from day to day from green to brown. Almost from the beginning of the experiment, the contents of the bottle emit an extremely offensive odor of decomposition, which increases as the plants decay and lose gradually their characteristic form."

Of the formation of the algæ, or of its cause, little is known, but it is remarkable that they appear suddenly in large quantities. Shallow flowage, it is said, favors their development, probably on account of the higher temperature which the water attains in such conditions when heated by the sun; but they are formed very rapidly also in deep water. I have observed several times that large quantities appeared in a very short time, equally distributed through hundreds of millions of gallons of water 20 feet deep, several hundred feet from any shore, and in very calm weather; the quantity of algæ in the water, as observed in Basin No. 3, seemed to vary constantly, increasing or diminishing with wonderful rapidity.

I have some reason to believe, from the statements of various persons, that the same kind of vegetation has been observed in years past in this vicinity; but in the absence of reliable evidence on the subject, some doubt may exist about the identity of the vegetable formations.

It has been suggested that the development of the algæ in the Mystic supply may have been favored, if not caused, by the presence of the sewage matter which finds its way into it; but the algæ have been observed also, in less quantity, however, in ponds which are entirely free from such pollutions. It is particularly remarkable that no vegetable growth of the same kind has been seen in the Abajonna river or its mill ponds, although they receive some very objectionable drainage, at the time that the other branch of the Mystic supply, the Horn pond branch, has been full of it. This fact is so much more noticeable, because Horn pond, where the algæ have been found in great abundance, is very deep generally, and presents a small proportion of shallow flowage. A somewhat similar condition of things has been observed in Framingham, where algæ have been growing in Basin No. 3, a deep, artificial reservoir built on the Stony Brook branch of Sudbury river, while Basin No. 2, on the other branch of the river, and of less depth, has been entirely free from vegetable growth.

The accompanying diagram, Plate LXXXVI, shows simultaneous observations made in October, November and December. It does not seem that the degree of the humidity of the atmosphere, or the barometric pressure, has any marked effect on the algæ; but their formation appears to follow the temperature, especially the temperature of the water, increasing and diminishing with it. The scale for the diagram showing the amount of algæ found in the water indicates only proportional quantities obtained by daily observations. To determine these quantities the water was examined at certain given stations, each representing a portion of the total area; and samples procured at each station, at top, middle and bottom of the water, were compared to a constant standard. Coefficients proportional to the areas represented by each station, and to the degree of turbidity of each sample as appreciated by the eye, were then used to establish the daily returns. These results may not be

strictly correct, but it is believed that they represent very fairly the condition of the water.

Although screens fail to free the water from the algæ, the latter can be retained on filter-beds constructed in the usual manner; when they are abundant, however, they form in a short time an almost impervious covering over the filtering surfaces. To filter large quantities of water under these conditions would require very extensive filtering areas and frequent cleanings, at a great expense.

A small experimental filter, one yard square, made of gravel and sand, and constructed in the well-known manner recommended for water supplies, showed that, in operating on water containing a moderate quantity of algæ, the filtering capacity of the apparatus was reduced in eight days from sixty gallons per twenty-four hours and per square foot to seventeen gallons; another time it was reduced in the same number of days from seventy-five to fourteen gallons.

The sand used being rather too fine, the filter was made over again, with well-washed sand, from which all fine particles had been excluded by using a wire sieve of twenty-four meshes to the inch; in this case the filtering capacity was reduced in eleven days from one hundred and nine to twenty-three gallons per square foot and per twenty-four hours. The table below shows the daily results of the experiments.

NUMBER OF GALLONS FILTERED PER SQUARE FOOT AND PER TWENTY-FOUR HOURS.

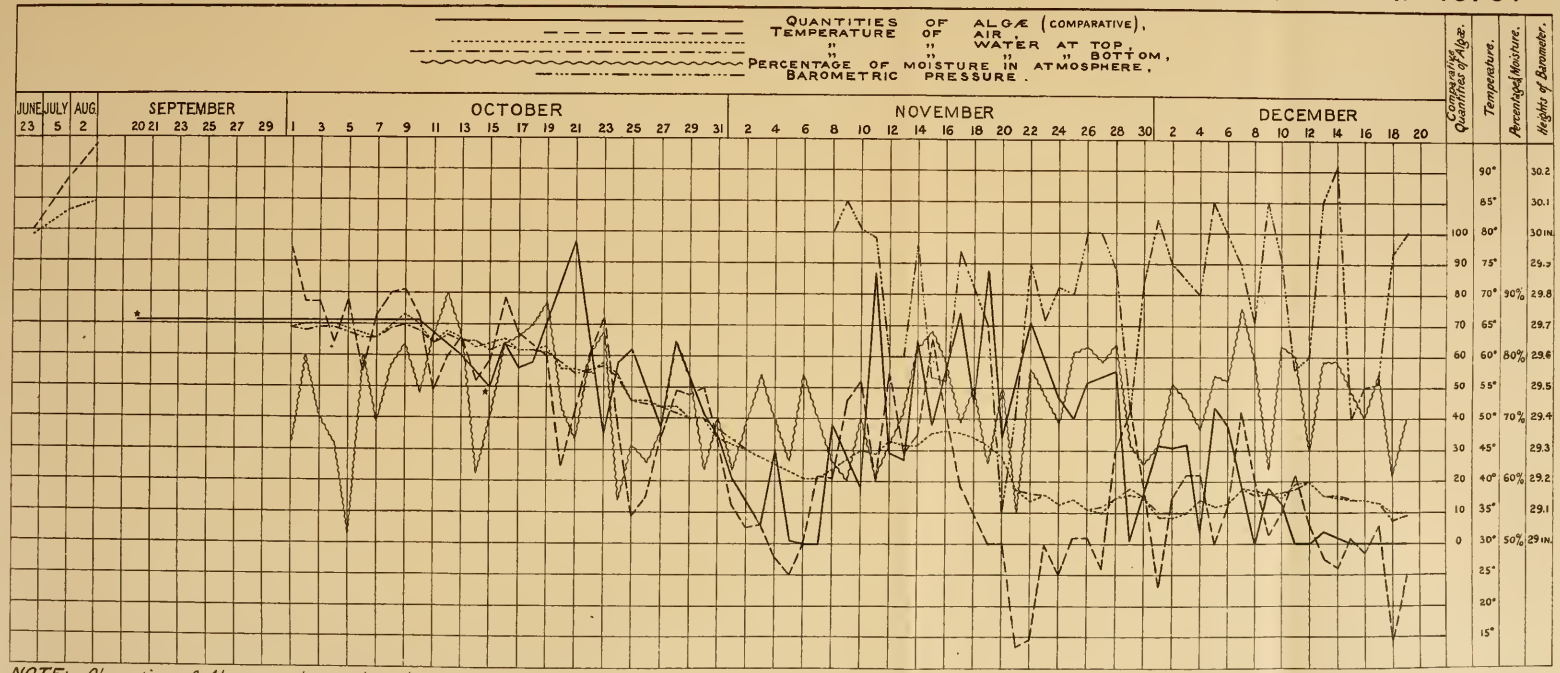
	First experiment. Gallons.	Second experiment. Gallons.	Third experiment. Gallons.
First day	60	75.1	109
Second day	50	19.2	109
Third day	53.3	18.5	120
Fourth day	40	17	80
Fifth day	19.17	16	80
Sixth day	18.50	15.5	65
Seventh day	16	15.5	54
Eighth day	17	14.1	42.8
Ninth day	.....	.....	34.3
Tenth day	.....	.....	26.6
Eleventh day	.....	.....	22.8

At the rate of the last experiment, it would require 233 000 square feet of filtering surface to filter ten million gallons per day, and each filter bed would need cleaning once a week.

After each experiment there was found on the surface of the filter a slimy, greenish matter, presenting the peculiar musty odor of the algæ; and the surface, when dry, became so much hardened by its mixture with the sediment, that, to the touch, it closely resembled slightly frozen sand. The sand did not appear to be penetrated by the vege-

ALGÆ IN STORAGE RESERVOIRS.

DIAGRAM SHOWING RESULTS OF OBSERVATIONS IN BASIN 3, FRAMINGHAM, MASS. IN 1878.



NOTE: *Observations of Algæ are only approximate between the signs* ★·····★

J.P.G.

table deposit; a slight scraping of its surface showed it very free from foreign matter.

Professor Leeds referred also to Professor Nichol's examination of Horn pond in Woburn, Mass., in 1873 or 1874. It that case the appearance of the pond was very different from what I have seen elsewhere; the stench arising from the water, the characteristic "pig-pen" odor was such as to drive away some of the inhabitants from the shore, and the surface was covered with a thick, slimy deposit, looking like animal matter in decomposition. This idea was encouraged by the fact that several tanneries were then draining into the pond; but the surface slime was recognized as decomposed algæ, and after a short time it disappeared, leaving the water in its ordinary condition.

The origin of the bad tastes and odors in public water supplies is, at best, obscure; and in many cases it is probable that they have been admitted as due to causes which were not sufficiently studied.

In 1881 the water in Farm pond, near Boston, became exceptionally bad in taste; the trouble became so serious, owing to the fact that all the supply from Sudbury river had to pass through that sheet of water, that the pond was emptied, and in a few days an artificial ditch, over 4 500 feet long, was constructed, partly in the sandy bottom of the pond, partly in a timber flume supported by sheet piling, of sufficient capacity to convey independently from the pond a maximum of forty million gallons. Professor Remsen, of Baltimore, was called to investigate the matter, and much credit is due him for having discovered in the waters of the pond a species of sponge, the presence of which had not before been suspected in the water supply. This *Spongilla* was declared to be the cause of all the trouble. Without being able to refute absolutely that opinion, I believe that the conclusion arrived at is too sweeping in its character. Farm pond is about 200 acres in area, and, at that time, the water contained in it was constantly changing to some extent, owing to the fact that a large proportion of the city supply was flowing through it; when it was emptied, the *Spongilla* was collected from a large proportion of the bottom which became exposed, but the whole of it would not have filled a bushel basket. It is difficult to suppose that this small quantity could have contaminated to such an extent so large a volume of water. Moreover, after collecting a bucket full of the *Spongilla*, I had it washed several times in well water, and although it was allowed to remain in that water for a considerable time, the bad taste was not reproduced. The same *Spongilla*, when again immersed in pond water, very soon resumed the old taste to such an intensity as to suggest a remarkable power of absorption.

As I am on the subject of Farm pond, I desire to state that when the construction of a permanent aqueduct across it was first spoken of, J. P. Davis, M. Am. Soc. C. E., then City Engineer of Boston, suggested the idea of adding to it an apparatus for forcing air through water. The

suggestion was not carried out, but may properly be mentioned in connection with the remarks on the same subject by previous speakers.

In the course of the discussion on Mr. Rafter's paper, a distinction has been made between natura and artificial reservoirs of water. It is possible that the necessities of a water supply, by producing rapid changes in the volume of water contained, and excessive fluctuations of the water mark, may place artificial reservoirs in exceptional conditions, favorable to the development of some of the evils complained of, but it is most probable that the differences between the two classes of sources of supply are sometimes imaginary, from the fact that artificial reservoirs are more closely observed, and that the slightest deterioration of their waters is at once necessarily discovered. I recall hearing loud complaints from people living in the vicinity of Farm pond (already mentioned) as to the deterioration of its water since it had become a part of the water supply of Boston; while an investigation disclosed the facts, well known to the oldest inhabitants, that the surface was, previously, sometimes covered with patches of green stuff which were then supposed to come from an accumulation of the pollen fallen from pine trees, and that the water was sometimes in such a condition of "fomentation" that when going fishing the thirst was better quenched with something stronger than the bad tasting pond water.

An allusion has been made in the course of these remarks to the possible trouble to result from the growth of algæ in the very extensive reservoir contemplated by the City of New York in connection with its new aqueduct in the valley of the Croton river. The opposition to the building of that reservoir was so great from some quarters that the Aqueduct Commissioners requested the Board of Health to give them its views in regard to the probable sanitary condition of the proposed reservoir. The board and its advisers investigated the question with care, and reported that the great depth of the lake would be conducive to the purity of the water. I fail to see that the chances for the formation of algæ will be more numerous in the future than at present, and this growth depends so much on local conditions that a comparison with the present reservoir is probably the safest ground on which to base an opinion. The present Croton reservoir is a comparatively small, shallow sheet of water, subject to numerous fluctuations which expose during the summer large areas of land, thus producing conditions favorable to the growth and development of algæ; its shores present at times a very objectionable appearance when the water is low; and yet for more than forty years it has furnished to New York a very acceptable supply superior to that of some other cities. The advantage with the same local conditions is in favor of the larger and deeper reservoir.

As to the advisability of removing the loam and all perishable matters from its area before construction, it is clearly out of the range of practicability, although some opponents of the project have freely

given their opinion that other sites for reservoirs could be easily procured by additional excavations to the tune of a goodly number of billion of gallons (five million cubic yards per billion gallons)!

It is certainly better, when within practical limits, to remove the loam from the surface of reservoir grounds near the water mark, although experience shows that inside of a very few years after flowage nature produces that result within the limits of fluctuation, except on flat grounds; but a general removal of the top soil is not to be advised.

Once in my experience, at a time when the public were under the influence of excitement produced by the temporary deterioration of the water supply, a large expenditure of several hundred thousand dollars was incurred in order to secure a perfectly clean bottom for a large reservoir. The principle was good, but although this occurrence took place a few years ago, I have failed to hear that the results are such as to justify the great outlay; the bottom, however, was so thoroughly denuded that although the foundations of the dam are very deep and well established, a large loss of water is produced by the general percolation through the ground.

As mountain streams cannot be secured for all communities, we must rest satisfied with what we can procure in many instances, *i. e.*, surface water, and turn our efforts toward the improvement of our water supplies. Before thinking of radical remedies, however, we ought to know better the causes of the evils from which we are suffering. We are in possession of the results of a large amount of very valuable research on the subject, and able men are constantly at work at it, but it seems that our efforts are not sufficiently concerted. Observations made at one spot under certain circumstances would acquire much more value if they could be compared with similar observations conducted elsewhere, and many individual efforts are wasted for want of the connecting links in the chain of evidence which could be furnished by others.

The results already obtained in this line of investigation by the Massachusetts State Board of Health in its efforts to make a rational and connected study of the question of water supplies throughout their State, indicate clearly what additional advantage could be obtained from extending the common field of research.

The subject is an important one to those whose fate it has been to wrestle with these questions in the face of the discontent, and even of the indignation of his fellow-citizens. It is a vital one and fit to be considered by this Society. I consequently suggest that a special committee be appointed for the purpose of corresponding with those who are most interested in the question of water supply for cities and towns, and of ascertaining whether it would be possible to join in a systematic study of these matters under concerted action.

Charles B. Brush, M. Am. Soc. C. E.—I have had some experience in this matter. First, in relation to the water of natural ponds. When

I was in the Adirondacks I took samples of water from the Great Lakes at a depth of about 60 feet and had them analyzed. I found that the quality of the water was not equal to the water we are supplying at Hoboken.

As to the remedy for algæ, I can only tell you what I have been doing. Our supply is rather complicated. We furnish water to nearly a dozen cities and towns of small dimensions as compared to New York, but they illustrate the difficulties which have to be met. I have found that the less I store the water, the quicker I deliver it to the consumer, the better is the supply. If I can only have a storage sufficient at any point for proper head and for relief in case of fire, then I have the most efficient storage for that particular town. We have a reservoir of some sixteen million gallons, and from that reservoir we supply, when the pumps are not working, all the little towns depending upon us. My practice during the past few years has been to put a tank on a suitable summit in each town and have an automatic arrangement so that the tank is always kept full. In such a case I have no trouble with algæ. One town had a reservoir built before the present company owned the works, and during eight or ten years there was at times complaint in this town as to the quality of the water from algæ. Since we have put in a tank and abandoned the reservoir there has been no further trouble.

I believe the reservoir was too large. For a time I supplied through the by-pass. While the water was rising in the reservoir there were no complaints, but they were renewed if there was a draft on the reservoir, especially if the water had been stored in the reservoir for any length of time.

The peculiar thing about algæ is that if a consumer tastes them once he imagines he is tasting them for several weeks afterward. On two or three occasions when complaint had been made I went to the town and found that the water was all right. I determined then to find out whether it was true that the water was bad in certain parts of the town and good in other parts of the same town. I gave directions as soon as the next complaint was received to have an examination of the town made and to locate the bad taste. I found after a little inquiry that the inspector simply took the word of the consumer. When the next complaint was made I immediately went up and took the inspector's report as he made it for the day and went to those houses myself and tried the water. In every case where they said the water was bad, I found that it was good. Then I called the consumer and asked him if he could detect any smell or taste in the water. He said he could not. When I asked why he had reported it as bad, he said, "Well, it was bad two weeks ago."

Now the first remedy, it seems to me, is to keep the water in motion. The next remedy that I found is to treat the reservoir purely and simply as an aquarium—to keep in it a proper equilibrium of animal and vegetable life. I have stored my reservoirs with carp, and after the

carp had gone, with black bass. I believe that in a reservoir, as in an aquarium, by having a proper proportion of animal and vegetable life, the water will be clean where otherwise it would be foul.

Mr. Fteley has spoken of the bitter prejudice with which this subject is treated. That is something in which I have had some experience. The Mayor of our city called public meetings, and I was informed that I was expected to explain why the water was bad, and to remedy it or suffer the consequences. Instead of considering the subject as a matter which was incident to water supplies generally, and which would have to be examined in a scientific way, it was treated with intemperance and ignorance. The prevailing theory at the first meeting was that the taste came from the cement that was used in the brick lining of the reservoir, and that the acids coming from the cement were the cause. Each meeting had its own peculiar advocate, equally intelligent and equally earnest.

It was absolutely necessary that we should do something; the algæ were there, and although I personally drank that water all through that period, and although I found that it did not injure me, still it was a most disagreeable condition of affairs. An expedient which was exceedingly simple was suggested to me by Dr. Leeds. We commenced to aerate the water, and we have been aerating it ever since. We did it first by simply pumping air into the mains. We have had no difficulty with water taken from the mains. Perhaps this was due to the fact that the water was in motion. Our consumers liked it, and we continued to aerate the water. We found that on certain days the algæ would be on the surface of the reservoir, not to the same extent as before, but to a certain extent. We then commenced to aerate the reservoir, and have been aerating it for the last two years. Since that time we have had no algæ. At times I have stopped when I have thought that there was a possibility of algæ coming in the water, and have let them accumulate. I then have turned on the air from the artificial springs, and the algæ disappear.

William E. Worthen, Past President Am. Soc. C. E.—Where do they go?

Mr. Brush.—I think they go to the air; I do not think they go to the bottom.

Mr. Worthen.—There is a difference of opinion about that.

Mr. Brush.—I do not care where they go so long as they go. I soon found that by keeping the water in motion I could keep it in better condition. As an experiment, during our trouble, I shut the water off from the reservoir. I wanted to see what it would do. Professor Leeds and I would go up and watch it. It was in bad condition for nearly two weeks, I think, and then it was bad enough. It had passed from the green scum to the bluish appearance, which is the most offensive condition. But after it had gone through its curing process it did not smell,

and there was nothing offensive about it. Some purging process is common to all bodies of water.

As to the question of diagnosis, I am a homœopathist myself, and have been all my life, and I care more for symptoms than for a diagnosis. When the symptoms appear I want to know what are the remedies. The remedies, I consider, are, keep the water in motion, keep the vegetable and animal life in proper equilibrium, and aerate the water. If there are any other remedies that do not injure the water I am anxious to know them.

Mr. Fteley.—In 1874 or 1875, when the City of Boston was looking around for a new water supply, a number of doctors—Dr. Edward S. Wood, Dr. Swan and Dr. Bowditch—were appointed to examine the waters of the various streams and lakes which had been indicated as best to serve as a supply for Boston. They went around and made an examination, and among the experiments they made was one that should be quoted in connection with Mr. Brush's remarks. They took some of the water, which was of a brownish tinge, water coming from a stream which flowed from a peat meadow, and in order to see what aeration and motion would do, they attached a bottle of it to the ceiling of their laboratory and let it fall, drop by drop, into a vessel on the floor. The change in the color of the water was very small.

Professor Leeds.—Mr. President, it surprises me to learn that a sample of water, after falling through the air, was not worse than when it started. I had occasion recently in some experiments on peaty water to filter water twice in the same manner. The amount of nitrogen was increased by dropping, drop by drop, through the air. It was filtered through a carefully cleansed filter of ignited quartz sand, and the increase of albuminoid ammonia in the filtrate was attributed to dust and impurities taken up by the water in falling through the air.

Mr. Fteley.—I was speaking of color and taste only, not of quality.

Emil Kuichling, M. Am. Soc. C. E.—Mr. Rafter's paper is one of much interest, not only to water works engineers, but also to hygienists and chemists, and every such carefully prepared contribution to the limited stock of exact knowledge on the subject which is now available in scientific literature should meet with a hearty welcome. Disagreeable tastes and odors are of common occurrence in public water supplies, especially in those cases where the source is impounded surface water; but while this fact is well-recognized, little attention seems to have been given to ascertaining the precise cause of such tastes and odors in the great majority of cases, and the public appears to be contented with the general assurance from physicians that no serious consequences to health have ever been observed to follow the free use of such water.

It must, however, be remembered that in absolutely pure water no form of life can exist for long periods of time, since the elements of

nutrition are then absent; hence the presence of life in a water indicates that the same is not entirely pure, and therefore it may be asserted that the amount and quality of living matter in a water-supply can be regarded as a measure of its wholesomeness. In the paper under consideration the discussion is nominally limited to algæ, although the author has introduced several species which botanists recently class with fungi and bacteria; but as the dividing lines between these three classes of thallophytic vegetation are not sharply defined, a greater scope is generally allowed to the first-named term so as to include both of the latter. To admit of a somewhat wider range in the debate, however, it is to be regretted that the author has not alluded to his researches in connection with the presence of *Zoophytes* and microscopic animal life in potable water, since *Spongilla*, even in small quantity, is known to have communicated an offensive taste and odor to large ponds and reservoirs.

As stated in the paper, the aquatic plants capable of producing disagreeable tastes and odors in both the water and the atmosphere above it are by no means limited to some of the colorless, bluish, or bluish-green microscopical varieties, but may belong to the grass-green families and even to other orders than algæ. Much depends upon the stage of growth of the plant whether it will develop such offensive qualities, and doubtless also the kind and amount of nutrition contained in the water play an important part in this respect. In the course of my examinations of certain shallow ponds in this State (New York) several species of coarse, flowering water weeds were found which emitted a nauseous odor when removed from the water, and which was highly intensified upon being crushed between the fingers. To make sure that this was not due to the presence of minute parasitic algæ, samples were submitted to a thorough washing and then to a microscopical examination, but, aside from a few diatoms, no other plants were found attached, and on crushing.the specimen the same characteristic odor was perceived as before. It was therefore concluded that the odor emanated from the said coarse weeds directly.

Between the temperature and the development of these peculiar tastes and odors in a water supply, a certain relation is generally believed to exist, depending, however, upon the species of the vegetation. Sometimes the development occurs in winter and sometimes in summer, but so far as has been learned from the reports, the species of plants producing it in different seasons are not the same. At Rochester, N. Y., the water supply from Hemlock lake was thus affected during the warm season in 1876 and during the cold season in 1888; in the former case, the cause was ascribed by Professor Lattimore, of the University of Rochester, to some obscure condition of a variety of minute algæ, without specifying any particular species, while in the latter, the author and his associates lay the blame upon *Volvox*, as set forth in the excellent paper. It should be remarked that in the list of algæ given in Pro-

fessor Lattimore's report on the condition of the water in 1876, *Volvox* was not mentioned. In other localities where surface waters are used as a source of supply, the difficulty is said to be regularly experienced as soon as the water in the reservoirs acquires a temperature of about 70 degrees Fahr., thus indicating that the same cause is in operation.

With a rise in temperature to above 45 degrees Fahr., the common bacteria found in all surface waters begin to increase rapidly, and generally attain their maximum rate of development when the water reaches its warmest state in open reservoirs, provided that the necessary quality of nutrition is available. A marked reduction of temperature, however, kills off many varieties after they have reached a certain growth, and in view of the enormous numbers of such organisms often found in this kind of water, it may be possible that the offensive taste and odor sometimes result from their decay. It is needless to say that the ordinary water bacteria here referred to are not recognized as being in any sense dangerous to health, and must not be confounded with the disease-breeding varieties; but as several eminent bacteriologists have recently advanced the proposition that, in general, the quality of a potable water stands in inverse ratio to the number of bacteria contained therein, the subject is mentioned in the hope that it will lead to further investigations in this direction.

The theory of Mr. Rafter that the final cause of the odors and tastes in a water supply must be sought in certain secretions of the algæ at different stages of their growth, is very ingenious and plausible, and opens the door to a new line of inquiry, analogous to that relating to ptomaines. The putrefaction of gelatinous and starchy liquids is attended with a peculiarly nauseous odor, and sulphur will form a variety of unstable and volatile compounds with some of the other elements contained in the water or the plant; as to the oily matter thus secreted, little concerning its character appears to be as yet known. It is to be hoped that on the completion of his investigations Mr. Rafter will kindly communicate the results to the Society in another paper.

In conclusion, it may be remarked that, inasmuch as the author has fully set forth the essential facts relating to the most interesting cases of the contamination of a public water supply by algæ, which I have had an opportunity of studying personally, and as reference to other kindred pollution is not in order, little further remains for me to say in the premises.

Dr. J. Edw. Line.—The statement credited to Mr. Fteley by the late Professor Nichols, that common salt added to water having a given vegetable taste develops therein at times a decidedly oily flavor, suggests a few observations pertinent to the discussion.

The dissociative effect of common salt is seen in the readiness with which the oily matter of the skin, also its superficial epithelial cells, are removed in the bath; in the separation from the teeth and neighboring

soft parts of salivary and mucous deposits and particles of food, thereby rendering the oral secretions miscible in water; in the separation of the globules of milk and the disks of blood; in the loosening up from each other of the fibers of such tough animal tissues as the cornea, sclerotica, tendons, fasciæ; in the disintegration of masses of vegetable matter floating in water; in effecting the discharge of the contents of decayed, broken or otherwise ruptured vegetable cells, as in the algæ.

This dissociative effect, studied microscopically, seems to be due to the induced shrinkage of the cells and fibrous components of the mass, whether animal or vegetable.

This shrinkage is toward a common center, the contour making a closer approach to modifications of the sphere than normal. Angular bodies lose their angularity, and bodies that are oblong shorten and thicken, as may be especially remarked in the oblong-celled algæ. In assuming the forms in question, the cells, fibers and filaments are torn asunder, and part permanently with their inherent tendency to aggregation.

With this dissociation we have large increase of surface upon which oxidizing and other reducing agents act to such a degree that there is marked intensification of taste or odor, or both, where before they were hardly or not at all perceptible. Briefly, the intensity of the taste or odor, or both, of masses of animal or vegetable matter is directly as the surface exposed to the reducing agent.

In the case in question, assuming the oily flavor to have had its origin in the oily matter found in the cells of certain algæ (*Vaucheria*, for example), the application of a solution of sodium chloride would disintegrate the masses, dissociate its elements, thus liberating the oily matter that had escaped from cells mechanically ruptured, or whose investment had been destroyed by decay, causing the cells to empty themselves of their contents into the surrounding medium.

This is easily demonstrated microscopically. In the case of *Vaucheria*, the oil globules may be observed in the cells and between the filaments, and on the addition of sodium chloride they are seen to escape from beneath the filaments and through the holes and fissures of the cells, suggesting an interchange between the medium in which the mass is floating and the denser contents of the cells, its oily and granular matter participating in and indicating the direction of the current.

Discussion at Convention.

Geo. W. Rafter, M. Am. Soc. C. E.—The paper on fresh water algæ has already been read at the regular meeting of the Society of May 1st, and I will therefore speak very briefly at this time. I desire to say that this matter has been with me, in the fullest sense of the word, a leisure-hour work. I have been in the predicament of having to do a day's work every day, and then if I cared to amuse myself in the evening of working at this question of the algæ. The result is, there are a few things I have found out and many that I hope to find out. The paper must be taken, not as indicating complete data as to the algæ in relation to the purity of water supplies, but, rather as a series of surmises, of which I have introduced only those appearing the most probable. My facts, of course, so far as I have introduced facts into the paper, I think are reliable.

I found at the beginning of this little side study that it was exceedingly difficult to get any information about fresh water algæ from the sanitary point of view. Scientific people have studied the question almost exclusively from the classificatory point of view, so that including the forms which are properly classified as fresh water algæ both in this country and abroad, there are catalogued, illustrated and described at least ten thousand forms.* Of the greater number of these, absolutely nothing is known from the point of view which we are now considering; the life history of many is still entirely unsettled. The field of work is, therefore, undoubtedly very large, and there are probably many questions of development which will bear on the final solution.

I also found, on consulting the limited literature of the subject, the opinion quite general that only a few species were likely to cause trouble, whereas the real fact as stated in the body of the paper is that the number is large. That these troubles usually occur in the summer or fall is due chiefly to the fact that the latter part of summer and the fall is the fruiting period with a considerable number of species, the period of complete development of the process of reproduction, and it is inferred that in many cases the objectionable tastes and odors are associated with the reproductive period, and while I am unable to prove the proposition, I am nevertheless quite certain that in some cases these tastes and odors are really the result of the reproductive process purely, and are entirely independent of the question of decay. In other cases it is equally certain that decay is the sole cause of the trouble, while in a third class, inasmuch as decay frequently follows closely on fruiting, it will be found that both are concerned in the trouble.

The question of ground water supplies has become a prominent one in this country and is likely to become still more so in the future.

It is common experience that there are parties going about the

* This includes the desmids and diatoms, the diatoms being by far the greater number.

country who, while without scientific attainments, are, as a matter of business, advocating the superiority of ground water supplies for any and all locations, the object of this advocacy being of course the sale of patented devices for obtaining such a supply. I wish to call the attention of these gentlemen, particularly, to the untruthfulness of their stock statement, that no growths of any sort can occur in ground water supplies. Undoubtedly, supplies of a high degree of purity can be obtained in many instances from the ground, but its advocates should remember that such a supply is not only essentially experimental but that it almost invariably imposes troubles peculiar to itself, which must be met and remedied frequently by considerable expenditure. This has happened both at Berlin and Jamestown, the two cases cited, while Professor Leeds has called attention in a paper before the New York Academy of Science * to the expense incurred at Greenwood Cemetery, because of offensive growths in water from driven wells. We are therefore sufficiently advanced in accurate knowledge of this particular phase of our subject to affirm with a considerable degree of positiveness (*a*) that a covered reservoir is always necessary in connection with ground water supplies;† (*b*) that such a reservoir will prevent algal growths provided *Crenothrix* is not present, and (*c*) if *Crenothrix* is present there is as yet no known remedy except to send the water to the consumer immediately on taking it from the ground or else to resort to expensive sand filtration or possibly artificial aeration, and inasmuch as it is frequently impossible to send the supply to the consumer at once, it follows that a ground water supply *may* impose the expense not only of covered reservoirs but of either artificial aeration or filter heads as well. Nevertheless, I should not hesitate, as an engineer, to recommend a ground water supply in any place where a decent consideration of all sources clearly indicated a minimum total expenditure for the ground water supply, questions of quality also receiving due consideration. My only object in introducing this phase of the question at all is to indicate to the advocates of the various driven well and other similar projects that they have hardly given the matter sufficiently broad consideration.

In this connection, I may say that I am not yet satisfied that artificial aeration will prove efficacious in every place, although the arguments which Professor Leeds has given elsewhere ‡ are strong and deserving

* The American System of Water Purification, by Professor Albert R. Leeds, Ph.D. Reprint, New York, 1887, pp. 11–12.

† I of course do not overlook the fact that ground water supplies may be constructed without any reservoir at all, *i. e.*, direct pressure, but such construction usually implies expenditure in excess of cost of reservoirs, either in additional first cost of plant or in capitalized cost of expense of operation, so that it may be affirmed that the cases are exceptional in which a rational design for a ground water supply would not include either a reservoir, standpipe or elevated tank of some description; at any rate the assumption made is sufficient for the purposes of the present argument.

‡ The American System of Water Purification, and Reports on Present and Future Water Supply of Philadelphia, in the Annual Reports of the Philadelphia Water Department for 1884 and 1885; pages 353–381 of Report for 1884, and pages 379–400 in Report for 1885.

of very careful attention. We must not overlook, however, the fact that many of the algæ flourish best under what can be appropriately termed conditions of ideal natural aeration, as for instance, on rocks constantly wet from dashing spray, in running brooks and other similar locations, and these, too, are mostly forms which can be classed as aquatic rather than aerial. It is quite probable, therefore, that the case may arise in which artificial aeration will lead to increase of the difficulty which it is intended to remedy.

The opinion has been expressed in the course of the discussion that circulation will prevent the formation of the objectionable algæ. Upon this point as well as the preceding one I cite the following: In the latter part of April, 1889, I had occasion to examine a rapid running mountain stream in Central Pennsylvania, with reference to its value as the source of a water supply for one of the towns of that region. The stream is fed by springs flowing from the base of sandstone ridges, and topographically and geologically is an ideal water supply. An analysis made at the time by a Philadelphia chemist resulted as follows (results in parts per 100 000):

Solid matter upon evaporation to dryness	4.84
Silica (SiO_2)	1.72
Alumina (Al_2O_3) / Oxide of iron (Fe_2O_3)	0.19
Lime (CaO)	0.54
Magnesia (MgO)	0.25
Sulphuric acid (SO_3) in combination	0.29
Chlorine (Cl) in combination	0.50
Free ammonia	Trace
Albuminoid ammonia	0.005
Nitrogen in nitrates	0.068
Nitrogen in nitrites	0.0003

From the point at which it was proposed to erect a dam and form an impounding reservoir to head of stream was about one mile, in which distance the stream fell at least 150 feet. The feeding springs were few in number, and each contributed a considerable volume to the flow. On April 22d, every point along the stream where algæ could find any chance to grow was swarming with them, and in one dozen collections made at various points, all within one mile, the following were identified:

Spirogyra,	Vaucheria,
Zygnema,	Oedogonium,
Ulothrix (3 species),	Fragillaria (2 species),
Tetraspora,	Synedra.

All of these were in an active growing state, and a mill pond further down the stream was nearly overrun with them. In this stream, in which flowed ground water of great purity, at any rate a process of natural aeration was going on far superior to any artificial process that could be

devised, and nevertheless the algæ flourished abundantly at the same time. The water however was deliciously pleasant and I had no hesitation in recommending it as being a desirable source of supply.

The clew to the reason for a vigorous development of algæ in this stream is undoubtedly furnished by the chemical analysis, from which it is learned that these waters contained quite an appreciable amount of inorganic nitrogen, and this would act as a stimulant of algal life. In such cases we are compelled to admit, however, that the algæ are in no sense purifying agents, but are, on the contrary, the cause of some deterioration.

In the same vicinity a spring was visited from which the flow amounted to several hundred thousand gallons in twenty-four hours. Nevertheless, the margins were thickly coated with a vigorous growth of diatoms. The analysis of this water stood (results in parts per 100 000):

Solid matter upon evaporation to dryness	15.76
Silica (SiO_2)	1.30
Alumina (Al_2O_3) / Oxide of iron (Fe_2O_3)	1.10
Lime (CaO)	7.83
Magnesia (MgO)	0.54
Sulphuric acid (SO_3) in combination	0.07
Chlorine (Cl) in combination	0.93
Free ammonia	0.003
Albuminoid ammonia	0.003
Nitrogen from nitrates and nitrites	0.38

Relative to artificial aeration, therefore, it appears that we are warranted in concluding that benefit will probably follow its application, in case water needs to be washed free of an odor; we may also expect benefit in sewage contaminated waters where bacteria are materially assisting nitrification. The experience of Mr. Brush at Hoboken and Greenwood Cemetery further indicates that it will sometimes assist in getting rid of the algæ, but that it will certainly be efficacious in every possible case, is still an unsettled question.

J. James R. Croes, M. Am. Soc. C. E. (the Chairman).—In an introduction to the discussion of matters relating to the purity of public water supplies, a few remarks on the general subject of the nature of the investigations referred to in the paper under consideration may not be inappropriate for the information of those present who are not familiar with the subject. We are all aware that at certain times most water supplies drawn from reservoirs, whether natural or artificial, are subject to a species of disease, rendering the taste and odor of the water offensive for a time. The conditions under which this disease has made its appearance have been so various, so far at least as examination by unassisted vision and observation could disclose, that the causes of the disease have

remained obscure. Carefully framed theories, based upon the conditions found to exist in one case, have been completely overthrown by observation of other cases, where the same apparent conditions existed, but the results were entirely different.

Chemical analysis alone also failed utterly to discover the causes of the disease.

In the general progress of microscopical and chemical investigation during the last few years it has become evident that the influence of the infinitesimal and to the unassisted eye, invisible, forms of animal and vegetable life with which nature abounds, on those organisms which from their greater size we look upon as the superior ones, is far greater than had been supposed. The generation, growth and decay of microscopic animals and plants have been found to produce, or at any rate to be coincident with, certain phenomena which previously had been supposed to be contingent upon or caused by chemical conditions for the variations in which no adequate cause could be assigned. The microscopic investigators of animal forms are striving to determine what species of bacteria are certain to produce injurious effects on human beings when taken into the system.

So the investigators of vegetable forms are seeking for evidence as to plants, which can be proven to cause certain unwholesome or unpleasant effects on the human race, and then how the growth of such species can be prevented, or at least the ill effects of their perfection and decay can be obviated. It is to this branch of the subject that the author of the paper now before us has devoted his attention.

By comparison of observations in many different places at different times, it is considered to be almost certainly proved that the unpleasant taste and odor referred to is to be attributed to algæ in the waters. These algæ are defined as flowerless plants, having no proper distinction of stem or leaf and always growing in water. They are generally thread-like structures of a more or less slimy consistency. Many of them are mere masses of jelly. They are divided into three great families, which are distinguished by their color, very much as races of human beings are, and as in the human race, there are also marked structural peculiarities attending families and habitat.

The nomenclature of the various classes, genera, species and individuals, is in that curious combination of mutilated Greek and Latin which is adopted by scientists, especially by microscopists. The long names which, at first, are somewhat appalling to the student of the subject are found on a little investigation to be derived generally from the Greek, and to denote some physical peculiarity of the class or individual, or to commemorate the person or the place by whom or where it was first discovered. Thus, the classes, *Cyanophyceæ*, *Rhodophyceæ*, and *Chlorophyceæ*, mean simply, respectively, blue, red and green seaweeds. The diatoms are composed of single cells w th walls of silex (*Gr.* διατεμνω,

to cut in two) while the desmids have no silicious walls, but are grouped together (*Gr.* δεσμη, a group) and the joints connected.

Of the subdivisions and individuals, there may be mentioned the *Schizosporeæ*, indicating plants whose spores are splinter like, the *Hydrodictyon*, which is Greek for a water-net, and the *Chrenothrix*, which signifies a water-hair, from its filamentous appearance, and its suffix, *Kühniana*, tells us that Dr. Kühn first described and individualized it.

Among the most curious of the origins of names is that of the *Confervoids*, which dates back to the time of Pliny, who called the water-sponge of that day *Conferva*, because it was said to possess the power to heal (Lat. *conferveo*) broken bones. The *Clathrocystis*, which Mr. Rafter mentions, means simply a closed bladder, while the *Cœlosphœrium* is a hollow sphere. But while this branch of the study may be interesting to some, it does not add to our knowledge of the habits of the plants, and I will ask one of the most acute and careful of the observers of these phenomena to give us some of the results of his studies, and will call on Mr. William E. Worthen.

William E. Worthen, Past President Am. Soc. C. E.—Most engineers connected with the water supplies of towns and cities have had their attention called to the presence of fresh water algæ in stored water, have had some experience in removing them, and have studied chemical and biological literature on the subject, and have generally come to the conclusion that in themselves algæ are neither dangerous to life nor detrimental to health, but that in life they are objectionable in color, and dying they give offense in odor and taste to the water.

In 1869 I put in the water works for Long Island City; the supply was from a well 50 feet in diameter, paved at the bottom with wall stone, with open pipes driven some 20 or 30 feet lower. On one occasion, before the well was covered, on a very bright still summer day, the surface of the water was covered almost instantly with a coat of brownish algæ, which continued nearly all day, but a fresh breeze sprung up, the sky became cloudy, and they suddenly disappeared. It seemed to me that they detached themselves from the rock paving, rose, and as suddenly sunk and again covered the rocks. The well was soon after domed over, and I have no record of the reappearance of the algæ. The algæ bruised in the hands gave no positive smell, analyzed chemically, no different constituents from former analyses of the water.

I had a similar experience with the spring in the ditch at Fort Wadsworth, S. I., from which the water supply of the fort is taken, the covering of the well stopped the floating algæ.

The supply of East New York is from wells, with an open reservoir of some three million gallons, for use in cases of fire or accident; here the algæ gave a distinct green tinge to the water, but without any offense in taste or odor. From some source the water flea came in, and it was

objectionable, as it was visible, but they were interesting as a study, and I kept them in a bottle for some weeks. Lime did not affect them, except as it precipitated their pabulum. The algæ grew and then faded out gradually. The same flea I have found in an underground reservoir which received its water from roofs.

In many large wells or filtering reservoirs, exposed to air and light, I have seen beautiful developments of algæ, even with the temperature of water at 52 degrees. My idea is that they supply oxygen to the water in which well water is deficient. But in all still waters exposed to air and light there are always more or less algæ, but without offense in taste or odor, if in a healthy and live condition.

Professor William Ripley Nichols says (Report on Cochituate Water, October 1st, 1887): "The peculiar tastes and odors are often noticed where the animalcules are comparatively scarce;" Mr. Burgess, in the sub-report: "The entomostraca are scarce in our water supply this season;" in allusion to which I suggested to the Professor that it was the funeral of these mites that the water-takers were attending. That they are objectionable to sight when living, and to taste and smell when dead, there is no doubt, but what are we going to do about it?

Can Water be Disinfected?

A Committee of the American Health Association (Vol. XIII, 1887) states absolute disinfectants to be but few. 1st, fire, burning, boiling, baking; 2d, chlorine. None applicable to a domestic water supply except boiling in small quantities and drank without contact with air, water or ice; but even boiling does not destroy the active principles of poisonous mussels nor the poison of typhoid fever.

Freezing will not always destroy fish life; my friend, Mr. A. J. Rossi, froze gold fish stiff in a cake of ice, which, melted out slowly, regained their former activity. And ferments continued to give off bubbles whilst ice cakes were being formed.

M. Melsons ("Annales des Genie Civil," May, 1870) filled a bomb shell with water and yeast and burst the shell by freezing, without destroying the yeast, showing that its vitality would resist both cold and immense pressure.

The development of germs may be checked by freezing, but under proper conditions they regain their activity. Dr. Cohn, of Breslau, states that a single bacterium would be able to fill up the whole ocean with its progeny in less than three days if only sufficient food and proper temperature were given. It is evident from all experience that it is impossible to annihilate all the germs and spores.

In the Minutes of the Proceedings of the Institution of Civil Engineers, July, 1886, will be found a communication on Water Purification, with an extended discussion on the same, which is so far conclusive against the possibility of removing entirely in any way these micro-organisms,

or even checking their growth, except temporarily. Filtration, except with great care, may increase rather than diminish the number of such organisms, and filtered water unless used at once becomes more offensive than the unfiltered; filtered water for ship's use was a failure. At Poughkeepsie, at certain times in summer, water must be used directly from the river.

The purification of water by agitation with solid particles, as coke, charcoal, chalk, or by precipitation, is only temporary in its character; the organisms carried down soon become redistributed.

In view of the universality of these micro-organisms and the impossibility of their destruction or removal, is there any necessity of either? Not certainly as to their danger to life or health.

Professor William Ripley Nichols, in "Water Supply:" "Their presence is not a sign of contamination, as they occur in natural ponds removed from all polluting influences."

Dr. Klein: "Septic organisms are absolutely inimical to pathogenic organisms, and not only are they inimical, but so much stronger, and their vitality so much greater, that it is not possible for disease germs to exist in their presence."

M. Bechamp ("Annales des Genie Civil," January, 1870): "M. Pasteur denies that sound wines contain living ferments, that those ferments only which spoil wine are living. I, on the contrary, say that both are living and capable of acting physiologically to produce chemical actions known under the name of fermentation. I am convinced that the cause which gives age to wine and occasions rapid transformation is not the destruction by a degree of heat beyond the capacity of endurance, but is an exaggeration of their function or direction in a determinate way. I believe that the whole secret of the art of giving age to wines and preventing them from spoiling consists in favoring the production of beneficent organisms."

Dr. H. P. Walcott (Public Health Association Address, 1886): "There is need of careful observation and experiment on the side of the life history of these organisms in natural conditions, not under the artificial arrangement of sterilized chambers, but in the presence of the uncounted superfluities of life ever present in the atmosphere we breathe, in the water we drink and in the ground beneath our feet. The phenomena of microscopic life are not likely to be found essentially different from those observed by the agriculturist."

With all due respect for chemical analyses, for I have them made of waters proposed for town supply, I investigate biologically the living visible fauna and flora of such waters and their surroundings and conditions of health. For many years I have been collecting magazine scraps on aquaria and in this line would refer to the Reports of Commissioner Thomas B. Ferguson, Paris Universal Exposition of 1878, as most valuable, and affording hints by which the condition of the water

may be judged and how the growth of forms and life may be modified and controlled.

The great principle to secure healthy water is to follow the teachings of the aquaria, keep up the circulation of the water and aerate it, favor the production of beneficent organisms and preserve the balance of animal and vegetable life. Death in its natural course by absorption of micro-organisms into those of a higher growth is inoffensive, and without a microscope we can judge of these last, we know a healthy plant or fish and feel assured that the water is good that promotes healthy organisms; "all vegetable aquatus, mostly microscopic, are requisite for the continuous oxygenation of water, and possessing a solubility much greater than that of the oxygen drawn from the atmosphere;" but there may be life in excess of what can be absorbed in the higher grade; study to keep down excesses in all forms looking largely to air and light to preserve the balance.

For reservoirs for well waters I recommend those of small capacity, only such as may be sufficient for a night supply or for contingencies of fires or the breakage of machinery. For reservoirs of streams make the distributing ones small and let the water flow down to them from the storage reservoirs, through open and rippling channels, keep up the circulation and give good chance of aeration.

Make the storage reservoirs ample and numerous, avoid the necessity as far as possible of changing the level by drawing it down largely. As far as they are drawn down keep the bottom clear of vegetation, as vegetation exposed rots and becomes offensive, contaminates the water.

When water has become offensive time is an element for the removal of the objectionable taste and odor by the oxidation of the effete matter, which will take place gradually in open reservoirs, but more rapidly by exposure to light and air in flowing streams with dams and ripples. Dilution may modify the offense, but a very slight taint makes water objectionable for domestic purposes, and boiling seems to intensify the offense. If there are a number of reservoirs, shut off the offensive ones for a time to give as long and broken flows to the water as possible from the reservoirs to the mains or closed aqueducts. Water sheds, reservoirs, streams, channels, aqueducts, mains, &c., in fact everything connected with a water supply should be under an intelligent supervision and management.

James B. Francis, Past President Am. Soc. C. E.—This matter of circulation is, I think, fundamental. My own house is supplied with water from a pipe connected with a reservoir containing, usually, rather less than two millions of gallons, the pipe serving both for supply and distribution, that is, the water in it is sometimes flowing to the reservoir and sometimes from it. The reservoir being small in proportion to the supply, the water in it is in constant circulation. My neighbors are supplied from a reservoir containing about thirty millions of gallons, and

complain, at times, of a bad taste in the water. I never have such trouble; both supplies are from the Merrimac River, and the only cause of the difference in the quality of the water, that I can discover, is in the difference in the activity of the circulation in the reservoirs.

Frederic Graff, Past President Am. Soc. C. E.—I am a firm believer in what my friend Mr. Worthen has said as to the value of circulation. I think that we may all see that water supplies taken from running rivers which have little or no pollution thrown into them from sewage are certainly much freer from any troubles of the kind indicated than water supplies taken from large impounding reservoirs where the water may stand in an almost stagnant condition for a long time.

In the utility of aeration, I agree with Mr. Brush. Of course, it is a very serious matter to do this on so large a scale as most of our cities now require, but when it can be done, I think it may be a cure for very many troubles.

I adopted it to a limited extent about the year 1872 in the Belmont Reservoir of the Philadelphia Water Works, by carrying the pumping main up vertically above the surface of the reservoir through the center of the division embankment, and then allowing the water to discharge over the lip of the main, which was enlarged to 39 inches diameter, forming a jet, so to speak, of that diameter, thence flowing into a basin 9 feet in diameter flowing over the circumference of that, and then passing into a reservoir with two overflow sides of about 20 feet long each, over these down the sloping brick-lined sides of the reservoir proper, until it reached the surface of the water therein, thus exposing all the water pumped into the reservoir through this main in thin sheets to the action of the air in its passage into the reservoir.

All the water supplied from that reservoir could not be thus treated, as only one of the pumping mains could be so arranged, but six millions of gallons passed into the reservoir daily in this manner. We never had trouble with bad water when it was in use. As far as I know, this was the first application of the overflow method of aeration used in this country.

I am a firm believer in aeration of water, and in methods of producing it by circulation, artificial or natural. I think that Rochester adopted a similar method of passing the supply from a storage reservoir into the distributing reservoir through jets, with somewhat the same object as I have spoken of.

Mr. Rafter.—Yes; the fact of the matter being that the water discharged from the conduit into the distributing reservoir is simply fed into it by jets. When we get to the limit of our supply we are not able to use the aeration scheme.

Mr. Graff.—I understand it is effective when in use.

Mr. Rafter.—Fairly so; yes.

Mr. Croes.—In drawing the water from the storage reservoirs in the Croton valley arrangements are made by which the water is aerated on leaving the reservoir; that is, it is discharged into the stream through a jet, thrown up into the air before it falls down into the stream.

Mr. Fteley.—There is such a thing in existence. I have seen the apparatus and I know it is used, but whether continuously or not I do not know. Mr. Cooper can tell us about that.

S. L. Cooper, M. Am. Soc. C. E.—The storage reservoirs in the Croton basin are provided with jets through which the water is drawn in the form of fountains.

The first year that the water was drawn from the Drewville reservoir, on the Middle branch of the Croton river, the gases set free at the fountains were so foul that remaining long in the neighborhood was very undesirable; and a watchman's house near by painted a light color was almost turned black by the gases.

After the water passed through the fountains, and had run down the stony bed of the Croton river for a few miles, the bad odor and taste had quite disappeared. This is a remarkable instance of the effect of thoroughly aerating water.

The trouble in the case was no doubt caused by the decomposition of the vegetable matter in the bed of the reservoir, which had not been removed before the reservoir was filled with water.

Mr. Croes.—At the outlets of the large artificial impounding reservoirs on the upper waters of the Croton, at Boyd's Corners on the West branch, and near Brewster's on the Middle branch, Mr. E. H. Tracy, then the Chief Engineer, had the outlet pipe carried some distance below the dam to a basin, the pipe terminating in a number of vertical nozzles, so that the water was thrown up in a number of jets when the reservoir was being drawn from. I understand that Mr. Cooper's statement of the effect produced, namely, that the bad water was aerated in that way and lost its taste in passing down the stream is quite correct.

Charles B. Brush, M. Am. Soc. C. E.—At the risk of some repetition of what I have said before in the discussion of this subject, I would like to call your attention to certain points.

In the first place, I think we have arrived at that stage where we can say that all water supplies are much better at some seasons of the year than at others, and that the diagrams we have here indicate the better and worse conditions of the water at different seasons and under different climatic conditions. Every river has its purging periods. On the supply of which I have charge, we have analyses of the water taken every month, both at the intake and in our distribution pipes and reservoir, and we find that we can plot a regular curve every year, showing that at certain seasons of the year the water is in better condition than at other seasons. The better seasons with us are in the fall and spring;

the worse mid-summer and mid-winter. The drainage area of the supply to which I refer is entirely free from all source of pollution in the shape of sewage. It contains no large towns and only a few scattered villages. The latter do not drain into our source of supply except by filtration and at a considerable distance from the river. The state of the water is due purely to natural causes. Those causes have produced a condition of affairs which was a very serious matter. In the year 1884 the algæ commenced to appear in our reservoir, in the shape of a fern-like appearance, which extended down below the surface about 5 or 6 inches. After a while this fern-like appearance commenced to grow thicker, until it took the form of a green paint on the surface of the water. At that time the smell and taste were very marked; they were so marked that the houses having this supply found it undesirable to use for domestic purposes. The green paint condition continued for a week or ten days, passing into a bluish color for another week, and soon after the water was perfectly good. It cured itself. During the trouble I found that the water entering the reservoir in motion was in a very much better condition than the water that remained in the reservoir, and therefore I shut the reservoir off and turned the water directly from the pumps into the city. I was exceedingly anxious to know whether that water was going to create sickness, and consequently, during the whole time these algæ were in operation I drank the water; it did not make me sick; I do not believe it will make any one sick. It is nauseating, and that is the worst feature of it. I think that the condition of water, in a case like that, shows its worst on the face of it; I do not think it will create anything like epidemic sickness.

Something had to be done. As soon as the condition of affairs that I have described began to take place, I called in Professor Leeds of the Stevens Institute. He is the chemist of the company, and he made analyses of the water during the whole time of the trouble. One thing that we noticed in connection with it, was that the supply of oxygen in solution was very low; it was one-third of 1 per cent. instead of two-thirds of 1 per cent , its normal condition. The thought occurred to Professor Leeds that aeration would be a good thing. We commenced aerating on a large scale. In the West they have been in the habit of having some kind of contrivance with a bucket on it, and when the water is in bad condition they churn it around, and in a little while the water will be perfectly clear. The best water is that which comes from mountain streams, and the nearer we can get our supply in that condition—broken up—the better it is. We forced fresh air in our pipes in 1884, and up to this time we have had no repetition of that trouble. I do not mention this as a universal cure-all, but I do say it has helped us out. We are now aerating the water in our reservoirs as well as in the pipes. The people see that air bubbling up in the reservoir. It looks like natural springs, and creates a favorable impression.

In some cases I have let a little of this green paint formation grow by stopping my aeration, and it has collected in a corner of the reservoir. When I turn on the air the algæ disappear.

Water becomes impure from natural conditions, from meteorological causes and local phases of animal and vegetable life. I believe with Mr. Worthen, that a reservoir should be treated as a large aquarium. I stock my reservoirs with carp and black bass, and with aeration I am able to keep the water in good condition; but whether that will always continue, or whether there will not conditions arise which the means I am now using will not be sufficient to counteract, I cannot say. It is perfectly evident that it is of the most vital importance to get at the bottom of this trouble. I do not believe that the algæ and the bacteria are the real difficulty. I believe that they are simply the results; that they are the indicators of the things which are wrong. I think that the attempt to physically remove algæ or bacteria is a mistake. I have tried that myself. When the green paint appeared on our reservoirs I was determined that our consumers should not see it; I had it carefully removed, but the next morning the green paint was all there. What I have tried to do is to cure the conditions of which the algæ and bacteria are the indicators.

Mr. Worthen.—Would you get rid of them entirely if you could?

Mr. Brush.—No; there are certain forms which indicate conditions which should not exist; but as for getting rid of animal and vegetable life altogether, no. I do not think we want for domestic supply water that is entirely pure. Distilled water is not desirable. I think any water that has been filtered is liable to deterioration very quickly; the mere fact of filtering the water takes the life away from it; if you are going to use it immediately, very well.

When a water supply is affected there is a popular excitement which is hardly understood except by those who pass through it. If consumers cannot get their meals and their baths when they want them, the people who supply the water have to encounter a storm which is not a very pleasant one. In this particular case of mine I was cited to appear at public meetings and given to understand that I would have an opportunity to explain matters, and if I could not do so satisfactorily the result would be on my own head. The excitement was very great. One man, who claimed to be a chemist, gave a long description of the troubles and the conclusion he came to was "that, in building the reservoir, cement had been used and that the cement in the water created the difficulty; if we had not used cement it would not have occurred; that I ought to have known better." Of course there are funny sides to a thing of this sort, but there are very serious sides also. Those who are weak and sick, and ladies and others who are easily frightened, immediately feel that the only thing to be done is to leave the city as soon as possible.

I think it is exceedingly important that there should be somebody to whom people could refer for information on the subject, and thus check the reckless statements which appear from time to time.

You have already heard from Mr. Rafter how carefully he has been looking into the matter. I, myself, was perfectly astonished to learn what they were doing in Massachusetts; their reports, I believe, have not yet been made, but next year they will be issued, and thus we will have the results of their labors.

Samuel Whinery, M. Am. Soc. C. E.—Mr. Brush alluded to a device that is in use in the West for aerating domestic water supplies and perhaps a few facts in relation to it may be of interest.

In many sections in the Mississippi Valley the supply of water for domestic purposes is collected from the roofs of buildings and stored in cisterns. These cisterns are practically small storage reservoirs, constructed by excavating a space under ground of the required form and capacity, lining the walls with brick and cement mortar, and covering the top with an arch or dome of the same material.

It is found to be a fact, notwithstanding that the water so stored is originally pure, and that the cisterns are frequently cleaned out, that this growth of algæ frequently occurs in them, rendering the water offensive to the taste and smell.

It is worthy of note, in this connection, that in these cases the development of this plant growth occurs, notwithstanding that these cisterns are covered over. It is found that if a pump is used in these cisterns that thoroughly aerates the water, this offensive odor and taste does not occur, and if such a pump is put in a cistern already offensive this trouble is soon remedied.

The most popular pump of this kind is a modification of the old fashioned chain pump, having for buckets small metallic vessels, holding about half a pint. The bottom of each bucket is perforated with a very small hole, and as the descending bucket enters the water in its inverted position it carries down with it into the water a volume of air, which gradually escapes in small bubbles, which rise to the surface, thus thoroughly aerating the water.

It is a very simple device, but has proved very effective, and cisterns provided with such pumps never become offensive from these growths.

Frederic P. Stearns, M. Am. Soc. C. E.—Mr. Rafter has referred to the experience with *Crenothrix* in the water supply of Berlin, and I think it may be of value to add the experience with this organism in the ground water supplies of Massachusetts.

There were in this State in 1887 fifty water supplies taken from the ground, which may be classified as follows:

Springs	16
Large wells	16
Tubular wells	7
Filter galleries	7
Filter basins	4
Total	50

The line of separation between the different classes is somewhat indefinite; but it is sufficiently exact for the purposes of this discussion.

When water comes from a natural spring or where it is filtered by some very slow process of natural filtration, the ground-water before exposure to the light is nearly free from nitrogenous organic matters, as is shown by the small amount of free and albuminoid ammonia found by chemical analysis. It is quite common to find about the following amounts of these constituents in parts per 100 000:

Free ammonia	.0002
Albuminoid ammonia	.0015

In ground-waters of this class *Crenothrix* has been found frequently in very small numbers, but it has never increased to such an extent as to give trouble, although it has been carefully watched for, particularly in districts where the ground contained much iron.

In several places in Massachusetts a filter gallery has been built on the shore of a pond or storage reservoir just at the high water line, or in some instances so that the gallery is buried to a depth of a few feet below the bottom of the pond or reservoir. In these cases the water is imperfectly filtered and contains after filtration a large amount of nitrogenous organic matter, the free ammonia being increased by the filtration and the albuminoid being decreased, as will be seen by the following typical case:

	Water from storage reservoir.	Water from filter gallery.
Free ammonia	.0004	.0206
Albuminoid ammonia	.0396	.0248

There are four instances of this kind in the State which have been carefully examined, and at each place *Crenothrix* grows in great abundance. In the worst case, during the hot weather in summer, the water contains so much of the *Crenothrix* coated with iron that it is very unsatisfactory for laundry purposes on account of the iron stains produced. The rapidity with which this organism multiplies as the water is passing through about two miles of pipe from the filter gallery to the village, is marvelous.

It will be seen from the foregoing that the experience in Massachusetts indicates that *Crenothrix* will not multiply in ground water, except

under special conditions which favor its growth, and these conditions appear to be produced when water is imperfectly filtered by continuous natural filtration. In view of this experience I have no hesitation in recommending ground water supplies in Massachusetts, where the conditions are favorable for obtaining a good supply, as being much less likely to cause trouble from the growth and decay of organisms than surface water supplies. The experience at Berlin and some other European cities should not, however, be lost sight of.

I am very much interested in Mr Rafter's paper, as it is an extremely valuable addition to our knowledge upon a subject concerning which our present knowledge is very limited.

Upon certain points we now have fairly definite information, as for instance the case already mentioned in this discussion, of *Crenothrix* developing in certain imperfectly filtered waters. We also know that ground water stored in an open reservoir will develop abundant growths of algæ, and that a portion of the inorganic nitrogen will be taken up by living organisms to the serious detriment of the quality of the water. It is further known that pure ground water stored in the dark will be practically free from these organisms and that its chemical constituents will remain unchanged.

When the case of the open storage reservoir is presented, it must be admitted that we have no definite knowledge based upon experience or well proved by scientific investigation which permits us to predict with a large degree of certainty the biological results to follow the construction of a storage reservoir, and we are equally unable to suggest a practical and sure remedy in cases where an existing reservoir is affected by growths of algæ.

Very large sums of money have been spent in the construction and improvement of reservoirs to prevent the growth of algæ, and in making these improvements theory has been the chief guide rather than scientific knowledge or experience.

This condition of affairs ought not to continue. The interests involved are large enough to warrant sufficient appropriations to keep a considerable force of biologists, chemists and engineers constantly employed on the solution of the problems presented.

Every isolated observation carefully made adds to the general fund of knowledge, but rapid advance cannot be expected without continuous observation.

I have here three diagrams, Plates LXXXVII, LXXXVIII, LXXXIX, prepared for another occasion, but which may be of interest in this discussion. These diagrams have been made from the results of monthly chemical analyses from June, 1887, to June, 1889, of the waters of two artificial storage reservoirs and a natural pond, all of them subject to abundant growths of algæ at certain seasons of the year. The particular feature of the chemical analysis indicated by the

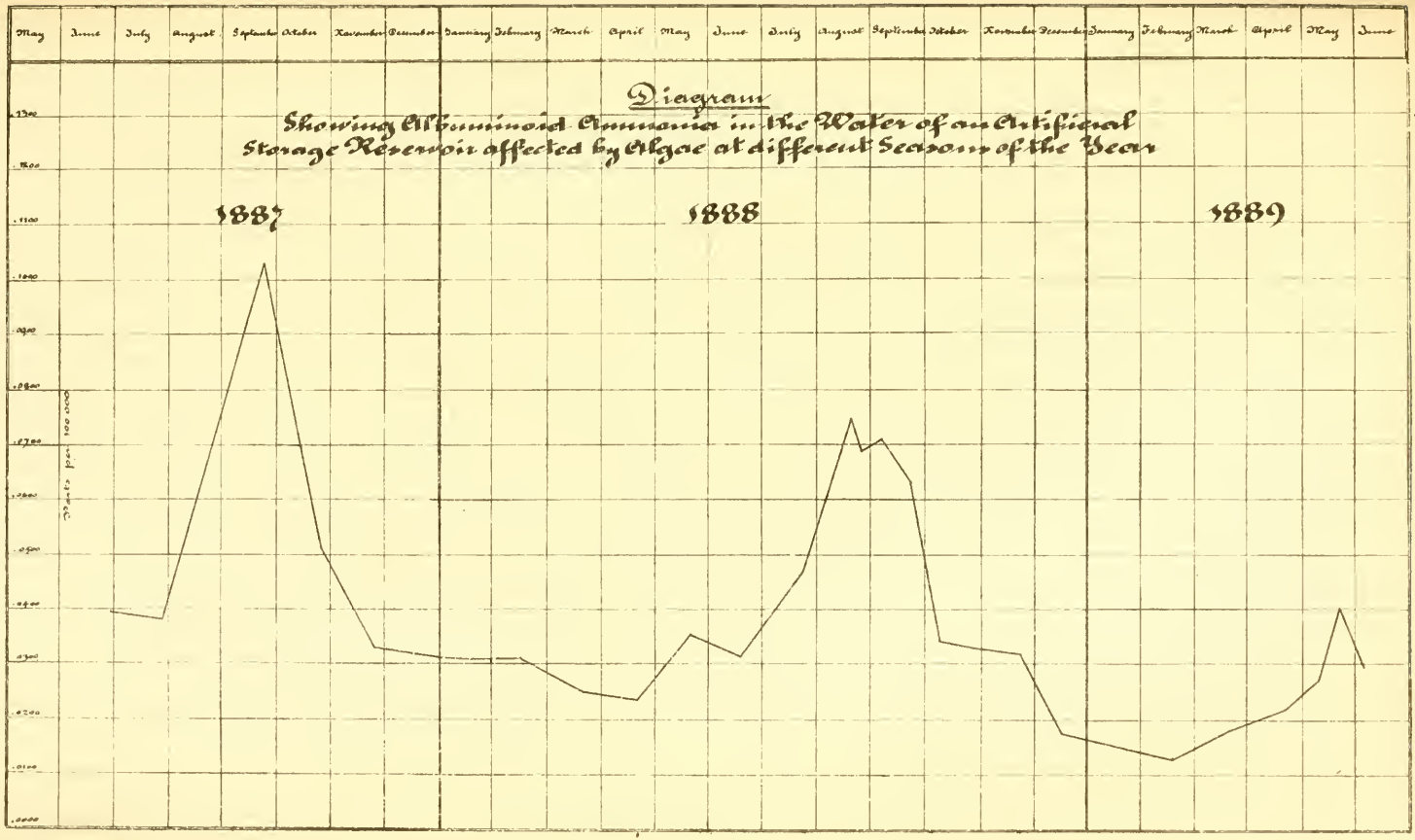
diagrams is the albuminoid ammonia, which may be considered in these cases as indicating approximately the amount of algæ in the water.

It will be observed that the first two diagrams exhibit the same general features : namely, a very marked rise in the amount of albuminoid ammonia between June and November, culminating in August or September. In addition to the two reservoirs to which these diagrams relate there is another not represented which is affected by the same summer trouble. With regard to the extent of the trouble, it may be said that it is so serious that the abandonment of reservoirs and mains, costing upwards of $1 000 000 has been seriously considered. In one case the water has been affected every year during the thirteen years that the works have been in operation. The immediate cause of the bad taste and odor of these waters is the presence of abundant growths of algæ of the blue-green group. The reason why these algæ are found so abundantly in these particular reservoirs is not apparent. In one case, at least, it cannot be attributed to the shallowness of the reservoir or to the pollution either artificially or naturally of the water entering the reservoir.

The third diagram shows very different characteristics from the others. In this case there is but one very marked rise in the amount of albuminoid ammonia in the two years, and this occurs in the spring of 1889. In June, 1887, the amount of albuminoid ammonia was quite large, and much larger than the following month, so it seems probable that the growth was abundant in the spring of 1887, and was disappearing when the examination of the water of the pond was begun. There was no marked rise in the spring of 1888. The trouble in this case is due to the growth of a minute plant which is entirely different from the blue-green algæ found in the other cases referred to. At the time when the trouble culminated this spring, the water was so filled with these organisms that it was entirely unfit for use.

The advantages of circulation have been referred to in this discussion, and I will say a few words about it.

The Town of Brookline, Mass., has in connection with its low service supply an open reservoir supplied by pumping from a filter gallery. It was found that water of satisfactory quality when pumped would become unsatisfactory when stored in the open reservoir, and that the more the water was made to circulate by the addition of fresh water from the filter gallery the worse it became. The rule was therefore adopted to pump sixteen hours per day at a rate but slightly in excess of the consumption during the same time, and to allow only the slight difference in quality between the water pumped and the consumption to go into or be drawn from the reservoir; moreover, circulation is avoided as far as possible by drawing water from the reservoir at the place where it enters. A special experiment was made by Mr. F. F. Forbes, the superintendent of these works, to determine the effect of


May
June
July
August
September
October
November
December
January
February
March
April
May
June
July
August
September
October
November
December
January
February
March
April
May
June
Diagram
Showing Albuminoid Ammonia in the Water of an Artificial
Storage Reservoir affected by Algae at different Seasons of the Year
1887
1888
1889
.1300
.1200
.1100
.1000
.0900
.0800
.0700
.0600
.0500
.0400
.0300
.0200
.0100
.0000
Parts per 100,000

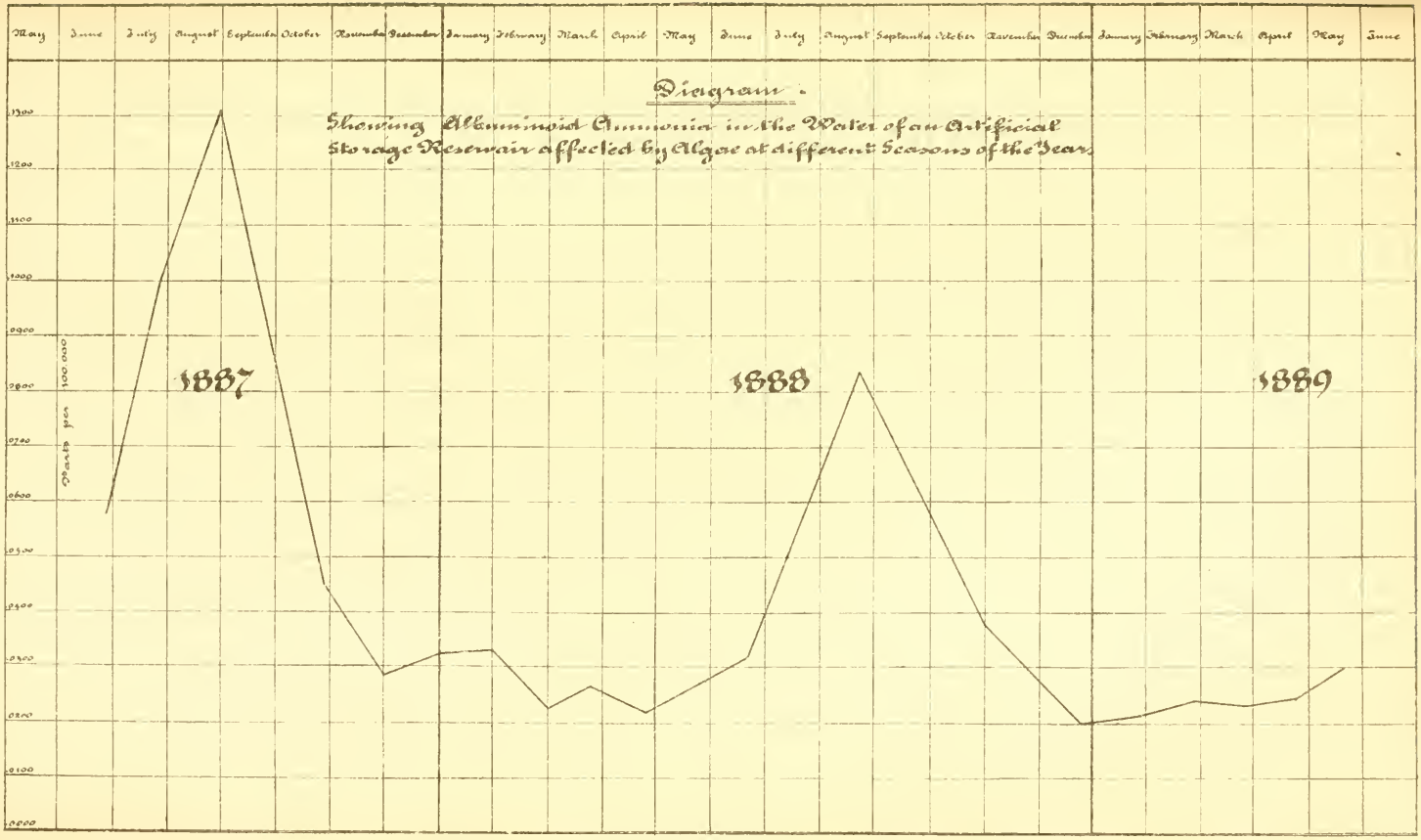


Diagram.
Showing Albuminoid Ammonia in the Water of an Artificial Storage Reservoir affected by Algae at different Seasons of the Year.
Parts per 100,000
1300
1200
1100
1000
0900
0800
0700
0600
0500
0400
0300
0200
0100
0000
1887
1888
1889
May
June
July
August
September
October
November
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January
February
March
April
May
June

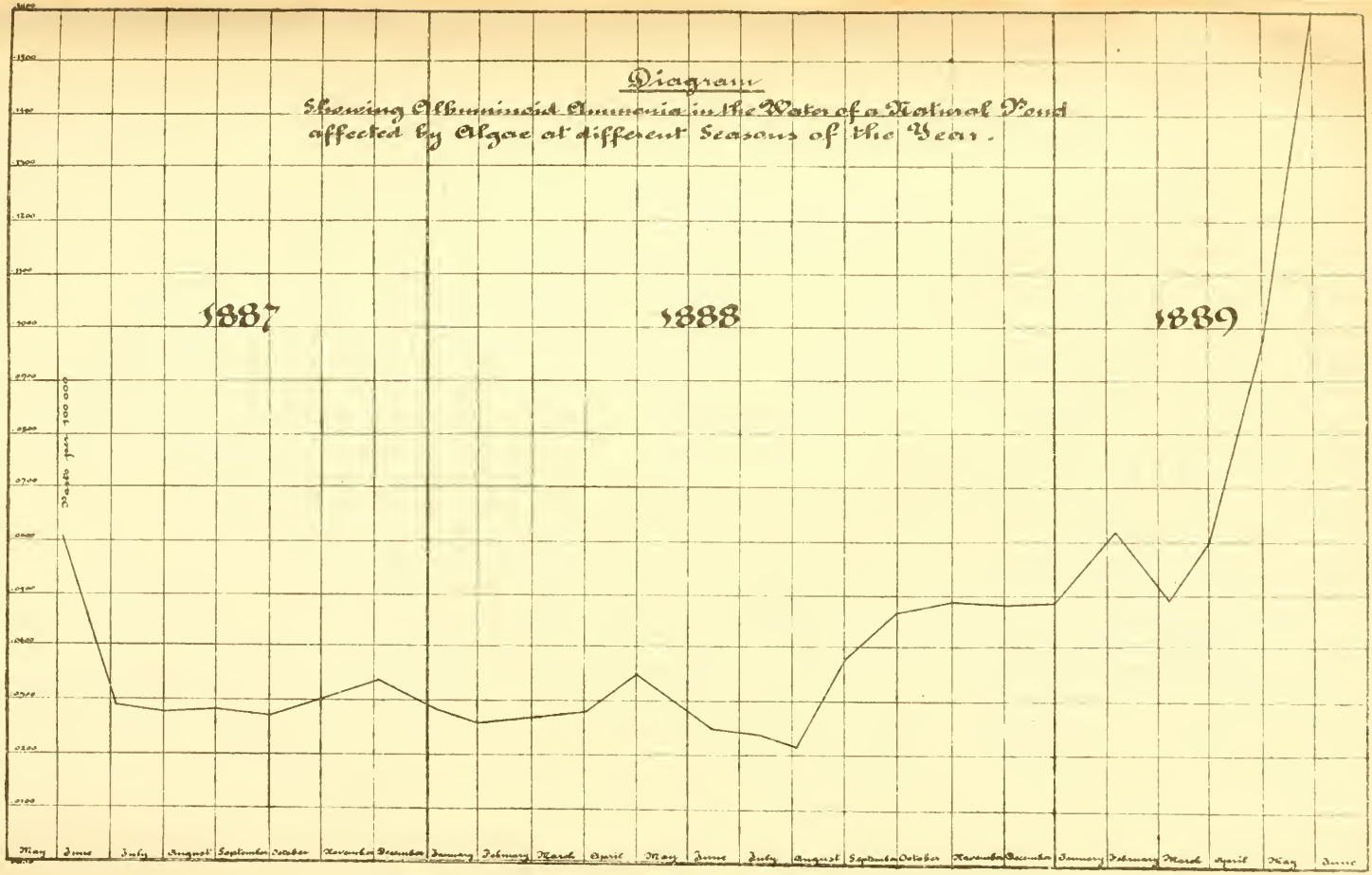
Diagram
Shewing Albuminoid Ammonia in the Water of a Natural Pond affected by Algae at different Seasons of the Year.
1887
1888
1889
parts per 100,000
May
June
July
August
September
October
November
December
January
February
March
April
May
June

adding fresh water. He first found that the water in the reservoir in its normal condition contained a certain number of organisms in a given quantity of water. The reservoir was next emptied, thoroughly cleaned, and filled with fresh water. The organisms, at first few, multiplied so rapidly that at the end of a week the water contained as many as before the reservoir was cleaned, and at the end of about three weeks it contained four or five times this number. After this time the number of organisms gradually reduced to the original number, but more than doubled when the reservoir was again partially drawn down and fresh water was added. Connected with these works is an iron tank supplying the high districts, having a diameter somewhat exceeding its height. This is said to have given more trouble than the large reservoir, and the superintendent attributed it to the fact that on account of its small size the water was necessarily changed in it quite frequently. The tank is now covered with a roof which excludes the light, and the water in it does not deteriorate. I do not wish these statements to be considered as an argument against the circulation of water in a reservoir under all conditions; but the facts stated show that circulation caused by the addition of fresh water containing food for the algæ will not prevent their growth.

Mr. Fteley.—It has been said that some very interesting experiments have been made in Massachussetts. It would be highly interesting to hear from Mr. Stearns the details of the work in which he has been engaged.

Mr. Stearns.—Three years ago an act was passed in Massachusetts, entitled "An Act to Protect the Purity of Inland Waters," and its execution was intrusted to the State Board of Health. The text of this act was published in the Transactions of this Society for January, 1888.

The primary object of this act was to regulate the disposal of sewage, so that the rivers and other inland waters should not be unnecessarily polluted. It is one of its requirements, however, that all cities and towns proposing to introduce a system of water supply or sewerage should submit their plans to the State Board of Health for its advice. It soon became apparent, from an examination of existing literature, that but little exact knowledge existed concerning the purification of sewage; and that there was much to be learned, locally and otherwise, with reference to the water supplies and inland waters of the State. For the next year an appropriation of $30 000 was asked for, with the view of establishing an experimental station for the purification of sewage, and of making a thorough examination of the water supplies of the State. This request and subsequent ones for $25 000 per year have been granted by the Legislature. As the subject of sewage disposal is not now under discussion, I will only say with reference to it that an experimental station was established at Lawrence, Mass., where experi-

ments upon sewage purification have been carried on for the past eighteen months under the supervision of Hiram F. Mills, C. E., member of the Massachussetts Board of Health. These experiments will probably add more to our scientific and practical knowledge of sewage disposal than any experiments which have ever been made.

Monthly examinations of all of the water supplies and many of the rivers of Massachussetts were begun in June, 1887, and these examinations of many supplies are still being made. The examinations are chiefly chemical and microscopical, though a few bacterial examinations have also been made. From this work much has been learned that will be of practical value in selecting sources and designing systems of water supply; but it is evident that there is an opportunity for a great deal of research in these matters before we shall know how to prevent bad tastes and odors in water stored in an open reservoir or pond.

The force engaged upon the examination of water supplies consists of six chemists, three engineers, and two biologists. A portion of this force, however, has other duties to attend to in connection with the work of the Board.

With regard to the importance of more information regarding the means of preventing the growth of algæ in water supplies, it may be said that out of about one hundred and thirty water supplies in Massachusetts, ten are seriously and almost chronically affected by bad tastes and odors due to such growths. Many other supplies are seriously affected at times. I have recently visited a pond in Massachusetts, which, fortunately, has not been used for a water supply, that has been afflicted for two years with an abundant growth of a dark jelly-like substance. This, at times, generally in warm weather, rises to the surface in patches as much as 10 feet in diameter and 2 inches thick, and is driven by the wind to the shore, where it accumulates in large quantities, and when heated by the sun is very offensive. The mass is composed chiefly of bacteria imbedded in jelly, but it also contains some algæ. I have mentioned this case, because it is apparently worse than any which have occurred in connection with water supplies, but I see no reason why the same trouble may not occur in any body of surface water used for a water supply. A possibility of this kind furnishes us with an additional incentive to acquire information upon these matters, which will enable us to undertake preventive or remedial measures with a knowledge of the results to be obtained.

Mr. Geo. W. Rafter.—In closing the discussion of the paper on fresh water algæ I may state that it was a matter of grave doubt with me as to whether the subject would be of interest to the Society, and I need hardly say that I am not only surprised at the interest manifested, but I am exceedingly grateful to the gentlemen who have, by taking part in the discussion, assisted very materially in the hammering process

which every idea must undergo before it attains the fixed form indicating complete evolution.

The remarks of Professor Leeds at the meeting of May 1st may first claim attention. Reference is made to the report of Dr. John Torrey upon the offensive condition of the Croton water in the summer of 1859, and Professor Leeds very justly ascribes great credit to Dr. Torrey for the clear views which he even at that relatively early day presented. I agree with Professor Leeds as to the value of Dr. Torrey's report. It has been in my possession for a number of years; and I have been in the habit of consulting it as the original American classic on this subject. (See Appendix, page 555.) Nevertheless, I cannot but conclude that we have made great advances in our knowledge since Dr. Torrey wrote in 1859. At that time there was absolutely no American work on cryptogamic botany in existence, and even the great work of Rabenhorst which, by reason of being written in the universal Latin language, speedily became the handy reference book of all students of the cryptogams, did not appear until five years later. In England, Hassall's work, a new edition of which had appeared two years previous, was then about the only available book,* and the American student of that day could hardly find a more satisfactory reference. If we consider, therefore, the paucity of the literature which even an eminent botanist like Dr. Torrey had at hand, it is not to be wondered at that he hardly compassed the whole question, and, at the same time, the clearness of his views in many particulars easily leads to the conclusion that he had given the subject as an independent proposition very careful consideration. Professor Leeds, however, without actually saying so, unintentionally leaves the impression that Dr. Torrey, at that time, clearly affirmed the oil globule as a cause of objectionable tastes and odors, but a cursory reading of what Dr. Torrey said will remove all misapprehension on this point.

Professor Leeds is right in assuming that the starch experimented with was not chemically pure, that it was in short ordinary commercial starch. As the result, however, of some farther trial I arrive at the conclusion that the failure of chemically pure starch to give the odors in question is a matter of little or no significance. We are not dealing with the starch as a finished chemical product, but rather with it as in process of formation; we are dealing in short with the chemical processes in the plant rather than with such processes in the laboratory. The formation of starch in the growing plant is a matter about which nothing positive is yet known farther than this, that while the completed starch grain is by itself a pure carbo-hydrate, it is, nevertheless, in some way a product of the nitrogenous protoplasm, and so intimately related to it that we do no violence to the present state of knowledge by taking the starch grain in process of formation as one of the typical causes of the class of

* "A History of the British Fresh Water Algæ," by Arthur H. Hassall, M.D., 1857.

difficulties under consideration. The question is one with more than theoretical interest, as will be sufficiently shown by considering that the production of starch in all the chlorophylaceous algæ is entirely dependent upon the quantity of light which these little plants receive. All of the free-floating forms are from a variety of causes, as, for instance, changes of temperature, cessation of the production of gas by the plants themselves, etc., quite susceptible to changes in specific gravity, and will, therefore, at different times, occupy different levels in the water. In light of less than a certain degree of intensity the starch is not formed, the protoplasmic matter, which, with sufficient intensity of light, would go to the production of starch, remains protoplasmic. Again, if algæ in which starch is fully formed are placed in the dark, or in light of less than the starch-producing degree of intensity, the starch already formed will disappear, such changes taking place as restore the starch material to its original state. On being again brought into strong light the starch will reform, and by treatment in a suitable culture cell, all these transformations can, under proper gradation of light, be easily observed for a considerable length of time.

The application to be made of these observations is in relation to the changes in intensity of the light which will exist at different depths in any given body of water, and consequently in relation to the varying quality of the water itself at different depths. In this connection it is important to clearly understand that the production of chlorophyl and starch is very intimately related to the chemical composition of water, and that if such conditions obtain as preclude the continuance of their formation marked changes in chemical composition will immediately result.

Professor Nichols made in 1879 and 1880 a series of chemical analyses of Mystic lake water at different depths, and the results show clearly the correctness of the foregoing proposition. (See Appendix, page 557.) They show farther that at both top and bottom the free ammonias attained their maxima in February, when the cold weather of winter had partially arrested the vital processes of the algæ, and that the free ammonia was lowest at that time of the year when the algæ are known to be in their most active state, namely, in October. We have only to consider the important office of the free ammonia as food for living plants to appreciate the value of these little plants as purifying agents.

Changes in quality of light as depth increases may result from two causes, or rather from a combination of two causes. There will always be either an increased opacity of the water itself, due to increase of coloring matter as the depth increases; or where the coloring matter is constant at all depths, the decrease in intensity of light will be in accordance with the general law that intensity decreases geometrically as distance increases arithmetically. By way of illustration, we may assume the opacity of a given body of water to be such as to cut off $\frac{1}{20}$ of the total intensity of light at the depth of one foot. We have then after

passing through one foot of such water $\frac{19}{20}$ of the original intensity at the surface. In passing through the second foot the light will again lose $\frac{1}{20}$ of the total quantity of light entering the second foot or $\frac{1}{20}$ of $\frac{19}{20}$. At the depth of two feet the total intensity is therefore $\frac{361}{400}$ of the original intensity, and so on for any depth whatever.

The remarks of Mr. Stearns bring into the discussion very forcibly the whole question of the relation of chemical constituents to organized life, and indeed open up a whole field of new discussion. Before entering upon this discussion of the subject we need to define clearly the relation of the plant life to the ammonias both free and albuminoid; thus, for instance, albuminoid ammonia in its relations to matters biological is the measure of the algal life present in the water at the time of making an analysis, while free ammonia is in the same way a measure of past algal life, of that which has existed at some former period, but which has decayed and passed beyond the state of organized life. If we make, therefore, a quantitative determination of the amorphous sediment present in all waters the results will probably run in some clear relation to the free ammonia. In the same way a quantitative determination of the life itself will show equally a clear relation to the albuminoid ammonia. This has been clearly apparent to me for some time, but in the absence of any method of making the quantitative biological determinations actual results could not be arrived at. Professor Wm. T. Sedgwick, of the Massachusetts Institute of Technology, has, however, recently published a method of making such determinations,* and improvements in that method by the present writer leave nothing to be desired. The quantity of life present in any given sample may now be determined with a considerable degree of precision, and if we gain nothing else by such examinations, the clearing up of our ideas as to just what the ammonias mean biologically is still a great advance.

The ammonia process has by common consent come to be regarded as the most satisfactory chemical determination of the sanitary value of potable water that has yet been devised, and its authors, working largely upon the polluted waters of London wells, fixed upon 0.015 parts in 100 000 as the highest permissible limit of albuminoid ammonia; but with a clear idea of just what the albuminoid ammonia means, this arbitrary standard is shown to be utterly misleading. Professor Wanklyn states in the preface to his last edition (October, 1888) that he has used the ammonia process for twenty-one years, and it is indeed astonishing that the biological side of the question has never presented itself to his mind, but a perusal of his last edition leads inevitably to the conclusion that it never has. Working largely on the polluted ground waters in the vicinity of London, he still affirms the original

* Paper on Recent Progress in Biological Water Analysis, in the Journal of the New England Water Works Association, September, 1889, by Wm. T. Sedgwick, Associate Professor of Biology, Massachusetts Institute of Technology.

standard the same as twenty years ago. This may be considered as merely the natural effect upon an individual of the environment, for equally eminent chemists differently situated have long since learned the inapplicability of this arbitrary limit, as a universal standard. Thus Professor Leeds, at the outset of his elaborate study of the Philadelphia water supply, found it necessary to prepare new standards, and as the result of several hundred analyses prepared the following, which he has designated as sanitary standards:*

Constituents Expressed in Parts per 100 000.	General Standard of Purity (for River Waters in the United States—Highest upper limits.)
Free ammonia	0.001 — 0.012
Albuminoid ammonia	0.01 — 0.028
Required oxygen as determined by permanganate	0.35 — 0.5
Required oxygen as determined by silver	?
Nitrous acid (HNO_2)	0.0001 — 0.001
Nitric acid (HNO_3)	0.35 — 0.5
Chlorine	0.3 — 1.0
Hardness	5.0 for soft—15.0 for hard.
Total solids	15 — 20
Oxygen dissolved per liter	?

Professor Leeds states in the same connection two principles which must be kept in mind in determining standards of purity, thus:

First.—The results of chemical analysis must confirm and explain the facts gathered by personal inspection.

Second.—There must be established for every source of water supply, whether cisterns, spring, well, lake or river, a standard of purity according to which the quality of the water must be judged.

That these two principles are literally true is becoming more and more apparent every day, and their clear enunciation in 1883 was a step in advance in our knowledge of the real significance of chemical analysis.

Professor Drown, working in the same direction, has shown that brown-colored waters require to be judged by a different standard from that applied to colorless waters.†

The field has thus considerably widened until at the present time we may affirm that a complete study of any given water supply should include *four* distinct examinations, which for present purposes are sufficiently indicated by the outline table on the opposite page.

* See a Preliminary Report of a Chemical Investigationin to the Present and Proposed Future Water Supply of Philadelphia, by Albert R. Leeds, Ph.D., in Annual Report of the Philadelphia Water Department for 1883, page 243.

† The Odor and Color of Surface Waters, by Thomas M. Drown, Professor of Analytical Chemistry in the Massachusetts Institute of Technology, etc., in the Journal of the New England Water Works Association for March, 1888, pages 2-29.

Environmental (1).	Including detailed statement of Topographical and Geological Conditions of Drainage Area, together with Observations on Extent and Character of Population and Industries of Region as Special Sources of Pollution, with study of Normal Samples by (2), (3) and (4).		
Physical (2).	Depth from which Sample is Taken.		
	Temperature.		
	Specific Gravity at the Actual Depth and Temperature.		
	Color and Odor.		
	Intensity of Light at Actual Depth.		
Chemical (3).	Albuminoid Ammonia.		
	Free Ammonia.		
	Nitrous Acid.		
	Nitric Acid.		
	Oxygen Required.		
	Chlorine.		
	Hardness.		
	Total Solids.		
Biological (4).	Chlorophyceæ.	Plants:	
	Cyamophyceæ.		
	Schizomycetes.		
	Animals.		
	Amorphous Organic Matter.		

The last column represents the formless material which makes a portion of the sediment, and for which as the most appropriate designation I have used the term *amorphous matter*. Zoogloæ has also been proposed, but I may suggest in reference to this term that it has already acquired a definite meaning; namely, a colony of putrefactive bacteria, imbedded in a mass of jelly. The amorphous matter, as representing organic matter, which has passed so far into the state of decay as to be without definite form, may or may not comply with the strict terms of this definition. In many cases it certainly does not, and in all such the application of zoogloæ would be inappropriate. At other times it would undoubtedly be scientific to say amorphous matter in the zoogloæ state; but the application of zoogloæ as a generic term for the phenomenon in question can hardly be considered permissible.

With a study of a series of samples carried out as here outlined, we could reasonably expect to gain information relative to a given water supply quite up to the present state of knowledge.

Professor Waller's discussion presents facts of very great interest. Thus, his observations in 1881, at the time referred to by Mr. Hyatt, that filtering the Croton water removed all of the free ammonia and nearly all the albuminoid ammonia was a great incentive to a clearing of somewhat misty views on my part, and I hope it may be equally prolific to others. Professor Waller is, however, mistaken in supposing that I absolutely deny the presence of oil in the *Cyanaphyceæ*. If he will kindly re-read the original paper he will see that the statement really is that starch and oil are not the most important characteristics, but that in the plants in question the mucilaginous jelly is a more important feature.

The last paragraph of Professor Waller's discussion demands momentary consideration. Thus, in regard to the ammonias, nitrites and nitrates, I may say that while these substances usually exist in potable waters in such minute quantities as to be, if just added, entirely harmless, nevertheless, the ammonias and nitrites, as representing organic matter in a state of change, are very intimately connected with any unwholesome conditions of the water. In the nitrates we may consider the organic nitrogen completely reduced to the inorganic form, and hence incapable of further change. In further extension of this part of the discussion I adopt the view of Professor Drown, that if the algæ take up and remove from the water organic substances capable of undergoing change, they are probably doing a good work, but we should not lose sight of the fact already referred to in a previous part of the discussion, that nitrates may themselves, under favorable conditions, become food for algæ and the direct cause of such growths in waters, which would be free from them if the nitrates were absent.

Since writing the original paper I have considerably extended my knowledge of the jelly-producing capacity of the diatoms, and as this is

undoubtedly a very important phase of the question, I will briefly record the results here.

On May 10th, 1889, I observed the sides of the masonry of inlet well and the rip-rap at Mt. Hope reservoir of the Rochester supply to be covered with a filamentous growth which, at that date, had a length of several inches. On examination it turned out to be principally two diatoms, *Encyonema* and *Cocconema*. The former is distinguished by a cymbiform frustule, arranged in longitudinal series within tubular filaments. The latter has a stipitate supporting frond with the frustule at the extreme end. Both were present in quantity, and the sub-membraneous filament of the one and the supporting frond of the other together made a considerable mass of nitrogenous material. On May 22nd and 23rd, reports were brought to the Water Works Department that Hemlock lake water, as delivered to the consumers, had suddenly acquired a strong fishy taste and odor, and further investigation showed that the moving filaments and fronds of two weeks before had contracted to the depth of one-half inch, and were only apparent as masses of compact jelly. Study at Rush reservoir and at Hemlock lake itself revealed the fact that a similar growth was present at both places, and after going over the lake and both reservoirs quite carefully, I estimated the total quantity of this compact jelly as at least 2 000 cubic yards. A careful study with the microscope showed that while the greater portion of this was *Encyonema* and *Cocconema*, there were also present in the jelly-mass *Fragillaria* (two species), *Cyclotella*, *Stephanodiscus*, *Asterionella*, *Synedra*, *Gomphonema* and *Pleurosigma Spencerii*.

Tests were also made of samples of water from different depths and upon the jelly itself, similar to those already described in the original paper in the account of *Volvox globator*, and no doubt remained as to the connection of the jelly with the fishy taste and odor.

The persistency of the fishy odor was well illustrated by accidentally spilling some hot water containing the jelly over my clothing, whereupon I carried about the smell of a fish-market for several hours.

On May 28th and 29th, very heavy wind prevailed for more than thirty-six hours, and at its cessation not a single vestige of the jelly-mass could be found at either lake or reservoirs. When, however, a few gallons of water from the lake itself were concentrated by filtering, into about 100 cc., it was found to contain a white, slightly flocculent sediment, whereas the same treatment previous to the heavy wind yielded nothing of the sort. Similar treatment of samples from the city distribution yielded a brown-colored sediment, evidently the same as that from the lake except in the matter of color. Samples from different depths in both reservoirs showed the same as that from the lake, *i. e.*, white-colored sediment, while those taken from the main conduit just before entering reservoirs, showed brown-colored, same as from city distribution, thus clearly showing the reducing effect upon the distributed jelly of agitation while passing through the mains.

This study was of further interest by reason of the light it threw upon the cases which have been cited by Mr. Fteley of tastes and odors apparently unaccompanied by the presence of any plant life. Had no knowledge been gained of the seat of the difficulty previous to the heavy winds of May 28th and 29th, there would have been absolutely no chain of circumstances by which to connect the fishy taste and odor with the rankly growing diatoms of May 10th, and I have no doubt but that an equally certain physical cause will be found in every such case if all the attendant circumstances can be got at.

On June 1st, heavy rains fell (+ 3 inches), and the consequent inflow to Hemlock lake was sufficient to immediately reduce the intensity of the fishy taste and odor by quite an appreciable amount, and finally about June 25th, it was found to have entirely disappeared.

Daily examinations were made by two observers during the whole time of the continuance of the fishy taste and odor, and one fact of considerable significance was clearly brought out, namely, the marked absence of all kinds of animal life. Hemlock lake water at the beginning of June usually abounds in certain rotifers and entomostracans, but at this time, except on the surface, they were almost entirely absent. The plant life was also quite low, less than usually observed, and it is matter of regret that no quantitative determinations were made. Had such a series been carried on both before, during and after the fishy taste and odor, we would be able to answer definitely many practical questions of which at present the solution can only be inferred.

What may be termed accidental experience during the last summer leads to the conclusion that this matter of the formation of jelly on a large scale by the diatoms is very common. Thus at Batavia, New York, in August, I found an area of several acres over which water enough flowed from a series of springs issuing from the base of a hill, to keep the ground partly free of vegetation. This area had growing upon it masses of diatomaceous jelly similar to that found at Hemlock lake and the Rochester reservoirs earlier in the season, and in sufficient quantity, if one cared to gather it, to fill many bushels.

Later in the season (October 15th) a similar growth was found at Canadice lake, different, however, in this, that while all the other jellys seen this year have been characterized by absence of animal life in the jelly itself, this Canadice specimen contained vast numbers of green infusoria, which had comfortably established themselves throughout all parts of the jelly.

The origin of the jelly mass is still involved in considerable obscurity, nothing like a complete explanation having yet been made. It is probable, however, that so far as relates to the diatoms the general statement of the Rev. William Smith, thirty-five years ago, that the *Diatomaceæ*, while possessing special features of their own are intimately allied to the other orders of the *Protophyta*, especially to the *Nostocaceæ*, in

the tendency shown by several genera to surround their frustules with frondose masses of mucus, is essentially correct.*

Among recent publications, the most complete observations on the origin of the gelatinous envelope of diatoms are those of Professor Hamilton L. Smith,† and his conclusion is probably true that the exceptional development of the enlarged sporangium, which is the usual accompaniment of extensive diatomaceous jelly masses, is merely a phase of reproduction, by which, after a long series of vegetative generations by division, there is in effect a rejuvenescence by conjugation, accompanied by a material increase in the size of the resulting primary generation over that of the parent cells, which uniting by conjugation, formed a new sporangium and its investing mucilaginous gelatine. From whence it happens that, after a long series of vegetative generations by division, the time arrives, in the due course of the life history, for reproduction by conjugation. If at such time the conditions are favorable for this taking place on a large scale, we will have in certain species in which this tendency to sporangial development is particularly pronounced, the formation of considerable masses of the mucilaginous gelatine. The basis of this view is stated by Professor Smith to have been first advanced by Braum in his "Rejuvenescence of Nature," in 1851. It is also held as true by McDonald and Dr. Greville, as cited by Professor Smith, and this consensus of distinguished opinion is entitled to the most careful attention.

The foregoing is merely a brief outline of a very small portion of Professor Smith's paper, and for the complete detail I must refer those sufficiently interested to pursue the matter further to the paper itself.

This question of the excretion of a protoplasmic gelatine either just before or during impregnation has been studied by A. Dodel-Port, who advances interesting views in a recent paper.‡

According to his view certain algæ always excrete protoplasm during the coalescence of the conjugating cells. In the higher types it would appear as if this excretion, which is usually from the female cell before impregnation, had simply for its object the formation of a nidus for the reception of the male fertilizing agent.

Professors Sachs and De Barry have both studied the origin and composition of the mucilaginous gelatine. De Barry, more particularly in relation to its connection with the development of the fungi and bacteria, while Sachs' studies are general in their character, including the vegetable mucilage, not only of phenogamous plants, but also of algæ, fungi and bacteria.

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*A Synopsis of the British *Diatomaceæ*, by the Rev. William Smith. London, 1853–56. Introduction to Vol. II, page 21.

† Paper—A Contribution to the Life History of the *Diatomaceæ*, Part II, by Professor H. L. Smith, in the Proceedings of the American Society of Microscopists, 1887, pages 126–167.

‡ Biologische Fragmente, by A. Dodel-Port, Part II. Cassel and Berlin, 1885. Also Bot. Centralbl., XXIV (1885), page 132. Also J. Roy, Micr. Soc., 1886, page 278.

Sach's latest views may be summarized as follows:* The change which results in the production of the gelatine may, under certain circumstances, occur as a diseased condition, while in other cases it appears as a normal change, serving definite purposes of life; and in the economy of the algæ and many of the fungi this change plays so important a part that the form and mode of life of such plants are to a certain extent determined by it.

De Barry's observations on the development and life-history of the fungi and bacteria lead to essentially the same conclusions.†

The views of both Sachs and De Barry are too elaborate to be gone into at length, and my present object in referring to them at all is merely to call attention to their works as throwing considerable light on the general subject.

A paper of this sort ought to include some discussion of the question of remedy. I do not, however, give that part of the subject special consideration at this time, chiefly because I am not fully prepared to make such thorough discussion as it demands. Many of the hints of the gentlemen who have taken part in the present discussion are valuable, and in some cases fully cover the ground, but for the more serious difficulties of the kind under consideration my present view is that sand filtration is the most efficient remedy that can be devised.

Mr. Fteley has suggested the appointment of a special committee of the Society for the purpose of corresponding with various water boards in order to ascertain whether it would be possible to join in a systematic study of these matters under concerted action. Such a study, if intelligently directed, would be productive of practical results of great value, and is clearly worthy the Society's attention.

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*Lectures on the Physiology of Plants, by Julius Von Sachs, Translated by Ward. Clarendon Press, 1887, pages 89–90.

†Comparative Morphology and Biology of the Fungi, Mycetozoa and Bacteria, by A. De Barry, Professor in the University of Strassburg, Translated by Garnsey. Clarendon Press, 1887, pages 9–12. 456–460.

APPENDIX.

The following is Dr. Torrey's Report:

New York, August 25th, 1859.

To Myndert Van Schaick, Thomas B. Tappan and A. W. Craven, Esqs., Croton Aqueduct Board:

Gentlemen,—At your request, and in company with Dr. Chilton, I visited Croton lake on the 18th inst., for the purpose of ascertaining, if possible, the cause of the disagreeable quality observed in the water distributed through the city, and which had been the subject of serious and general complaint for a week or two previous. On driving down the hill that leads to the dam, and before reaching the lake, we noticed the same musty odor that characterized the water of the hydrants in the city. Mr. Adamson, the gate-keeper, afforded us every facility in prosecuting our researches, and piloted us to various places on the lake. He brought to our notice a bright green substance that had appeared within the last twenty-four hours in the water near the dam. This material had been driven by a favorable wind to the outlet of the lake, where it accumulated so as to form a considerable stratum in quiet recesses near the shore. The water at the time was very low, only a small portion flowing over the dam. A quantity of the water, quite thick with the green material, had been reserved for our examination, and we had collected some of it ourselves from the surface of the lake. We soon became convinced that this unusual ingredient was the cause of the peculiar taste and odor of the water. After examining the character of the larger aquatic plants that grew abundantly in the western portion of the lake, and taking new samples of the water from various places, we returned to the city, and I commenced my examination immediately, before any obvious change had taken place in the properties of the water.

On placing a portion of it under a microscope, it was found to be filled with little straight filaments, which were composed of oblong roundish cells, in a single row like a string of beads, and in no case did I find these threads to be branching. Most of the cells were about one-third longer than broad, and were filled with a bright green substance, composed of irregular grains. This matter was proved to be chlorophyl, or the green coloring substance of leaves. Interposed here and there in the bead-like filaments were two other kinds of cells, the one perfectly spherical, three times the size of the oblong ones, and filled with green spores or seeds, by which the plant is reproduced; the other also spherical, but much smaller, and containing a yellowish fluid, but no green grains. The function of the latter is unknown to me.

There were only a few other kinds of microscopic plants in the water, nearly all of which belong to a particular tribe, called by botanists *Desmideæ*. In ordinary seasons these constitute the chief vegetable forms existing in the Croton. The number of living animalculæ was also unusually small. In repeated instances, on concentrating the water of the city hydrants, at the time that it exhibited the offensive properties complained of, I found its characteristics precisely those of the water taken at the dam. The day after collecting the sample of bright green water at the lake, I submitted it again to the microscope, and was surprised to find that all the beaded filaments had disappeared, but there was floating in the liquid, in a separate state, the larger spherical green cells and the smaller yellowish ones. I had noticed in my first examination several of the filaments break up by the successive bursting of the little

cells. Without doubt the myriads of little plants obtained in my sample had disappeared in the same way. I have since repeatedly seen the filament break up in water taken from the hydrants. They undergo dissolution much more quickly when they are confined in a bottle. After the rupture of the cells the water retains its green color for some time, but it finally becomes bleached from the decomposition of chlorophyl. The bright green water from the Croton dam became quite viscid in less than twenty-four hours, and the following day it was somewhat putrid, and emitted a little ammonia, but not the least trace of sulphureted hydrogen. Most of the spherical cells just noticed remained entire, but the green ones had evidently matured their spores or seeds.

Before the green water had decomposed, I evaporated a portion to dryness, with great care, as as to avoid scorching it. Sulphuric ether agitated with the residue became of a lively grass green tint, but without materially diminishing the color of the mass. The solution after spontaneous evaporation of the ether left a thick brownish green matter, which was resoluble in ether and alcohol but not in water. It was doubtless one of the coloring bodies of chlorophyl. A portion of the green water was then boiled for some time; it lost its color and odor, and deposited brownish flakes on cooling. The water that distilled over contained the odorous principle in a concentrated form.

From this examination, and from the researches of Dr. Chilton, I think we are warranted in concluding that the recent offensive condition of the Croton water was owing to a rapid and abundant growth o m croscopic conferva-like plant, which abounds in a volatile, odorous principle, soluble to some extent in water.

We were extremely fortunate in making our visit to the lake at just the time when a favorable wind drove the little plant (floating by adhering bubbles of oxygen gas) to the outlet; thus bringing within the space of a cubic inch as many of the filaments as could be separated by filtration from a hogshead of the water a short distance from the dam.

I have not yet satisfied myself as to the origin of these little filaments; whether they are an entire plant, or once constituted a part of a more complex alga. They are more minute than in any true conferva known to me, being over 2500th to 2000th of an inch in diameter, and from a 50th to the 20th of an inch long. I strongly suspect that they are derived from a species of the genus *Nostoc* of botanists. The genus usually consists of a globular or oblong vesicle, from the size of a buckshot to the bigness of a plum, and filled with mucus which is loaded with minute, bead-like filaments. When the little bladders burst the contents escape, the mucus dissolves in the water, and the filaments are set free. Sometimes the waters of small lakes are filled with these small bladders of *Nostoc*. Since examining the water I have had no opportunity of revisiting the lake to verify my conjecture.

The question naturally arises, why this plant should have made its appearance in such quantities, and not have been noticed before? We can only reply that the case is not a singular one. Even in the higher order of plants, it is a common circumstance for a particular species to abound at one time, and then almost disappear for years; and in the lower vegetable tribes, especially those which inhabit the water, is this strikingly the case. The present summer has been unusually favorable to vegetation of all kinds; but what are the circumstances which have so remarkably multiplied this little alga I have not yet determined. It is a plant of short duration, and should it reappear another season, the probabilty is that it will not annoy us long. Very likely more or less of

it occurs every summer, but this is the first time that it has been offensively brought to our notice. Even when it was most abundant in the Croton, I do not believe that it communicated any unwholesome quality to the water. Its odor and taste were certainly unpleasant. While the unpleasant quality of the water continues, the ordinary filter will remove all the suspended matter, and a little fresh-burned vegetable charcoal or animal carbon, will take away the disagreeable smell and taste. A good method of using the charcoal is to heat some small pieces red hot, and, after quenching, inclose them in a little bag of gauze, and suspend them in the water. The charcoal should be renewed or reburned every day or two.

I am, gentlemen, very respectfully,
Your obedient servant,
(Signed) JOHN TORREY.

Professor Nichols' results so far as the ammonias and solids are concerned are embodied in the following table compiled from Report of Boston Water Board for 1880.

EXAMINATION of Mystic Lake Water by Professor Wm. Ripley Nichols (Results expressed in parts per 100 000):

DATE.	18 feet below surface.			50 to 78 feet below Surface.		
	Free Ammonia.	Albuminoid Ammonia.	Total Solids.	Free Ammonia.	Albuminoid Ammonia.	Total Solids.
October 18, 1879....	0.001	0.017	9.1	0.007	0.011	7.8
November 8, "	0.008	0.011	9.6	0.009	0.011	9.6
" 15, "	0.008	0.013	9.6	0.023	0.027	11.6
" 22, "	0.015	0.013	9.8	0.016	0.017	9.6
December 8, "	0.011	0.011	9.6	0.011	0.008	9.7
January 30, 1880...	0.031	0.016	11.1	0.027	0.016	11.8
February 6, " ...	0.035	0.019	11.1	0.080	0.019	11.8
" 17, " ...	0.041	0.016	10.8	0.075	0.017	11.5
March 6, " ..	0.040	0.016	10.4	0.040	0.015	9.5
April 5, " ...	0.040	0.013	10.2	0.041	0.013	10.2
" 14, " ...	0.040	0.016	10.4	0.043	0.017	10.3
May 3, " ...	....	....	....	0.048	0.019	10.1
" 12, " ...	0.032	0.016	10.5	0.064	0.016	10.3
" 24, " ...	0.040	0.016	10.6	0.064	0.015	10.5
June 3, " ...	0.036	0.017	10.3	0.049	0.016	10.6
Averages	0.027	0.015	10.2	0.040	0.016	10.3
Average at top during the same time, October, 1879–1880.....				0.028	0.018	10 0
Average as drawn in Charleston during the same time..				0.019	0.015	9.8

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TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

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(Vol. XXI.—December, 1889.)

ON THE EFFECT OF A RAPIDLY INCREASING SUPPLY OF WATER TO A STREAM, ON THE FLOW BELOW THE POINT OF SUPPLY.

By JAMES B. FRANCIS, Past President Am. Soc. C. E.

WITH DISCUSSION.

In the Chezy formula for the flow of water in channels, viz.:

$V = C\sqrt{RI}$;

V = velocity in feet per second;

C = a co-efficient depending on the nature of the bed of the channel;

R = the hydraulic mean depth;

I = the slope or descent per foot in length.

In this case C and I may be taken as constant. The depth of the stream, and consequently the value of R, will increase with the quantity of water flowing, and by the formula, the velocity will increase as $\sqrt{R}$ increases.

The increased velocity due to the increased quantity will be first felt near the point of supply, and later in the stream below, and the effect will be that after a sufficient time the upper parts of the stream having

a greater velocity, will overtake the lower, combining with them and flowing on together, with a depth and velocity due to the greater quantity.

This effect will be most distinctly indicated in a stream having a rapid descent, and may be illustrated as follows :

In a stream having a rocky bed, for which $C = 50$, suppose the descent to be 1 foot in 100 feet, or $I = 0.01$; and suppose the section and wetted perimeter to be such that $R = 1$. Then, by the above formula,

$V = 5$ feet per second.

In 100 seconds from a given moment suppose the supply of water, and with it the depth of the stream, to be increased so that $R = 4$. Then the velocity will be increased to $5\sqrt{4} = 10$ feet per second.

Put $x =$ the time from the admission of the additional supply in which the increased current will overtake the original current, which has had a start of 100 seconds, $100 \times 5 + 5x = 10x$, from which we find $x = 100$ seconds.

The distance from the point of admission of the additional supply to the point where it will overtake the stream flowing 5 feet per second will be $100 \times 10 = 1\ 000$ feet.

After the lapse of another 100 seconds, suppose another additional supply is admitted from the same source, increasing the depth so that $R = 9$, and the velocity becomes $5\sqrt{9} = 15$ feet per second.

For the time x from the admission of the second additional supply, when it will overtake the first additional supply, which has a start of 100 seconds, we have $100 \times 10 + 10x = 15x$, and $x = 200$ seconds.

The distance from the point of admission of the additional supply where it will overtake the preceding will be $200 \times 15 = 3\ 000$ feet.

After the lapse of another 100 seconds, another additional supply is admitted, increasing the depth so that $R = 16$, and the velocity becomes $5\sqrt{16} = 20$ feet per second.

Similarly, the time x from the admission of the third additional supply when it will overtake the second additional supply, which has a start of 100 seconds, will be $100 \times 15 + 15x = 20x$; and $x = 300$ seconds, and the distance from the point of admission, where it will overtake the preceding, will be $300 \times 20 = 6\ 000$ feet.

After the lapse of another 100 seconds, another additional supply is admitted, increasing the depth so that $R = 25$, and the velocity becomes $5\sqrt{25} = 25$ feet per second.

The time x when it will overtake the preceding will be $100 \times 20 + 20x = 25x$, and $x = 400$ seconds, and the distance from the point of admission where it will overtake the preceding will be $400 \times 25 = 10\ 000$ feet.

If there is no further increase in the supply there will be no further increase in the value of R or in the velocity. If the supply diminishes the value of R will diminish also at the point of supply, but the effect of the preceding additions will not be reduced, even if the supply should entirely cease, and R become zero at the point of supply, and from the moment when the current due to the latest increase in the supply has overtaken the preceding currents, and of course become the head of the torrent, it will continue to flow down the channel with the same depth and velocity so long as the descent and form of the channel continue uniform, and for the same length of time that the maximum supply is continued, after which it will begin to be reduced in depth and velocity.

Instances of these effects in mountainous countries are not uncommon. A heavy local rain in an elevated region would obviously cause a rapidly-increasing flow in a channel in which the flow was previously small or even nothing, and as the flow increased, the effect would be as above described, and a torrent would appear suddenly at points below, where there had been no rain or even knowledge of its occurrence elsewhere.

There have also been several instances of the failure of reservoir dams, in which similar effects have been observed; notably in this country the Mill River Reservoir Dam, which failed in 1874, involving the loss of 143 lives, mainly due to the sudden advent of the accumulated torrent at a village some miles below; and the great loss of life in the recent catastrophe at Johnstown, Pa., from the failure of the South Fork Dam, was undoubtedly largely due to the same cause.

The bore in certain estuaries and tidal rivers appears to be due to the same cause.

In a deep stream with a small descent, and consequently small velocity, the effect above described would be complicated with what are sometimes called waves of translation, which cause a rise in the surface of the stream ahead of the arrival of the additional supply.

DISCUSSION.

ELIOT C. CLARKE, M. Am. Soc. C. E.—I understand how the increased value of R with each added volume of water from the point of supply would make the upstream portion of the torrent tend to overtake that which was flowing ahead of it. But, on the other hand, the effect of this would be so to pile up the water at the lower part of the torrent as to increase greatly the other factor I, in the formula. Then the front part would travel the faster, and not until the inclination due to this increased velocity at the front had subsided, would the water in the rear again begin to overtake that ahead of it. It seems as if theory required that alternately the upper and lower portions of the torrent should flow the faster.

Mr. FRANCIS.—I have no doubt it is on an inclined plane in front, but still the water behind would be perpetually catching up to it.

Mr. E. C. CLARKE.—Is it not a fact that there may be sudden great freshets in the upper portions of streams and yet little rise in their lower portions? If so, the rapid advance of the upper portion of the freshet must be retarded in some way, so that the inclination is reduced and the flood comes down gradually.

Mr. FRANCIS.—These floods that we hear of in the mountainous regions of the West sometimes come down when there is no water in the stream to begin with. One described to me occurred where they were building a bridge over a dry channel; the water came down in a torrent, without warning, and swept away the derricks, stagings and part of the work.

J. P. FRIZELL, M. Am. Soc. C. E.—The reasoning adopted by Mr. Francis, in explanation of the movement of sudden floods in the channels of streams, is strictly analogous to that used in elucidating the phenomenon of the "bore" in tidal rivers. The analogy is not broken by treating the facts he refers to as instances of the flow of water in channels, while the bore is regarded as a case of the propagation of waves.

This remarkable phase of tidal movement occurs on but few rivers, notably the Hoogly Branch of the Ganges, on the Seine, and on the Amazon. It shows itself in its full strength on these, only at the time of spring tides. At the mouth of the river, shortly after the time of the tide, a definite wave is seen advancing up the channel. It increases in height as it proceeds till long after the period of high tide is past at the river mouth. It advances, breaking with a loud roar, having low water in front of it and leaving high water behind it, till it exhausts itself in the upper reaches of the estuary. This fact is accounted for thus: The velocity with which an undulation is propagated in water of

limited depth increases as the depth increases, but does not depend upon the amplitude of the oscillation, so long as the latter is small as compared with the depth. We may regard the tide as rising by a succession of indefinitely small instantaneous steps or jumps. Each successive increase of depth would extend up the channel according to the law of wave movement, each moving a little faster than the preceding. The result is that all unite and move up the channel in one grand wave. The velocity of the main wave is not affected by its amplitude, but only by the depth of water in front of it, so that it is always open to accessions from below. This is precisely similar to the reasoning of Mr. Francis except that he admits that the wave front increases in velocity as its amplitude increases, which is undoubtedly correct, as its amplitude is out of all proportion to the depth of water in front of it.

Analogous behavior is observed on great rivers in case of ordinary floods. It is a fact well known to boatmen that the current is much stronger on a rising river than on a falling one, or when the river is at a stand. This is perfectly explicable on the principles alluded to above, viz., that oscillations and changes of depth propagate themselves more rapidly as the depth increases. This action tends to concentrate the fall in front of the flood. If we fix our attention on any point of the stream, we can readily understand how the velocity there, on the same stage of water, can be greater when the front of the flood is at the point than when it has advanced further down the stream. In the latter case, the slope at the point of observation has greatly diminished. The supply has fallen off. The velocity at the front of the flood is maintained in large measure by the volume of water accumulated in the channel, although the velocity at the front of the flood is unchanged, it is constantly diminishing from that point up to the point of observation, without regard to the effect of influent streams.

In the case of a bursting reservoir, the statements of witnesses are, generally, to the effect that the flood advances in the form of a wall of water. This phenomenon is not to be explained on precisely the same principles as the tidal bore, because the movement is wholly one of translation, the initial depth of the stream being generally insignificant, whereas, motion of translation cuts but a small figure in the case of the bore. The action, in this case, appears more analogous to that of a breaking wave. Waves break on a shelving beach. They break because they have reached a depth in which the cyclic movement constituting the wave is no longer possible. The fluid mass, suddenly freed from cyclic motion, moves forward under the simple action of gravity, very much as it would if suddenly released from confinement by the breaking of a dam, the bottom retarded by the friction of the ground, the upper parts tending to outstrip the lower, and so combing over and breaking. The movement rapidly exhausts itself because it takes place on ascending ground. If we could conceive of its occurrence on de-

scending ground, with the infinite mass of the ocean behind it, we could assign no reason why it should not continue indefinitely.

In the case of a finite mass of water, the breaking action would exhaust itself rapidly after the exhaustion of the supply. The depth would diminish from the front of the flood upstream. All parts of less than a certain depth would lag behind. The mass of water would simply strew itself along the channel, and when the breaking action at the front was exhausted, the whole movement would proceed as in the case of an ordinary freshet resulting from natural causes.

Mansfield Merriman, M. Am. Soc. C. E.—Speaking the other day with a gentleman who has had extensive experience in the location of railroads in our western territories, he stated that the same phenomenon had been observed by him in cañons. Often when the water was very low in the streams, a vertical wall of water would come down, due to a local rainfall several miles up stream; and this was so common an occurrence that in pitching their tents for the night they took care to place them so that they would not be carried away. His description of the vertical wall of water agrees with what has been stated of the Conemaugh flood, that the upper portion of the crest of the wall of water was of a rolling appearance; as he said, it resembled the heads of buffaloes pitching over.

The complete explanation of this phenomenon is doubtless of a very complex character, being a problem in the theory of the non-uniform flow of water to which the formula $V = C\sqrt{RI}$ only approximately applies. The paper of Mr. Francis contains the only algebraic discussion of the subject that I have seen, and as a general explanation it is very satisfactory.

Mr. Francis.—In the Conemaugh flood the effect of the break of the South Fork reservoir dam was felt most disastrously in the river below. In one of the early reports on the reservoir its storage capacity was estimated at 525 000 000 cubic feet; at the time of the break it was about 8 feet above the level of the wasteway, which added 152 000 000 cubic feet, making the total contents of the reservoir 677 000 000 cubic feet. This all went out out through the breach in about an hour, making the average rate of discharge about 188 000 cubic feet per second. Water was flowing into the reservoir from the water shed at the same time at the rate of 8 000 cubic feet per second; this, of course, was discharged with the storage, making the total average discharge through the breach about 196 000 cubic feet per second.

The breach was formed by the water flowing over the top of the embankment and washing away the earth; this was, of course, done gradually. As the breach enlarged the quantity of water discharged would increase up to the point when the diminished height in the reservoir would so reduce the velocity as to compensate for the increased

size of the breach. At this point the discharge would be a maximum. I have no means of estimating with any precision this maximum discharge or the time at which it occurred, but I think it must have been nearly double the average discharge, certainly not less than 350 000 cubic feet per second, and the time about thirty minutes after the commencement of the breach, or say at about 3.15 P.M., May 31st. The streams below were in very high flood at the same time from other water-sheds. The South Fork River from the reservoir to South Fork Railroad station, where it joins the Conemaugh River, is about 2½ miles, and the first appearance there of the flood caused by the break in the reservoir dam was a great wave through the South Fork, which carried away the railroad bridge over the Conemaugh at 3.08 P.M. This was at least twenty minutes after the commencement of the breach, but before the discharge had reached a maximum.

The flood in the Conemaugh River caused the water to be 4 to 5 feet deep in the lower streets of Johnstown before the South Fork wave reached there, which was about 3.56 P.M. or about one hour and eleven minutes after the commencement of the breach in the dam and about forty-one minutes after the discharge had reached its maximum as estimated above. The distance by the river from the dam to Johnstown is about 13 miles and the descent about 380 feet. If the first wave at Johnstown corresponded to the maximum discharge at the broken dam it traveled at the rate of about 19 miles an hour.

Mr. Thomas P. Roberts, Chief Engineer of the Monongahela Navigation Company, has endeavored to ascertain the time when the flood from the South Fork reached Johnstown, and informs me that, "as is to be expected, the statements are conflicting as to time, depending on the location, *i. e.*, distance up or down the stream, with reference to the railroad station at Johnstown, accuracy of the clocks and watches at the moment of stopping," etc. W. S. Jarboe, M. E., member of our local society of engineers, tells me that he saw at least twenty clocks and watches which had been stopped by the flood, and he made special inquiry on this point, and I rely chiefly on his statement.

The time of the flood, *i. e.*, when it had reached a height sufficient to overturn houses in Johnstown, opposite the Pennsylvania Railroad depot (which point is more than a mile below the beginning or upper end of the built-up part of the town traversed by the flood), was just about 3.56 P.M., May 31st.

I think the time occupied by the water in rising to such a height in the case of the majority of the structures was not more than two minutes, if indeed so much. The time at which the flood reached its maximum height at Johnstown would be difficult to learn. I think most of the houses were gone before the full height was reached, as this was the first wide expanse of the water; the first or violent effect passed, it rose more gradually to the maximum height. The railroad

bank and the stone bridge acted as a dam, converting the entire area into a lake, through which, however, swept very powerful currents, creating the "whirlpool" mentioned by so many witnesses.

All the evidence appears to agree in the statement that the flood advanced down the valley with quite a head, as much as 10 feet, I imagine—most of the eye-witnesses assert that no water was visible in the advance of the wave (I now refer to the valley above the town)—but simply a moving mass of tree trunks, logs, etc., "boiling up" violently, and "producing a cloud of spray."

The Conemaugh flood differs from the case contemplated in the paper, being complicated by the great flood from other sources than the breach in the dam, but although no doubt much modified, there does not appear to have been anything observed which conflicts with the principles discussed in the paper.

The problem in a more general form would be to find the velocity V, after a given time if R increases uniformly or according to some other law, but I have been unable to resolve it into this form.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

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Vol. XXI.—December, 1889.

DISCUSSION

ON

AMERICAN RAILROAD BRIDGES.*

H. D. BUSH, M. Am. Soc. C. E.—1. *Historical.*—Mr. Cooper's valuable paper on American Railroad Bridges should have as complete a discussion as possible. The difficulty of getting facts concerning the historical portion of this subject will increase rapidly as time goes on. I should not be faithful to my native city if I did not call attention to the work done in Springfield, Mass., in the development of the American bridge.

Springfield, on the banks of the Connecticut River, in the direct line of travel from Boston to the West, was one of the first places having a large bridge. This was built in 1805 (Mr. Cooper refers to it as being built by a Mr. Walcott), and consisted of six arches, which must have been of nearly 200 feet span.

The travel was said to be "neither on the bottom or top chord, but ascending and descending with the curve of the arches on each span." This bridge failed in July, 1814, by crippling of the arches.

In 1816 a Burr truss bridge was built on this site by Captain Isaac Damon, of Northampton, Mass., "a man of great capacity for construction and superior workmanship."

* American Railroad Bridges, by Theodore Cooper, Transactions, Vol. XXI, No. 418, July, 1889.

This bridge consists of six spans of about 100 feet each, and three of 180 feet each, total length. There are three trusses, the center one having two arches side by side. This bridge is still in use, and carrying two horse-railway tracks in addition to ordinary travel.

About 1881 a 5-foot sidewalk was added on the outside of the southern truss, this truss being strengthened by the addition of a Towne or plank lattice truss bolted to it. This bridge bids fair to outlast the present century.

William Howe, the inventor of the Howe truss, lived at Warren, Mass., and came to Springfield in 1840, when Amasa Stone and D. L. Harris formed a firm to build bridges under Howe's patent. This was probably the first bridge-building firm in the United States organized practically, as our bridge companies are now, with an office force of contracting agents, draughtsmen, etc. (a shop turning out iron-work), and a corps of experienced erection men. From this parent firm in Springfield there branched out, as bridge builders, Joseph Stone in Eastern New England, Daniel Stone in the Middle States, A. B. Stone and L. B. Boomer in Chicago, Thacher, Burt & McNary in Ohio, and A. L. Maxwell in Knoxville, Tenn.

Of the original firm Amasa Stone went to Cleveland, Ohio, where, as President of the Lake Shore Railroad, he had the Ashtabula Bridge constructed, and D. L. Harris became President of the Connecticut River Railroad. The original firm has been succeeded by Harris & Briggs; Harris & Hawkins; Hawkins, Herthel & Burrall and R. F. Hawkins, who now is building iron bridges near the site of the original shop in Springfield.

Of the early bridges built by this firm, two deserve special notice. One crossed the Connecticut River at Springfield, carrying what is now the Boston and Albany Railroad, and consisted of seven spans of 180 feet each of double intersection Howe trusses. This was the first Howe truss railroad bridge ever built, and was replaced by a more modern Howe truss of single intersection form in 1855. This, in its turn, was replaced in 1873 by the double track riveted bridge, to which Mr. Cooper has referred. The widening of the old piers for a double track bridge presented some difficulties which were met by a new departure in coffer dams, designed by Mr. Burrall, of Hawkins & Burrall (now consulting engineer for Mr. Hawkins), and described by him in Van Nostrand's magazine at that time.

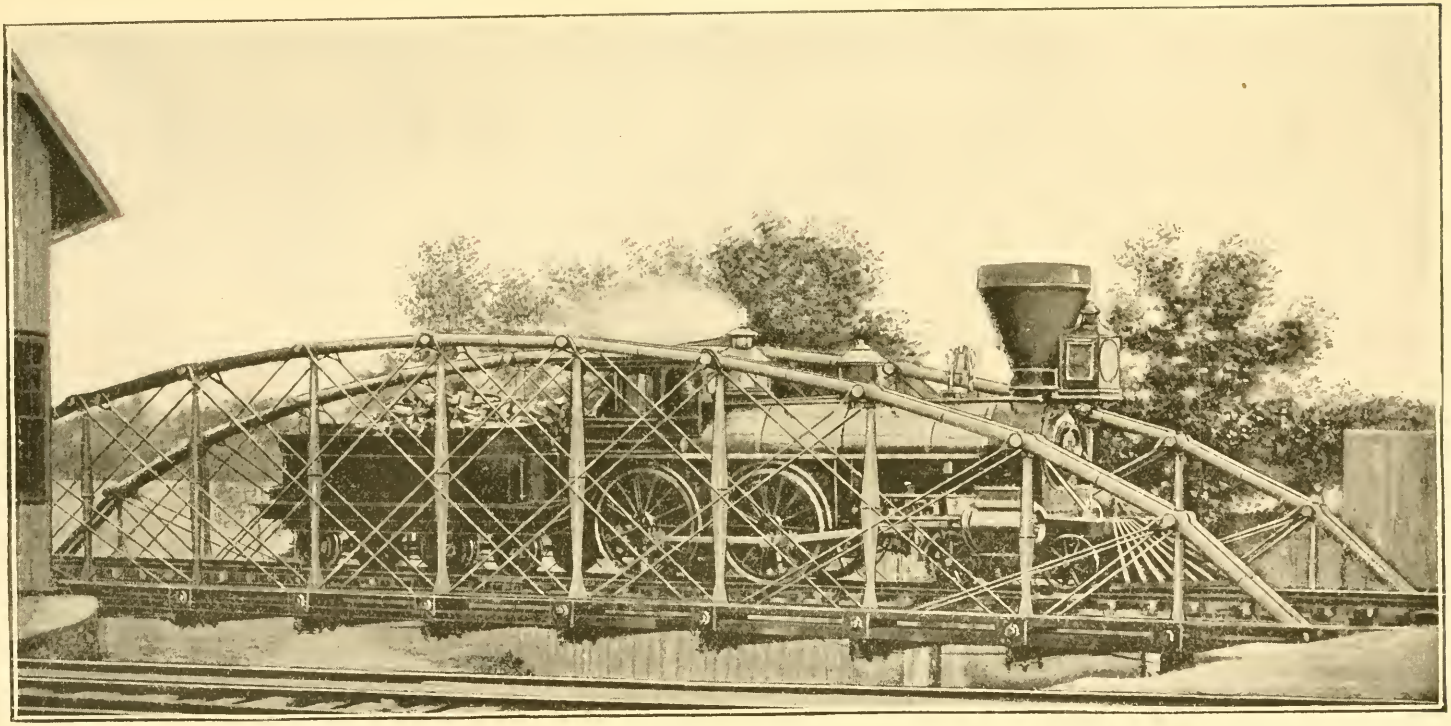
The second bridge was one of two spans of about 260 feet each, at Bellows Falls, Vt. These were Howe trusses, strengthened with arches, and though probably the longest railroad spans of the kind ever built, were in use under the very considerable traffic at this junction point of several railroads until 1882, when they were replaced by two riveted lattice spans of the same length.

The Springfield bridge builders early turned their attention to iron bridges, and, in 1849, constructed an iron Howe truss bridge of double track for the Boston and Providence Railroad. This bridge was of about 50 feet span. The brace and counterbrace of each panel were cast together in one piece, and were of cruciform cross-section. The bottom chord was made of flat iron, with a peculiar form of joint, shown with other details of construction in accompanying drawing. (Plate XCI.)

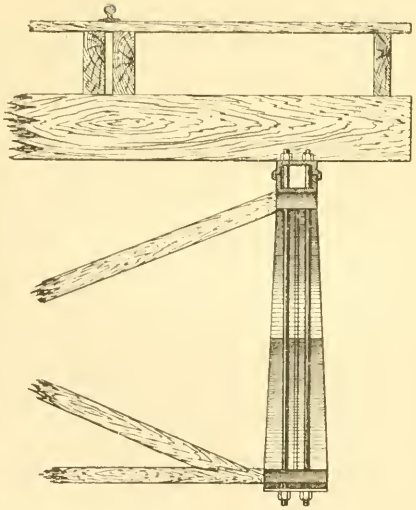
In 1866 the Herthel Parabolic Truss was patented by George P. Herthel, of Hawkins, Herthell & Burrall. Mr. Herthel was an able engineer, educated at Carlsruhe, and, in this truss, used cast-iron to the best advantage. Though, at this date, wrought-iron bridges were coming into general use, it must be remembered that this material was very expensive in New England, and credit should be given to men who tried to use cast-iron intelligently. Many bridges of the original Herthel truss or its modifications, with straight top chord, made by engineers William H. Burrall and Phelps Johnson, of the Springfield works, were built. The accompanying photograph (Plate XC) shows one of the original bridges still in use after twenty-one years continuous service on a side track of the Connecticut River Railroad over the entrance to Hampden Park in Springfield.

2. *Typical American Bridges.*—The ancient discussion concerning pin-connected and riveted trusses was called American *vs.* English bridges. Mr. Cooper recognizes that there has been a distinctive American practice in the building of riveted bridges, but, I think, does not give the builders of these bridges sufficient credit in the evolution of our modern bridge. He acknowledges the excellence of the early riveted trusses, beginning in 1859, and I think any person who looks into the matter carefully, must acknowledge that, until within the last few years, the average riveted structure has been as good, if not better, than its contemporaneous pin bridge. This is especially evident when we consider what a large percentage of the early pin bridges were constructed of "forms requiring cast-iron joint boxes, now obsolete," and that nearly all of them, so great was the confidence in their theoretical perfection, were made excessively light throughout, to say nothing of their defective details, as for instance, unstiffened end panels and deficient lateral systems attached to hanging floor-beams.

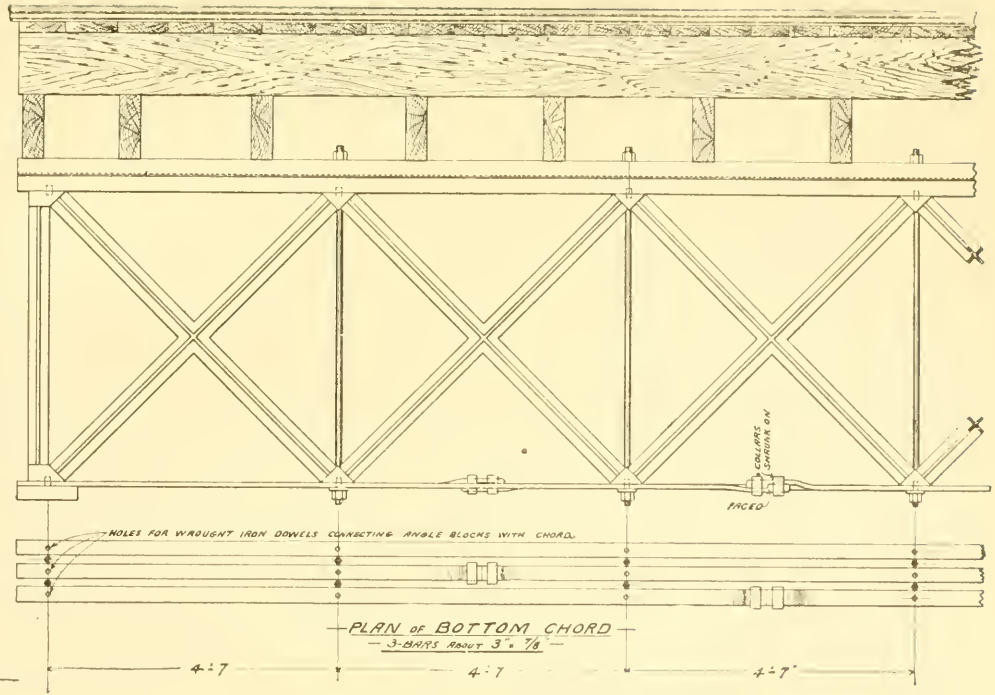
The railroads buying riveted bridges have always insisted on heavy work. In 1880 the Boston and Albany Railroad's specifications prescribed a load of 3 000 pounds per foot with excess loads from heavy engines, and allowing a tensional strain of only 7 500 pounds per square inch of net section. The Bellows Falls riveted spans were figured for a load of 2 750 pounds per foot (with customary engine excess loads), while a year or two later a much more important bridge, the Niagara Cantilever, supposed to be an example of the best practice in pin bridges,



HERTHEL PARABOLIC TRUSS.



— PARTIAL CROSS SECTION —



— PLAN OF BOTTOM CHORD —
— 3-BARS ABOUT 3" × 7/8" —

— 50FT SPAN DOUBLE TRACK —
— IRON HOWE TRUSS —
— BOSTON & PROVIDENCE R.R. 1849 —
— BUILT BY STONE & HARRIS —
— SPRINGFIELD MASS. —

PLATE XCI.

was designed for a load of only 2 000 pounds per foot, headed by comparatively light engines.

In spite of its stiffness, which we used to be told was "no indication of strength;" in spite of the evil drift pin, and the fact that "even the fiercest inspector cannot keep every hole and rivet before him," riveted bridges have continued to stand up well under their work, and the loads of abuse heaped upon them. The late very extensive alterations made on the Springfield Bridge of the Boston and Albany Railroad failed to reveal the existence, to any appreciable extent, of any of the evils supposed to be inherent in this form of bridge; and the fact remains that the skilled riveters and heaters employed by the firms building riveted bridges exclusively, invariably do better field riveting than the comparatively unskilled gangs of "climbers" and laborers usually used in doing the very considerable amount of riveting in the modern pin bridge.

However, though the writer's first work was on riveted bridges, and though he now happens to be building fourteen spans of riveted railway bridges, all of over 200 feet each, and in which all compression joints are faced, and work so laid out that separate members can be carefully inspected as to length and accuracy of workmanship, he is willing to acknowledge that in designing for the heavy loads of present specifications, the best type of the modern pin bridge is superior to any riveted bridge in spans of over about 125 feet, or say 100 feet in double-track bridges.

But it must be remembered that the modern pin bridge owes much of its excellence to the adoption of features formerly condemned in the riveted bridge.

Floor-beams are now almost universally riveted to the posts calling for much field riveting in important "joints," and giving rise to a certain amount of ambiguity of strain; stiffened or riveted end lower chords and collision struts are used, and the old-fashioned angle-iron lateral bracing of the riveted bridge is coming into general favor—even Mr. Cooper now specifying that "preference will be given to lateral bracing in the floor system, which is capable of resisting both compression and tension."

The old bugbear of the riveted bridge now applies with double force, for the reasons above given, to the modern pin bridge. This is the uncertainty of hand riveting in erection work, and the writer does not understand why "the engineers acting for the railway companies" have not specified that the bulk, at least, of this riveting should be done by power, instead of by hand, and is confident that American ingenuity would soon produce machines capable of doing this work well and cheaply.

3. *The Manufacture of Bridges.*—Mr. Cooper's description of an American bridge shop might be misleading to one not familiar with our lead-

ing works, and leave the impression that seven or eight different shops were generally used. All work, except forging and lathe and planer work, is generally done under one roof. In at least one shop, that of the Dominion Bridge Company (Limited), specially built by an American engineer for the manufacture of American bridges, all iron work is done under one roof, there being a separate shop for template making only.

In the various departments which Mr. Cooper names there are likely soon to be many changes. In the straightening department, even at this late date, plates are generally subjected to the barbarous process of hammering and "peaning," instead of being straightened both sideways and edgeways by the rolling process in use by the Dominion Bridge Company, and now adopted by one only of the new American shops.

In the rivet shop, punching and reaming in "high" steel work is likely soon to be superseded by drilling from the solid. At least one of our leading engineers is about ready to call for this in his specifications, and some of the manufacturers are in favor of it.

This will at least prevent, in this class of material, the use of questionable processes of straightening angles curved by punching.

In the manufacture of eye-bars a good deal of success has been obtained with rather crude processes and roundabout methods. However, no shop is now ready to make the largest bars now called for, and one of our leading shops has lately been compelled in the long and very heavy span of the Wheeling Bridge to adopt the somewhat doubtful expedient of the use of two lower chords, one above the other.

The probability is that with increasing development and cheaper money such very heavy long-span bridges as are now building and talked of will become more and more frequent, and the probable general use of steel will call for a considerable remodelling of our bridge shops. More capital will have to be employed, and more shop capacity used to keep up the present output, to say nothing of increasing it.

In short, the complete history of the American Bridge cannot now be written.

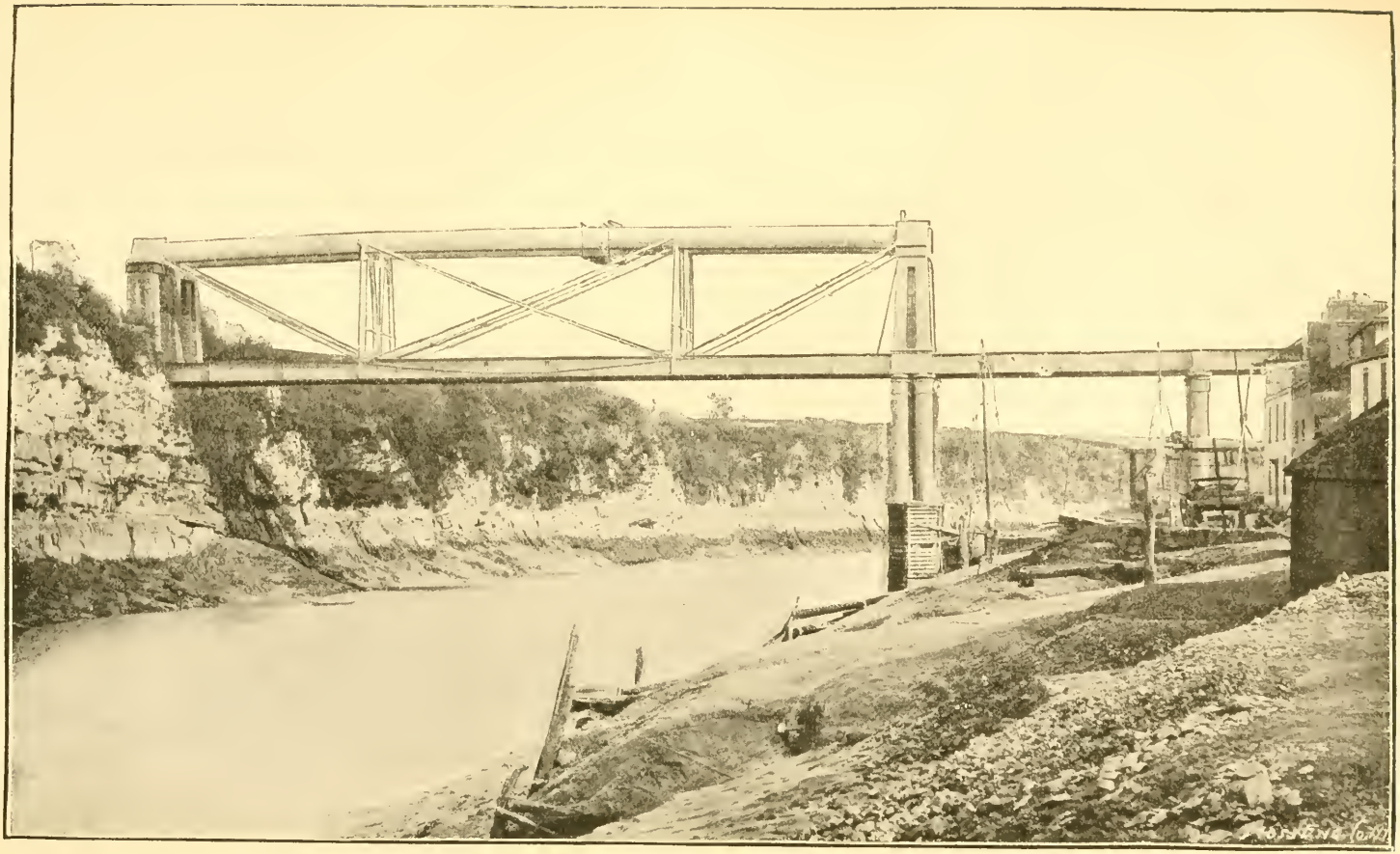
We are now at the opening of one of the most interesting chapters, and there is still great need of the ingenuity and energy of the pioneers to whom Mr. Cooper pays such high tribute.

J. H. Cunningham, M. Am. Soc. C. E.—May I say that as an Englishman, I hope many English engineers will read Mr. Cooper's description of modern American practice in bridge building, and his remarks on "Bridge Failures," for I believe that what he has written may clear away prejudices against pin-connected work and misunderstandings as to what it really is, which at present sometimes prevent its adoption in circumstances in which it might be used with great advantage.

The year 1852 may, I think, be regarded as an epoch in the history of bridge building, both in America and in England. Mr. Cooper tells us that

in America, about that time, the bridges designed by Messrs. Fink and Bollman first came into use, and the earliest Pratt and Whipple trusses were built. About the same time, in England, Mr. Brunel completed his bridge across the Wye, at Chepstow (Plate XCII). This was a striking departure from the tubular system, which, at that time, had recently been introduced with much success. Mr. Brunel reduced the diameter of the tube so that it formed the top boom of a truss which he divided into three panels, each 100 feet in length. The only description of this bridge which I know of is to be found in the life of Brunel, published by Longmans, but its details are very similar to those of the Saltash (Royal Albert), drawings of which are given in Humber's work. The Saltash bridge was opened in 1859. It has two spans of 455 feet, and the track is carried by main parabolic (Pauli) trusses, 56 feet deep, divided into ten panels of 39 feet 4 inches, and two of 28 feet 6 inches. The tension members are flat bars 7 x 1 inch, connected to pins $4\frac{5}{8}$ inches diameter. Thus long panels, flat eye-bars rolled in one piece, and pins were introduced in England more than thirty-five years ago. Brunel died a few months after the completion of this work, and unfortunately the line he struck out in these large bridges was not followed up. Short panel trusses of moderate depth fully riveted became fashionable in England, and continue in vogue, though in recent years there has been a tendency toward longer bays and greater depths.

John Sterling Deans, M. Am. Soc. C. E.—I have read with much pleasure Mr. Cooper's paper upon "American Railroad Bridges," and particularly that portion in which he so ably presents the merits of "American Designs." As Mr. Cooper there emphasizes, the final erection of the structure must always be kept in view, from the skeleton diagram through all the details of connection. It is this facility of erection, in addition to reducing the ambiguity of strains to a minimum, to which the American pin-connected truss owes its popularity among bridge engineers. As an instance of rapid erection, considering the magnitude of the structure and great size and weight of individual pieces, the erection of the channel span of the Ohio River Bridge, at Cincinnati, by the Phœnix Bridge Company, in 1888, and to which Mr. Cooper calls attention as the longest and heaviest pin-truss span in existence, is believed to be the greatest feat yet accomplished in bridge construction. This whole structure, of over a mile in length of double track and double roadway, containing novel and interesting points, will, no doubt, form the subject of a paper to be presented to the Society in the near future. But it seemed appropriate to add this one item of the channel span erection to the discussion of Mr. Cooper's paper. As is well known, the false work under this span was washed out clean on August 26th, 1888; the Phœnix Bridge Company losing complete false work, traveler completely rigged, and thirteen panels of floor of new span, 650 000 pounds. The company immediately put themselves in



Vol. XXI, p. 572. CHEPSTOW BRIDGE. PLATE XCII.

correspondence with lumber companies throughout the country, and in just five weeks, by the greatest effort, and working continuously by aid of electric light at night, had received and driven 1 200 piles 50 feet long, framed and erected 550 feet of false work 85 feet high, containing 700 000 feet B.M. of lumber; sheathed and braced 1 000 lineal feet of pile protection; framed, erected and rigged a traveler 114 feet high, 55 feet wide, 65 feet long. In the meantime shops had replaced the lost iron of floor, and the first iron was run out September 30th, at 11 A.M., and the span was swinging October 30th. During this time there was a loss of four days owing to heavy rains. This span is, for double track and double highways, 545 feet long, 84 feet deep, 30 feet center to center trusses, 65 feet center to center sidewalk railings, and weighs 3 800 000 pounds. When it is remembered that when this span was swung, it was complete (except roadway brackets) and in working order and carried trains twenty minutes afterwards, all eye-bars, bracing, laterals and struts in position, it must be acknowledged that it reflects great credit, not alone on the engineering firm who designed and built the great span upon the very latest and best single intersection American pin-connected plan, but also upon the American workmen whose skill and energy made such a record possible.

William E. Hoyt, M. Am. Soc. C. E.—One point that Mr. Cooper makes in his paper might, perhaps, be treated a little more fully. It is that well-designed and well-built pin-connected bridges are quite as capable of withstanding sudden shocks, resulting from derailments or collisions, as those of the riveted-lattice type. Since testimony to the contrary has been given by other writers, it might be well for some of us who have had experience in these matters to support Mr. Cooper's argument by evidence in its favor.

I am able to recall a considerable number of serious derailments on bridges that have been under my care, and at least two collisions that happened in through bridges. From a careful consideration of the results of the accidents that have come under my observation, I am convinced that pin-connected bridges have invariably withstood such extraordinary strains fully as well as the riveted-lattice forms. In one instance, particularly, it seems almost certain that the accident would have caused more serious damage to the structure if it had happened on a riveted lattice instead of a pin-connected bridge. In this case, a freight train was thrown entirely through the web of one truss, bending an intermediate post and the suspension bars. The shock knocked one end-post also clear from the shoe, so that the bottom of the broken post stood on the masonry, and yet the bridge in this condition carried trains for some hours afterwards. It was a light structure of its kind, without any special merits of design or construction.

If testimony of this kind is needed to prove the stability of the so-called "American type" of railroad bridges, let us have more examples.

Major O. E. Michaelis, M. Am. Soc. C. E.—The acknowledged repute of the author and its own intrinsic merits will undoubtedly give this paper in coming years the weight of accurate history, and I trust Mr. Cooper will pardon me if I call his attention to a matter which he has very modestly omitted to dwell upon—a matter which concerns one of the most interesting periods in American bridge building, and which should certainly have mention in this paper. I allude to the remarkable work done by our bridge engineers during the war; and in this connection the name of one of the most prominent and regretted members of our Society deserves to be recorded. I allude to Colonel W. W. Wright, who was General Sherman's chief bridge engineer in his march from Chattanooga to Atlanta. I especially recall the rebuilding of the very long bridge over the Chattahoochie River. Colonel Wright put that bridge up in eighty-four hours, in the very presence of the enemy. I trust that Mr. Cooper, in the revision of his paper, will say something regarding the work of our deceased member, a man who died in sickness and penury, unknown to the friends who would have tenderly nursed and cared for him, yet whose name deserves to be placed upon every soldier's monument from Maine to California.

Theodore Cooper, M. Am. Soc. C. E.—Some years ago I, by deputy, sought to collect a great deal of the bridge history of the war, and it was surprising how little I succeeded in getting. If Major Michaelis will exert his influence in that direction, I think he could make a very interesting paper, but in the rush of the war the records of such things pass into tradition. Mr. Stauffer, at my suggestion, some two or three years ago, endeavored by correspondence to secure some records of bridge building performed by our army, and utterly failed. If Major Michaelis can reach them, however, they would be very valuable to have.

J. Elfreth Watkins, Assoc. Am. Soc. C. E.—I have been particularly interested in the paper which has just been read to the Society by Mr. Cooper. In the section of Transportation and Engineering in the National Museum which is under my charge we are making an effort to preserve the history of transportation by locomotive and steamboat, and in the models and drawings and originals that we have there we are attempting to make some advance upon what is done at the Patent Museum at South Kensington, the Conservatoire des Arts et Metiers at Paris, or the Naval Museum at Venice, where similar objects are exhibited more as curiosities or relics. As some of you perhaps know, we are attempting to arrange these objects so that we shall be able not only to preserve their history, but to show the progress of thought from time to time. We are especially deficient in matters relating to bridge work; in fact, our collection so far is only a nucleus; for this reason I have been greatly interested in hearing this paper read, and shall take pleasure in preserving the illustrations in our files.

George H. Pegram, M. Am. Soc. C. E.—The concise history and statistics of American bridge development presented in this paper must be of great interest and value to all engineers. The author's long experience and wide and intimate acquaintance with the engineers and engineering of the country have peculiarly fitted him for the preparation of such a paper.

In submitting a few ideas at variance with some of the views expressed, I would not be misconstrued into a want of appreciation of what has been done for us, or as descending from the high plane of the paper to the consideration of a few details of minor importance; but it is such occasions as the discussion of this paper that one must take to bring himself into line with his fellow workers.

Live Load.—In my discussion of a paper by Mr. Joseph M. Wilson, M. Am. Soc. C. E., on "Specifications for Strength of Iron Bridges," Transactions of the Society for June, 1886, I showed that a uniform load in combination with a single concentrated load would give all of the results obtained by the use of the engine loading with substantial exactness.

Examples were given of spans from 8 to 400 feet girders; and single and double intersection trusses, with a variety of panel lengths sufficient, it was thought, to place the question of the entire equivalence of the two loadings beyond dispute.

The severity of the test was increased by the requirement of Mr. Wilson's specifications that the maximum strains from three different classes of engines should be taken.

This requirement was made by Mr. Wilson to cover the greater concentration of weights on the wheels of some tank and passenger engines. If he had gone a step further and required different positions of these engines with reference to each other and to the train he would have represented still more accurately the loading to which his bridges were subjected. Such a condition would, however, be proscribed on account of the complexity of the specifications and large amount of labor required.

It is, therefore, claimed that the proposed loading through practically representing an average of all of the combinations of loadings gives more accurate results than the engine loading specified, and since through its use the strains can be determined in about one quarter of the time ordinarily taken when only one class of engine is specified, its general use would seem desirable. In still other respects it is better in insuring the same results by all calculators, being independent of prepared tables and diagrams, and allowing a choice of finding the strains by either the analytical or graphical method with equal convenience.

From the table (No. 1) given by Mr. Cooper, showing the increase in the live load for the past seventeen years, we see that if we allow six

months for the time from the commencement of calculations to the completion of a bridge, the loading will have increased in the time 3 per cent. If we further consider that the bridge will last twenty years, in which time the load will have increased 120 per cent., and should last a hundred years except for this growth of the load, there would seem to be little excuse for the refinement to which some calculations are taken, especially when based upon one of many reasonable assumptions with regard to the load.

I would suggest now that a still further advance be made by making the relations between the uniform and concentrated loads such that the effects of impact shall be included.

Impact is known to vary with the length of train motion through which a member receives its maximum stress. It is a custom with some engineers to provide for it by a sliding scale of percentages to be added to the stresses produced by the load considered as static. With some others it is the custom, instead, to vary the unit stresses. Both of these methods involve a complication and inexactness which can, it seems, be avoided in the manner proposed.

As actual cases are most satisfactory in such a discussion, I shall illustrate the point by the Erie loading of 1879. This loading is so much out of date that the results can be of little use, except for illustration, but it will be a more satisfactory test because the difference between the average weight of the engines and that of the train is much greater than in subsequent loadings, making it rather more difficult to approximate with a uniform load, and also because Mr. Cooper has given some tables for this loading with which it will be interesting to compare.

I would propose in lieu of the Erie loading, and as containing provision for impact, a uniform load of 2 400 pounds per foot, combined with a concentrated load of 40 000 pounds, the concentrated load being placed at that point which will give the maximum stress in the member under consideration. A concentrated load passing over the length of the span is equivalent to a uniformly distributed load of double the amount, and hence the flanges of girders and the chords of trusses will be figured for a total uniformly distributed load of $2\,400\,S + 80\,000$, S being the span; and the web stresses of trusses by adding 40 000 pounds to the common panel load at the end of the train and backing it off the span in the usual manner.

In this latter it is assumed that the uniform load will extend over the panel in front of the concentrated load, and the half panel load at the advance panel point shall be neglected, a condition in favor of simplicity and involving no error when it is an understood feature of the calculations.

The following table (No. 1) gives a comparison of the proposed loading with the approximate equivalent loads given by Mr. Cooper for the Erie loading.

TABLE No. 1.—Erie Loading.

Span in Feet.	Equivalent for moments, Cooper.	$2\,400 + \frac{80\,000}{S}$	Percentage for Impact.	For Double Shears. Cooper.	$2\,400 + \frac{40\,000}{S}$	Percentage for Impact.	For Single Shears at end, Cooper.	$2\,400 + \frac{80\,000}{S}$	Percentage for Impact.	For End Web Shears, Engine.	Proposed Loading.	Percentage for Impact.
10	5.290	10.400	96	4.620	6.400	40	7 200	10.400	45	5.940	9.576	61
15	5.280	7.730	55	4.210	5.100	17	6.240	7.730	24	5.280	7 176	55
20	4.640	6.400	38	3.830	4.406	15	5.400	6.400	19	4 817	5.976	24
30	4.200	5.100	20	3.400	3.730	10	4 740	5.100	8	3.866	4 776	24
40	3.850	4 400	15	3.175	3.400	6	4.300	4.400	2	3 554	4.176	17
60	3.400	3.730	10	2.970	3.070	3	3.830	3.730	....	3.146	3.576	10
80	3.260	3.400	5	2.870	2 900	....	3 620	3.400	....	3.100	3.300	6
100	3.230	3.200	....	2.750	2.800	....	3.450	3.200	....	3.200	3.200	0
150	3.070	2.933	....	2.450	2.670	....	3 180	2 933	....	3.048	2 936	
200	2.910	2.800	....	2.370	2 600	....	3.000	2.800	....	2.900	2 900	
300	2.710	2.667	....	.....	.....	....	.....	.....	....	2.690	2.666	
400	2.610	2.600	....	.....	.....	....	.....	.....	....	2,643	2.600	
500	2.540	2.560	....	.....	.....	....	.....	.....	....	2.547	2.560	

Let us consider this table. In the columns for moments the high percentages for impact in spans under 30 feet will provide for the greater concentrations and speeds of passenger and tank engines. They agree well with current practice. The percentages for double shears conform exactly with those for moments; thus for a 10 feet span the percentage should be for a train motion of 20 feet, which it is. Double shears over 100 feet would be of no practical use, except for loadings on piers, where the engines should be assumed to be preceded as well as followed by cars, in which case the proposed loading would be exact.

Mr. Cooper has designated as "single shears" those at the extreme ends of the spans, where one heavy wheel is just moving on to the support. The percentages for these columns should be the same as those for moments, whereas we see that they are much less.

The only use that would ever be made of these values would be in the riveted attachments at the ends of spans generally under 40 feet, as stringers to floor beams. The end shears that would be used to determine the web would be taken, in girder spans, at a distance from the end equal to the depth of the girder, and in truss spans at the first panel point. These are given in the last columns, and are seen to be substantially correct. I think they may also be taken as representing more correctly the shears at the end attachment than those given in the preceding columns, because the latter depend upon an ideal theoretical condition that has no parallel in practice. Taking, for instance, the attachment of a stringer to the floor beam, and assuming that a heavy wheel is over the center of a cross-tie placed close to the floor beam, the distance from the end of the stringer to the wheel becomes one foot, modifying in a considerable degree the percentages given, and if we further take into account the distributing effect of the rail, which Mr. Cooper's specifications place at three ties, we are reduced practically to the conditions in the last column.

The heavy passenger engines referred to, say with 30 000 pounds on drivers 8 feet apart, while producing greater moments than the consolidation engines in small spans, will produce less shears.

For trusses, comparison between the actual stresses in the different members and the amounts, as well as percentages, of difference are so much more satisfactory than those between approximate equivalent loads that, at the risk of being prolix, they are given in Tables Nos. 2, 3 and 4.

The values are for one truss, being those due to half of the loads previously given. The letters *a*, *b*, *c*, etc., refer to the panel points at which the stresses occur, calling the first panel point from the pier *a*, the next *b*, etc.

In spans over 200 feet it is thought necessary to give only the shears in the end panel, the middle panel and the counters.

The values are for *live load only*, and the counters are given to the panel at which the dead load shear overcomes the live.

TABLE No. 2.

Chord Stresses for Erie Loading—Single Intersection.

Panel.	100 feet span, 25 feet deep. 5 panels of 20 feet.			150 feet span, 25 feet deep, 6 panels of 25 feet.			200 feet span, 33⅓ feet deep. 8 panels of 25 feet.		
	Engine Load	Proposed Load.	Pr. ct. Diff.	Engine Load.	Proposed Load.	Pr. ct. Diff.	Engine Load.	Proposed Load.	Pr. ct. Diff.
a	51.160	51.200	0	95.220	92.100	—3	95.700	91.900	—4
b	72.520	76.800	6	147.500	147.300	0	155 400	157.500	+1
c	.....	.....	....	156.700	165.700	+6	196.500	196.300	0
d	.....	.....	....	.....	.....	....	202.090	209.900	+4

TABLE No. 3.

Web Shears for Erie Loading.—Single Intersection.—All 25 feet Panels except in 100 feet Span.

	100 feet Span.			150 feet Span.			200 feet Span.		
	Engine.	Proposed.	Pr. ct. Diff.	Engine.	Proposed.	Pr. ct. Diff.	Engine.	Proposed.	Pr. ct. Diff.
a	63.950	64.000	0	95.200	91.900	— 4	126.900	122.500	— 3½
b	35.430	40.800	14	62.500	62.600	0	95.900	93.700	— 2½
c	17.440*	22.400	30	36.700	38.900	3	68.400	68.800	0
d	.....	.....	..	15.300*	20.500	30	46.600	47.500	+ 4
e	.....	.....	..	.....	.....	....	25.400*	30.000	+ 19
	300 feet Span.			400 feet Span.			500 feet Span.		
a	185 300	183.400	— 1	247.800	243.700	0	302.600	304.000	+ 1
Mid.	61.800	62.500	+ 1	77.300	77.500	0	92.400	92.500	0
	44.000*	45.800	+ 4	60.120*	61.800	2	60.400*	62.000	3
	28.200	31.700	+ 11	45.000	47.000	5	46.800	49.000	5

* First counter.

TABLE No. 4.

Web Shears for Erie Loading.—Double Intersection.

	200 feet Span. 8 Panels of 25 feet.			200 feet Span. 12 Panels of $16\frac{2}{3}$ feet.			500 feet Span. 20 Panels of 25 feet.		
	Engine.	Proposed.	Pr. ct. Diff.	Engine.	Proposed.	Pr. ct. Diff.	Engine.	Proposed.	Pr. ct. Diff.
b	63.300	61.900	— 2	61.200.	67.500	10	152.500	153.800	
c	45.500	44.400	— 2	50 300	55.800	11	137.400	137.700	
d	29.400	34.400	+ 17	42.500	47.500	12	127.200	124.700	
e	19.400	24.300*	+ 25	32.300	37.500	16	110.600	110.200	
f	13.100	14.400	+ 10	26.900	30.800	13	99.000	98.700	
g	.....	.....	....	19.600	22.500*	15	86.500	85.800	
h	.....	.....	....	14.800	17.500	19	76.500	75.800	
i	.....	.....	....	.....	.....	..	65.200	64.300	
j	.....	.....	...	.....	.....	..	56.700	55.700	
k	.....	.....	....	.....	.....	..	46.700*	45.700	
l	.....	.....	....	.....	.....	..	39.600	38.700	

We see that the percentages of excess available for impact in ties and counters decrease with the length of span, but are about right up to 200 feet, and, therefore, to make the loading cover all cases, we should add the specification that 10 per cent. shall be added to live load stresses in ties and counters of spans over 200 feet.

Table No. 4, for double intersection trusses illustrates a defective feature of the engine loading to which attention was called in the discussion of Mr. Wilson's paper alluded to, and shows how the proposed form of load, by covering the maxima of the various cases, meets the point. The centers of gravity of two consolidation engines in the positions specified are about 50 feet apart, and hence the maximum concentrations of weight will occur on the same system of bracing with panels of 25 feet, while with $16\frac{2}{3}$ feet panels they will occur on different systems and give less results than would be obtained with other positions or combinations of engines, a particularly unfortunate state of affairs when we consider that the latter panel is about a half a car length, throwing the maximum concentrations upon one system of bracing while we are calculating for a uniform weight of train, which implies an equal division between the systems, and, therefore, adding to the error. The proposed loading gives the maximum effects regardless of the panel length.

I have not gone into the question of the 101-ton consolidation engine loading, further than to make a comparison with Mr. Cooper's uniform loads for maximum moment, from which it would seem that the proper equivalent, impact included, would be 3 000 pounds uniform plus 50 000 pounds concentrated. As a substitute for the Lehigh loading I would suggest 4 000 pounds per foot uniform plus 70 000 pounds concentrated.

In the following table (No. 5) the equivalent values for the Lehigh engine loading for spans over 40 feet are the averages of those giving the

maximum middle moments and end panel shears. Although the agreement seems quite exact, in the calculation of trusses 3.900 + 70.000 will be found more nearly the equivalent load. The greatest disparity will be found in the chord stresses, attributable to the fact that while we take positions of the engines which we know give maximum web shears, they are positions which we know do not give the maximum chord stresses. For these the engines should obviously be preceded as well as followed by cars.

TABLE No. 5.

EQUIVALENT UNIFORM LOADS FOR MOMENTS.

Span.	101 ton consols. + 3.000 lbs.	$\frac{100.000}{S}$ + 3.000	Per cent. for impact	Lehigh Engines + 4.000.	$\frac{140.000}{S}$ + 4.000.	Per cent. for impact
5	12.000	23.000	92	16.000	32.000	100
10	6.750	13.000	90	9.610	18.000	87
15	6.670	9.666	45	9.600	13 300	39
20	6.190	8.000	29	8.560	11.000	29
30	5.470	6.333	16	7.290	8.700	19
40	4.760	5.500	16	6 690	7.500	12
60	4.110	4.666	14	5.630	6.300	12
80	3.950	4.250	8	5.270	5.750	9
100	3.910	4.000	3	5.070	5.400	6
150	3.740	3.667	—2	4.830	4 930	2
200	3.590	3.500	—2	4.680	4.700	
300	3.390	3.333	+2	4 410	4.470	
400	3.180	3.250	+2	4.300	4.350	
500	3.150	3.200	+2	4 230	4.280	

The high percentages in spans under 15 feet, as before remarked, will serve to cover the greater concentrations and speeds of passenger engines.

From a study of the three loads given it would seem to be a good rule to make the uniform load equal to the weight of the train, and make the concentrated load seventeen times that amount.

The actual wheel loads must, of course, be the basis of any adopted form of loading, and the idea of specifying the simpler equivalent is to avoid the enormous waste of high-priced labor involved in specifying the engine loading, where every engineer competing for the bridge must make elaborate calculations, which had better be made once for all by the party making the specifications, particularly where, as it has been shown, better results may be obtained.

Camber.—In the matter of camber it would seem better to provide for the deflection due to the web members by changes in the lengths of these members, rather than in the chords, as proposed in this paper.

In fact one cannot be made to cover the other because they do not act together, the deflection due to the web members increasing with the depth of the truss, while that due to the chords decreases.

The deflection of a truss being due to the change of lengths of all of its component parts by stress, why not anticipate this deflection by alter-

ing the lengths of all of the members, so that when sustaining its maximum load it shall be of the theoretical figure.

This is really the object of camber, and it would seem the scientific way to provide for it.

Thus, assuming 8 000 pounds per square inch as the average stress in the top chords and end posts, 6 000 pounds that in the intermediate posts, and 10 000 pounds that in the ties and bottom chords, and a modulus of elasticity of 24 000 000 pounds, the alteration would be as follows: Lengthen top chord panels and end posts $\frac{8\,000}{24\,000\,000} = \frac{1}{3000}$ length, and intermediate posts $\frac{1}{4000}$ + ($\frac{1}{50}$ inch play in pin holes). Shorten bottom-chord panels and diagonal ties $\frac{1}{2400}$ length + $\frac{1}{50}$ inch. This presumes that the web members shall be under their maximum stresses at the same time as the chords, which is not strictly true.

Let us consider the members in detail. The counter ties being adjustable are independent of alterations for camber. The main ties at the end are under their maximum stress at the same time as the chords; the main tie at the middle of a 200 feet span, for example, is under but 75 per cent of its maximum at that time. The average stress in the ties is, therefore, $87\frac{1}{2}$ per cent., making an error of $12\frac{1}{2}$ per cent. in the above assumption, which would be exactly compensated in the case considered by disregarding the play in the pin-holes of all the ties. The intermediate posts would be under a still smaller proportion of their maximum stresses, and the correction should be more properly $\frac{1}{4800}$ than $\frac{1}{4000}$.

The camber from the chords would be dependent upon the relative lengths of the top and bottom chords, and would obviously not be affected if we added $\frac{1}{50}$ inch to both chords in a common panel of say 25 feet. This would be equivalent to disregarding the play in the pin-holes of the bottom chord, and making the elongation of the top chord $\frac{1}{2400}$ in place of $\frac{1}{3000}$. But in doing this we react upon the web members by increasing the horizontal projection of those in one panel $\frac{1}{50}$ inch. This will be fairly compensated by neglecting the play of the pinholes in the posts.

We come, then, to the following simple rule which would seem to be as near theoretical exactness as can well be made, viz.: Camber shall be provided by changing the members of the theoretical figure of the truss $\frac{1}{2400}$th of their respective lengths except intermediate posts, in which the change shall be one-half this amount or $\frac{1}{4800}$th; compression members to be lengthened and tension members shortened.

The correction under this rule is simply $\frac{1}{2}$-inch per 100 feet for all members except intermediate posts in which it is $\frac{1}{4}$-inch. The fraction is such a small one that we can neglect the inches in the expressed lengths of the members, and the corrections can be written down from inspection; being a constant for each class of members in a Pratt truss,

the method is thought to be as easy of application as that given in the paper, besides being more correct and independent of the form of the truss and taking into account the play in the pin-holes.

I have taken the modulus of elasticity—24 000 000—as given by Mr. Cooper. This is too small, but provides an excessive camber of about 20 per cent., which it is desirable to have in view of the constantly increasing rolling load, and hence its use.

The members have been assumed to be iron, and when steel is used at higher stresses the elevations under the rule should be proportionately increased; but in the ordinary use of steel eye-bars at 12 000 pounds per square inch, with all other parts iron, it is more convenient, and I am inclined to think sufficiently exact, on account of the slightly higher modulus of steel and the excess which the rule gives, to apply the rule without alteration.

Palmer C. Ricketts, M. Am. Soc. C. E.—Whilst the credit for the development of the method, now generally in use in this country, of calculating the stresses in railroad bridges by wheel concentrations, is doubtless due to Messrs. Cooper and Escobar, the effect of engine wheel loads on spans had been considered abroad by Winkler, Von Ott and others some years before the date (1880) mentioned by the author as the time at which he first used it.

Mr. Cooper is somewhat in error in saying that this method has never been published before, since the principles and conditions as now used were given in an article by Mr. C. E. Moore, in the *American Engineer* of December 19th, 1884, and they have also been ably presented and extended by William H. Burr, M. Am. Soc. C. E., in his "Stresses in Bridge and Roof Trusses" (1886), in which, after a general treatment, he deduces stresses for special engine loads and spans. In both these cases credit is given to Mr. Cooper.

It is well, also, in this connection, to forget neither the valuable paper by Professor G. F. Swain, Assoc. Am. Soc. C. E., published in our Transactions of July, 1887, nor the Treatise on Bridges (1888) of Professor Merriman, M. Am. Soc. C. E.

Henry B. Seaman, M. Am. Soc. C. E.—I would offer a correction to the statement that it is the general American practice to brace our trusses for a definite amount of lateral force per lineal foot of structure. This rule is empirical and is not consistent with the American methods of scientific design. The specification quoted provides for a moving train, ten feet high, with a wind pressure of 30 pounds per square foot, but it makes no provision for the height of the train above the track, which is an important factor in the calculation of the moment of instability in trestle towers. In providing for a uniform pressure of 150 pounds per lineal foot of each chord, irrespective of the length, depth, or details of the trusses, it makes no allowance for the floor system at

the loaded chord, and in the case of check bridges where the floor system and the upper chord act together, the difference between this and the lower chord, composed of eyebars, is very marked. The specification was apparently framed with a view of lessening labor and to facilitate the manufacture of so many tons of iron in a certain limited time. As such, it has naturally found favor with contractors, and may generally be found in their specifications, though to this there is an exception in the specification cited by Mr. E. Claxton Fidler, which shows unusual thoroughness in its preparation and from which engineers have wisely made liberal abstracts. Of several railroad specifications in my possession, including those of the Pennsylvania Railroad and the Baltimore and Ohio Railroad, there are none which adopt this rule, but in each case vary the amount of wind pressure according to the surface exposed.

The objection to the use of engine distribution does not arise merely from the increased labor of the calculation of the strains. The chief objection is the over-confidence it gives in the validity of our results. There is no certainty that the concentrated weights assumed are the same as those to which the bridge is subjected by a moving train, and with the constant increase in the weights and the changes in their distribution, the expedient of engine distribution must be only a temporary one, while the tendency of railroad management is to increase the weights of the engines; there is an equal and less restricted tendency to increase the weights of the trains which follow; and as long as the co-efficient of sliding friction is greater than that of rolling friction, we have no assurance that economy of transportation in reducing the lengths of trains and the increased facilities of our manufacturers requiring the transportation of heavy weights will not ultimately give us trains of equal weights with the engines. It is just announced that a special car has been constructed by the Lehigh Valley Railroad for the transportation of heavy machinery, while two years ago a similar car was constructed by the Pennsylvania Railroad for the transportation of cables. The only limit to be placed upon these loads and their concentration is the strength of the bridges and the solidity of the track. Is it then wise to devote our attention to the detailed effects of engine distribution, ridiculing our factor of safety, while we ignore conditions which already exist and of which we have no knowledge?

C. F. Stowell, M. Am. Soc. C. E.—The value of the historical portion of Mr. Cooper's paper can hardly be over-estimated. He has collected and recorded a mass of facts relating to the early history of American bridge building, which was in danger of becoming permanently lost.

It seems to me that it would have been well to include in his list of early iron bridges the one over the Mohawk River at Utica, on the Rome, Watertown and Ogdensburg Railroad, as being probably the

oldest iron truss bridge now in use for railroad purposes in this country. This bridge was built by the late Squire Whipple in 1854, and is understood to be the second railroad bridge built by him on his well known trapezoidal plan, the first being the one mentioned by Mr. Cooper, on the Rensselaer and Saratoga Railroad, near Troy, and which was removed in 1883. The Utica bridge is 125 feet long, and has cast-iron top chords and posts, rods with screw connections for tension members, and welded links for lower chords. Some time after being erected the span was carried away by a flood, but the pieces were picked up and replaced, and the bridge is doing duty to-day in substantially the same condition as when first built, except that a new and stronger floor system has been put into it.

As regards the date when the modern type of bridge specification first appeared, I think Mr. Cooper is somewhat in error. He claims priority in a number of points for the old Erie specifications drawn up by him in 1878, but I find that the "Specifications for Wrought-iron Bridges" of the Lake Shore and Michigan Southern Railway, which were published at least as early as July, 1877, cover substantially the same points. These specifications covered the designing, proportioning and detail of construction with that completeness necessary to give the railroad company full advantage of the competitive system, and at the same time insure that the resulting structure would not be below the standard. They specified definitely the working strains to be allowed on different parts of the structure, according to the service to be performed, and while stating clearly the exact loads and working stresses to be used and degree of excellence of workmanship to be adhered to, they still left the contractor at perfect liberty to adopt such style, depth and panel length of truss as might seem to him most advantageous. In view of these things, therefore, I think Mr. Cooper errs in claiming for the Erie specifications of 1878 priority in the points just mentioned, and which are now universally incorporated in all the American bridge specifications of any degree of merit. The Lake Shore specifications were drawn up by the late Charles Hilton, under the direction of Mr. L. H. Clarke, then Chief Engineer of that road.

The remainder of Mr. Cooper's paper, after the historical portion, is largely occupied with a description of the processes of computation, manufacture, erection, etc., in common use in American bridge shops, and while in the main accurate and reliable, still this portion contains a number of statements laudatory of the pin-connected type of bridge, and derogatory to its riveted congener, to which, I think, exception may reasonably be taken.

The point usually insisted upon most strenuously by the advocates of the pin-connected system is the asserted ambiguity as regards the stresses in a riveted bridge compared with a pin-connected one, but as Mr. Cooper does not urge this objection I have nothing to say about it

here, except that it is usually much exaggerated. It is not true, as has been asserted by a prominent advocate of the pin-connected type, that it is utterly impossible to make any estimate of the strains in a riveted lattice truss, nor is it true that the stresses in a pin-connected truss can be computed with absolute exactness in every member. There is more or less ambiguity in the stresses in every bridge, and the disparity between riveted and pin-connected ones in this respect is less than some would have us believe.

As an objection to the use of riveted construction for long spans, Mr. Cooper says, "for the larger spans the necessity of performing so much riveting of important connections at the bridge site, where that care and accuracy obtainable at the shops cannot be depended upon, does not render them acceptable to many engineers." I know of no more vital or important connections in a bridge than those of stringers to floor beams and of floor beams to the trusses. These connections must resist the first shock of a locomotive as it jumps upon a bridge, and they get very much more wear and tear and rough usage than any other connections in the bridge. But rivets are always depended upon in first-class, modern, pin-connected bridges for these connections, and not only this but the stringer connections always and the floor beam connections frequently are necessarily made by means of hand-driven rivets at the bridge site. As long as the primary dependence of a train on a bridge always has been and always will be upon hand-riveting done at the bridge site, it seems to me inconsistent to object to "so much riveting of important connections at the bridge site."

The facts about the erection of pin-connected bridges given by Mr. Cooper are certainly remarkable, especially if we are to understand that the spans mentioned were riveted up complete ready for track-laying in the periods stated. But if he means by "erection" the putting together of a truss sufficiently to make it self-supporting, and then doing the necessary riveting at leisure afterwards, the feats mentioned lose somewhat of their brilliancy. It is undoubtedly true that to erect a riveted bridge complete takes longer than for a pin-connected span of the same size, and this is due to the extra time required for riveting. But simply to render a truss self-supporting so that the false work may be removed if necessary, is quite another matter. If this be called "erecting," I am of the opinion that the records of riveted bridges where speed is essential would compare favorably with those of the other type. It is not necessary that all or any field rivets be driven for this purpose. Only a few bolts are needed. It is not even necessary that all or nearly all the members of a truss should be in place. I know of one case where the false work was carried from under a riveted span of considerable size before any of the top chord was in place, but the truss stood without a top chord. A pin-connected truss must inevitably have fallen. Moreover, it should be considered that in the

erection of a riveted lattice bridge no false work is ever required above the lower chord, so that all the time and expense of carrying false work up to the full height of the trusses, as is essential in pin-connected spans, is saved in using riveted work. In cases where it is not desirable to obstruct a waterway at all by false work a riveted bridge may be, and frequently is, built entirely on shore and shoved across when completed. Thus the York street bridge in Buffalo, a 182 feet span over the Erie canal, was built in the street to avoid interfering with navigation by false work, and pushed across when fully erected, the outer end being supported by a canal boat during the transit. A riveted bridge requires no special design or extra members whatever to enable it to be shoved about in this way. After all, though it is true that speed in erection is sometimes of the utmost importance, nevertheless such cases in ordinary practice are comparatively rare. In a great majority of cases any reasonable length of time which may be desired can be taken.

In looking back over the field of American bridge building during the past few years I am fully persuaded that the use of riveted work is gradually becoming more extensive. This is shown (*a*) by the general extension of the limiting span for which riveted trusses are permitted on roads which do not employ them for all spans, (*b*) by the greater dependence on riveted connections in various parts of pin-connected bridges, and (*c*) by the adoption of the riveted type for long span bridges by roads which formerly preferred the pin-connected type. The Erie specifications of 1878 required riveted girders to be used for spans of from 40 to 75 feet, and prohibited them for longer spans. The present Erie specifications require their use up to 80 feet, and for longer spans they are optional. Mr. Cooper's own specifications, dated 1888, require them up to 100 feet, though in 1884 his general specifications put the limit at 90 feet. In 1878 it was considered good practice to depend upon abutting joints in top chord sections with just enough rivets to hold the pieces in place; but now it is stipulated in all good specifications that "all joints in riveted work, whether in tension or compression members, must be fully spliced, as no reliance must be placed on abutting joints." The old Erie specifications required for floor beam hangers only that they should be so placed as to be readily examined, that check nuts shou d be used for screw connections and that bent loops should fit closely around the pins. The use of plate hangers for floor beams, where the dependence is necessarily on rivets, is now almost universal unless special circumstances require some other form. The modern Erie specifications read, "Floor beam hangers [*i. e.*, bent loops or screw connections] shall be avoided if possible," and Mr. Cooper's own specifications of 1888 say, "Preference will be given to hangers without screw connections."

When the Erie specifications of 1878 were published the bottom lateral bracing of through pin-connected bridges was always com-

posed of round or square bars with pin or screw connections, in the attachments of which, by the way, the specified prohibition against using rivets in direct tension was almost always conveniently disregarded. But in Mr. Cooper's 1888 specifications, paragraph 8 says: "Preference will be given for a stiff angle-iron lateral system between the chords on the level of the floor." And again in paragraph 24: "Preference will be given to lateral bracing in the floor system, which is capable of resisting both tension and compression." In first class work the practice of stiffening the lower chord in the end two panels of pin-connected bridges is now becoming common. The Lake Shore and Michigan Southern Railway, for instance, makes it a specified requirement in all cases where the first panel point is a suspender only. These things go to show that not only is the limiting span for which riveted girders proper may be used being gradually extended, but that the use of riveted construction and the dependence in pin-connected bridges upon riveted connections, where other forms were formerly considered better, is becoming more and more common. In all the points mentioned where riveted work is making inroads into pin-connected construction —in the use of fully spliced top chord sections, in the dependence of floor beams upon riveted connections instead of bolts and nuts, in the use of stiff lower chords and of rigid lateral bracing—in all these things the tendency is precisely towards the practice which is now and has been for years universal among the builders of riveted bridges. But besides this the use of riveted work is becoming more general for entire spans even of considerable size on roads which until comparatively recently used only pin-connected bridges. As an instance of this I need only mention the new bridge over the St. Lawrence River now being built by the Canada Atlantic Railway. This bridge will comprise one span of 163 feet, two of 190 feet, ten of 217 feet, four of 223 feet and one draw span of 355 feet in the clear. All the iron bridges heretofore built on this road are pin-connected, but this bridge, I am informed, is to be entirely of the riveted lattice type, the management of the road having wisely decided upon that style of construction as presenting a greater measure of safety in the event of a train accident on the bridge.

Even admitting all that the advocates of the pin-connected type have claimed in favor of their structures and against the riveted type to be true, still it seems to me that the latter would be preferable for railroad purposes, for the single reason indicated as governing the choice in the case of the Canada Atlantic Bridge.

The paramount consideration in the building of any bridge must always be safety. No matter to what degree of refinement the computation of stresses may be carried under a system of wheels placed just so many inches apart, and just so many pounds on each wheel; no matter how careful the inspection of material and workmanship or how much pulling of test pieces may be done; no matter how many engineers may

have pronounced the finished structure as possessing the utmost degree of excellence, still I maintain that if some other type of bridge proves itself in actual use capable of withstanding the ordinary accidents of railroading better than this, then it is a safer and a better bridge. The Apple River Bridge on the Chicago, St. Paul and Kansas City Railway illustrates this point very well. This bridge was a single span of 180 feet. It was built with all the refinements known to modern practice. The stresses in all the members were computed to a nicety, and the dimensions of the parts proportioned according to accepted rules. The workmanship and material were subject to rigid inspection and the testing machine added its evidence that the pieces broken in two were of the proper standard. When finished, this bridge might well stand as a good example of what is generally considered the most approved American practice. It did so stand for about one month, when the breaking of an axle under an ordinary freight car suddenly put an end to its usefulness. This axle broke some 3 600 feet from the bridge, and dragged harmlessly over the ties for that distance. But as soon as it struck the open spaces between the bridge ties it stuck, the car to which it was attached "slewed" around, the trusses were hit, and the whole structure fell into the stream below. The breaking of an axle is by no means an uncommon occurrence, and is as likely to happen on a bridge as anywhere else—more likely, some think. The breaking and dropping of a brake-beam or draw-head is another common accident. So is the shifting of a load or the falling off of some article not sufficiently secured on a platform car. I have records of pin-connected bridges wrecked by all of these common accidents, but after years of diligent inquiry I have failed to find a single instance where a riveted bridge fell in consequence of any such accident, though I have a great many records of these accidents happening upon such bridges.

Mr. Cooper asks, "Does any one advocate the designing and building of bridges to withstand the impact of a railroad train or the bursting effect of piling two trains on one another inside the trusses?" I answer, that as long as trains run into bridges or cars pile up inside the trusses, a bridge to be safe must be designed and built to withstand just those things. The Erie Canal Bridge at Tonawanda would have been in a bad way on that January day in 1873, when the train ran into it, had it not been able to withstand the impact. On that day a spar was being drawn across the track at a road crossing near the bridge. It sagged considerably in the middle, and as the railroad track was higher than the road on either side, it "grounded" on the track. Just then a train came around the curve, and, the distance being too short to stop, struck the spar, shoved it along against the bridge and finally came to a stop after shoving the whole span bodily some little distance on the abutments. The pilot, head-light and smokestack of the engine were smashed, but the bridge was not appreciably injured, and after being pushed back

into place continued to be used, and is used to-day, without further repairs. It is a riveted lattice bridge, about 100 feet long. Now, by way of contrast, the 150 feet span, pin-connected, Pratt truss bridge at Fish's Eddy, on the New York, Ontario and Western Railway could not withstand a glancing blow from a derailed caboose which was being dragged along at the tail end of a freight train. This bridge was built under specifications substantially the same as those of the Erie Railroad of 1878, but with a very liberal allowance of material, so that the span was a heavy one. It collapsed under the blow in March, 1886, and dropped into the stream below, with five cars.

If bridges could not be built to withstand the piling up of cars inside the trusses, then the Delaware, Lackawanna and Western Railroad Company's bridge over Wappaseuing Creek, near Nichols, N. Y., would have come to an untimely end last year when the freight train ran into a preceding coal train upon it. The cars were piled up to two-thirds the height of the trusses, some of the web members broken off, and others badly bent in both trusses. But the trusses stood, because the members were rigidly riveted together, though it is not a particularly good specimen of a riveted bridge either. On the contrary, when the collision occurred between the two sections of a broken Erie freight train on the bridge over the Delaware and Hudson Canal, near Port Jervis, in 1882, the cars piled up and the bridge went down. It was a pin-connected bridge, and considered a good one too.

To say that such accidents are to be classed as failures of management and not of bridges is only begging the question. I apprehend that as long as a bridge goes down it is of little consequence to those most interested, whether they be the people aboard the train or the stockholders who foot the bill, whether the fault lay in the bridge or the management. If a bridge is properly constructed a fault of the management will not destroy it.

In regard to the list of bridge failures, the publication of which Mr. Cooper so strongly deprecates, as I am responsible for the collection of these statistics I may, perhaps be permitted to offer a few statements in their defense.

Mr. Cooper objects to these reports because they "are not based entirely on official reports, but are collated from the newspapers." This is by no means true. In every case where a bridge accident has been brought to my notice I have used every possible means to verify the facts reported. If by "official reports" Mr. Cooper means the reports of any public officials who may have power to investigate accidents, his statement is incorrect, for such reports have invariably been largely relied upon wherever accessible. But Mr. Cooper must be aware that only in comparatively few instances is the failure of a bridge ever made the subject of public inquiry. If he means by "official reports" the reports of railroad employees to their superior officers, then his state-

ment is in the main correct, for a railroad company is not willing, as a rule, to furnish for publication any facts relating to an accident on its own line, especially if due to a defect in its road or equipment, or to a fault of its management. Other sources of information which I have used besides "official reports" are the files of local newspapers very often; photographs of wrecks whenever obtainable; personal correspondence with individuals in the neighborhood in almost every instance, and occasionally the testimony before a coroner's jury. The assertion that these reports of mine are "undoubtedly full of sensation and inaccuracies" is a gratuitous piece of misinformation, and it is to be regretted that such a sweeping charge should be publicly made without at least some attempt to ascertain how much basis there might be for it. I have never claimed absolute accuracy for any such lists of bridge failures, though I have never published as a fact any statement which I had not reasonable grounds for believing to be true, Mr. Cooper's assertion that the "data * * * have been magnified and distorted" to the contrary notwithstanding. In spite of all attempts I have sometimes found it impossible to get any information from any source about certain accidents. In every published list of accidents which ever came from my hands attention has been called to these doubtful cases, and correction and criticism invited. As the result of such invitation, after the list of two hundred and sixty-five failures for ten years ending December 31st, 1888, was published, to which Mr. Cooper refers, I was advised of one error in the case of a wooden bridge reported to have fallen from some unknown cause. This proved to be a trestle, and as the list was not intended to cover these structures the total number of failures was reduced to two hundred and sixty-four. This correction was published in the same journal in which the original list appeared, but seems to have escaped Mr. Cooper's attention.

Mr. Cooper lays much stress on the fact that only ten iron bridges are positively known to have squarely fallen down in the period mentioned. An American iron bridge seldom falls through sheer weakness. It never does except in consequence of some defect of design, manufacture or maintenance, which engineering skill or intelligent inspection could have detected. The most common cause of failure among American iron bridges is a blow either from some projecting part of a passing train or from derailed cars. In the list of two hundred and sixty-four failures referred to sixty were known to have been caused by blows. Of these bridges eleven were of wood, twenty of iron and twenty-nine of unknown construction. If the same proportion between wood and iron holds among the unknown as among the known cases we may reasonably estimate that forty spans out of the sixty which were knocked down were of iron. Moreover, the list includes sixty-five bridges of unknown construction which failed from unknown causes. It is altogether probable that some of these were of iron and were knocked down.

Every one of the twenty iron bridges known to have been knocked down was of the pin-connected type. They were of all ages, from one week to many years. They represent the engineering talent and manufacturing skill of many of the most prominent bridge building establishments in America. Some of them were on roads especially noted for the care displayed in the construction and maintenance of bridges, and were furnished with all the safety appliances known to modern practice. As long as pin-connected bridges continue thus to fall, even though they be of the very best design and workmanship, while riveted bridges have repeatedly stood precisely the same kind of accidents and even worse ones without serious injury, I believe the latter are safer and therefore better. I have never known a pin-connected bridge to stand after its end post had been broken in two, but I know of at least two such cases of riveted bridges. I have never known a pin-connected bridge to stand after being shoved out of place so that one corner was pushed off the abutment, but there are a number of cases on record where a riveted bridge has thus stood "on three legs" as it were.

I believe with Mr. Benjamin Baker that "it is clearly necessary to provide for the contingency of a train accident on a bridge." I believe that the only way we know of as yet to accomplish this result is by using riveted connections, and that the time will come when the failure of an iron bridge from an ordinary accident of train service will be regarded as discreditable to its builder and not excused as a fault of the management. Until that time comes we may expect to see published occasionally lists of bridge failures in which the number of iron pin-connected bridges knocked down will be a considerable item, however distasteful the publication of such lists may be to the builders of those bridges.

In closing his treatise on the theory and practice of bridge building, the late Squire Whipple uses these words: "After all, practical test is generally the only satisfactory means of determining the value and utility of any mechanical device." It is when judged by this criterion that the merits of riveted construction show at their best.

C. L. Strobel, M. Am. Soc. C. E.—Mr. Cooper's paper contains a fair and full exposition of the merits of the American type of bridge construction. After many years' trial, the American pin-connected truss is as firmly rooted in favor among engineers who have had experience with it, and who are competent to judge it, as it ever was. The arguments which have been advanced in favor of the riveted lattice as against the pin-connected type of truss, are also applicable in favor of the multiple-system pin-connected truss as against the single system. That these arguments have not found favor among bridge engineers is proved by the fact that the tendency is universal to design trusses of both the pin-connected and the riveted types as single systems, thereby reducing the number of parts and increasing the definiteness of strain

in each part. The verdict of engineers has been, that the advantage claimed in favor of a multiple-system truss, viz., that it offers greater security in case of accident to train, is illusory and of minor consequence compared with the counter advantages which a single-system truss offers, the most important of which is, that it is better suited to take care of the normal or every-day service for which a bridge is primarily built.

As Mr. Cooper has pointed out, vibrations can be guarded against in a pin-connected truss so that they will not be greater than in a riveted truss; therefore this feature has little or nothing to do with the comparative merits of the two types.

A properly proportioned pin-joint never gives trouble, but a properly proportioned riveted joint, for which the riveting must be done in the field, frequently does, for the following reasons:

First.—The riveting must be done by hand.

Second.—Field riveters, generally speaking, are inferior to shop riveters in skill. As high a degree of discipline cannot be maintained among the former, and supervision and control are more difficult.

Third.—It is frequently difficult to do field riveting properly because of inadequate space and awkard positions in which the men have to do their work, or the interference of trains or bad weather.

Fourth.—There will be loose rivets, notwithstanding all precautions that can be taken, so much so, that it is a practice much to be recommended, to go over the work after the lapse of a certain time, say a year, to replace these.

The methods for determining the maximum shears and maximum moments for wheel loads in a single system truss described by Mr. Cooper are much older than the year 1880. I have used the method for determining the maximum shears since 1874, but both methods had been taught in German polytechnic schools prior to that. It cannot be successfully maintained that the method for determining the maximum moments is as short of application as the method of equivalent loads. Furthermore, the former methods are not applicable to draw spans, nor to double intersection trusses, still much used and a very advantageous form in some cases, nor to the Petit truss, of late years preferred for long spans. To overcome these difficulties and to shorten the work of computing the strains, I adopted, about the year 1879, a method of calculating the strains in trusses by assuming an "equivalent" uniform load so determined that the end shear (*i. e.*, the vertical component of the strain in the end brace) obtained from it would be the same as the maximum end shear for the actual train load. At that time the engine load generally specified was much heavier than the freight load is, yet it was found that the strains obtained by this method were close approximations to the strains obtained from the train load, except that in the chords of trusses exceeding 150 feet in span, the former method

gave an appreciable excess in the middle panels. This excess is about 5 per cent. for a 200 feet span of eleven panels, Erie train load. To overcome this difficulty I used an equivalent load for the chords of spans exceeding 200 feet made up of the freight load covering the entire span, and an excess load of about the length of two engines of such value, that again the end shear was the same as the maximum end shear for train load.

At the present time the difference between engine load and freight load is very slight, some specifications even prescribing a heavier freight load than engine load. That being the case, the errors resulting from a calculation by the method of equivalent loads, using only one series of loads, are inappreciable, and it is no longer necessary to use excess loads for the chord strains of long spans.

Mr. Cooper's table showing the equivalent loads obtained from center moments, from "double shear" and from end shear, is misleading and erroneous. It applies to Erie train load, which is not the usual train load specified at the present time, and, because the difference between engine and freight load is great, would make, if possible, an unfavorable showing for the method of equivalent loads. The column giving equivalent loads from moments is not correct for spans of 80 feet and over, *i. e.*, for the truss spans. For these spans the loads given in this column correspond to the end shear of trusses having panel lengths of 15 to 18 feet, and not to the center moments. The panel length has, of course, some influence on the value of the end shear, it being greater for short than for long panels. This column appears to be taken from the specifications of the Keystone Bridge Company, where the equivalent loads were determined in two ways, viz., from center moments for plate girder spans (70 feet and under), and from end shears for truss spans (80 feet and over).

Mr. Cooper's "end shears" are apparently the shears next to the support. There is no use for these shears in the calculation of strains for a truss, and for a plate girder. These end shears are used only to determine the thickness of web at the end of the span, also the strength of rivet connections, stringers to floor beams. No one would have difficulty in making the proper allowances to determine these correctly from one series of equivalent loads.

As regards the column of equivalent loads from "double shear," to be used in the calculation of floor beams and trestle columns, the "span" has not been assumed properly. It must be taken equal to two panel lengths for floor beams, and to two spans for a trestle column, *i. e.*, to the distance between columns on either side of the one considered. Making this correction there will be found very close agreement between the values of Mr. Cooper's second and first columns, where before there was great divergence (see the following table), and it follows, therefore, that the method of equivalent loads, using one series only, is also applicable

to the calculation of floor beams and trestle columns, and consequently also to vertical suspenders in trusses.

EQUIVALENT LOADS FOR ERIE TRAIN LOAD.

1	2	3	4	5
Span, Feet.	Number of Panels.	From moments to 70 feet. From single shear for 80 feet and upwards. Cooper and Keystone Bridge Company.	From double shear. Cooper transposed. (To be used for floor beams and trestle columns.)	From single shear. No panels. Cooper. (To be used for rivet connections of stringers and to determine thickness of web at ends of plate girders.)
5	....	8 800	.....	9 680
10	....	5 290	5 280	7 200
15	....	5 280	.....	6 240
20	....	4 640	4 620	5 400
25	....	4 400	.....	5 040
30	....	4 200	4 210	4 740
35	....	4 080	.....	4 540
40	....	3 850	3 830	4 300
45	....	3 700	.....	4 220
50	....	3 590	3 588	4 240
55	....	3 450	.....	3 930
60	....	3 400	3 400	3 830
70	....	3 220	3 270	3 680
80	5	3 260	3 175	3 620
90	6	3 250	3 090	3 570
100	6	3 230	3 030	3 450
125	7	3 150 (3 231)	.....	3 280
150	9	3 070 (2 913)	.....	3 180
175	10	2 990 (2 852)	.....	3 090
200	11	2 910 (2 768)	2 750	3 000

The figures in parentheses in third column are the equivalent loads from center moments.

In the above table the difference between the figures in third and in fourth columns show the errors that will be made by calculating floor beams and trestle columns by the use of the equivalent loads given in column 3. For column 4 the panel length is considered as one-half the span length.

Mr. Cooper's method of providing for the camber by a fixed addition to and deduction from the lengths of the chords, is not, in my judgment, sufficiently accurate for all cases. By this method a larger camber is provided to take care of the deflection due to the changes of length of the web members, for a shallow than for a deep truss, whereas the deflection is the reverse, and for trusses of irregular form (chords not parallel) or those of the Pettit order, difficulties are encountered to determine the proper lengths of the web members. For large bridges I have preferred the following method, which is both simple in application for all forms of trusses, and accurate as well.

It is assumed at the outset that the main object of providing camber is to make the truss take its most advantageous form when fully loaded, *i. e.*, when the chords are strained to their maximum. This condition is

attained, for a Pratt truss, when the posts are plumb and the chords are in straight line under full load. The top chord joints will then be closed theoretically, the abutting surfaces bearing squarely upon each other. The camber lengths which will give this condition are obtained by first calculating the lengths of the members for the normal form of the truss, which it is to assume under full load, and lengthening and shortening each member according to the changes of length produced by the strain it receives under this load. The chord strains for full load are always known, as they are the maximum, but the strains in web members for this condition of loading have to be calculated, which is easily done. If the posts and suspension bars are of the same length respectively, it will be desirable to take an average strain per square inch for all the posts, and the same for all the bars, to avoid having these lengths vary slightly according to the different strains per square inch, corresponding to this loading, but for trusses with variable lengths of web members this approximation need not be made.

In conclusion I wish to express my high appreciation of the admirable manner in which Mr. Cooper has treated his comprehensive subject.

J. A. L. Waddell, M. Am. Soc. C. E.—Mr. Cooper's paper is most timely in that it refutes the erroneous conclusions of English engineers as to the alleged inferiority of American railroad bridges in comparison with those of English design and manufacture, which conclusions were drawn from an ill-advised paper read a short time ago before the Institution of Civil Engineers by an American engineer. It would have been much better for all concerned, if that gentleman had presented his views on American bridges to an American society, where his statements and deductions would have received due comment, and where the effect of his paper would have been productive of good; instead of submitting it to those who are only too ready to severely criticise and throw discredit upon the work of their professional brethren in this country. No one is more ready or willing to expose faults in American bridges than I, nevertheless, I contend, and have in the past maintained, that the modern American railroad bridges are the best bridges in the world; not only because they are designed according to strictly scientific principles, but also because they are detailed in a thoroughly practical manner with reference to simplicity, economy and effectiveness.

In comparison with them the bridges of English design, which I have seen and read of, appear to be very crude affairs. The English bridges which were being built three years ago in Japan involve more serious faults of design than do the pioneer iron bridges of the United States, that are now being taken down and replaced by modern structures. And yet these Japanese bridges were designed, or were supposed to have been designed, if the name-plates which they bore were reliable, by one of the most prominent English members of the Institution of Civil Engineers.

That American railroad managers have been so short-sighted and parsimonious as to let bridges by the span in keen competition without employing the services of a consulting engineer, and that many structures in consequence are both light and defective, is no reason for English engineers to claim superiority over their American brethren, when the former, while under no pressure from competition, but on the contrary, with *carte blanche* as to the use of metal, design such structures as those sold to the Japanese Government. Was an American bridge engineer ever guilty of designing a 100 foot span railroad truss-bridge without a single diagonal lateral member in the structure? And yet the standard railroad bridges of Japan three years ago were built in this manner! Nor was this their only fault of design—the trusses were entirely unsupported against overturning, except by small fish-bellied floor beams which were most inefficiently riveted at their ends to the bottom chords. As these beams were only, if I remember rightly, 8 inches deep at their ends, their actual strength when tested to destruction would probably be very low indeed.

The limiting length of span on the Japanese roads under control of English engineers was for a long time 100 feet; but some three years ago there was built over the Karasugawa a bridge with spans of 209 feet; and, *mirabile dictu*, the design for said spans was merely an amplification of the design for the 100 feet spans, except that overhead struts without diagonal laterals were adopted. However, when the drawings were sent to England for approval, the upper lateral rods were put in; but the bridge was built without a single lower lateral rod in its whole length.

The accompanying photograph illustrates very well the character of the structure (Plate XCIII). The truss depth appears to be 16 or 17 feet, which is about what English authorities regard as the economic depth for a girder of this length. American engineers would make such spans fully twice as deep for economy. A comparison of web and chords in the photograph will convince one that most of the metal is in the latter; and it is well known to American engineers that with parallel chords the greatest economy of material is obtained when the weight of the web is about equal to that of the chords. European engineers and technical writers maintain that the economic depth of truss varies from $\frac{1}{8}$ to $\frac{1}{12}$ or even $\frac{1}{14}$ of the span, while American practice shows it to be from $\frac{1}{5}$ to $\frac{1}{7}$ of the span—just about twice as great. Again, English engineers adopt very short panels, while American engineers use long ones. Both theory and practice prove that the latter are superior to the former in respect to every important consideration, viz.: economy of material, rigidity and facility in erection.

Secondary strains due to eccentric action of stresses in both tension and compression members are a very common feature in bridges of English design—not only in short spans but also in the longest and heaviest

manufactured. Professor W. C. Kernot, M. Am. Soc. C. E., of Melbourne, Australia, in *The Engineer* of June 5th, 1885, pointed out how in the curved top chord of an important Indian bridge the intensity of working stress varies from zero on one side to twice the average on the other, thus making the member only one-half as strong as it was estimated.

For quite a while a certain American engineer has been advocating in the engineering papers that the building of pin-connected spans be abandoned, and that lattice girders be used in their stead. Such action would be retrograde and entirely uncalled for, because pin-connected spans can be designed so as to embody all the advantages of riveted girders without any of their numerous disadvantages.

The principal plea of this gentleman is that pin-connected bridges when run into by a derailed locomotive or car will collapse, while the lattice girders, on account of their multiple system of cancellation, may not. But as it is practicable to design all railroad bridges so that the trusses will not be struck by derailed trains, or any permanent part of the structure be injured thereby, it would be folly to adopt a style of truss that had so many objectionable features as has the lattice girder. The most important of these are the following: Ambiguity of stress distribution on the several systems of triangulation, secondary strains due to fixed ends of web members, the use of single angle irons for diagonals, eccentric action of stresses in chord members, unavoidable ambiguity of stress distribution on connecting rivets, excessive amount of field riveting, and in long spans the unnecessarily great dead load caused by loss of section from rivet holes, the great number of parts, and the large amount of what may be termed idle metal.

Bridges can be properly protected against injury by derailed trains in the following ways:

First.—Widening the clear distance between trusses to 16 feet. This would also reduce the danger from shifting loads, such as piles, that occasionally strike the trusses.

Second.—By adopting an efficient design for the floor system proper, (or ties, rails, guards and their attachments).

Third.—By having a re-railing apparatus at each end of every bridge, with ditching apparatus at a convenient distance from each end, also a center rail extending from re-railing to ditching apparatus.

Fourth.—By driving two very heavy protecting piles in the embankment at each end of each bridge, and bracing them effectually, so as to break up or push back any unduly projecting portion of a car load.

General managers and superintendents of railroads are much to blame for the improper styles of floor system used on many American lines. A few extra dollars spent on each span would change a bad floor system into a good one, because a first-class floor system, or rather that portion of it above the stringers, can be built at a cost of four or five dollars per lineal foot of track.

Vol. XXI, p. 598. KARASUGAWA BRIDGE, JAPAN. PLATE XCIII.

All the old iron bridges in present use should be thoroughly examined and investigated by experts; then, if found deficient, as is liable to be the case, repaired or replaced as may seem advisable. I have had occasion to examine a number of iron bridges in the West, which were built fifteen or twenty years ago, and have found them invariably dangerously weak in important details. Unless speedily removed, some of these old iron bridges will surely cause accidents that may prove to be catastrophes.

Railroad managers, in order to insure obtaining really first-class bridges for their roads, should either have complete detailed drawings and specifications concerning quality of material, workmanship, &c., prepared by competent bridge specialists, then let the contracts on schedule prices and engage competent inspectors to look after the material during manufacture and erection; or should call for tenders upon standard specifications, submit the papers of all the competitors to a bridge specialist, and let him award the contract, then have the contractor submit complete working drawings to the expert for approval before manufacture be commenced; and finally, as before, arrange for proper inspection.

It is decidedly bad policy to let bridges by the span, owing to the temptation which the contractor is subjected to in regard to saving metal, and thus scamping the work. On the other hand, if bridges be let by the pound without any supervision by a bridge engineer, they are liable to cost an excessive amount, to say nothing of the chance of some of the details being imperfectly designed.

Every railroad company should either have in its employ a competent bridge engineer, or should retain some specialist as consulting engineer on all its important bridge work.

In order to become a good bridge designer, an engineer should have experience in the employ of both contracting bridge company and railroad company, for with the first he will learn economic methods of design, and with the second he can observe how bridges act under loads, and what are good and what are bad details.

The effect of keen competition upon bridges, even when standard specifications are used, is often prejudicial, in that, for the purpose of saving a little material, the designer will go to extremes, thus impairing some one or more of the attributes of a first-class structure. It is here that the usefulness of the consulting engineer becomes evident; for he would rule out in competition such extreme designs. The various effects of wind pressure are not completely treated in some standard specifications, consequently bidders will naturally take advantage of such omission, although as engineers they know that it is not proper to do so.

My personal experience as contractors' engineer, and conversations that I have had with bridge companies' computers, cause me to differ with Mr. Cooper in respect to the facility of application of the moment

diagram in ascertaining stresses in bridge trusses. For short spans, square bridges and panels of same length throughout, that method is certainly the best; but for long span bridges with sub divided panels and inclined top chords, skew bridges and spans different from the ordinary, the uniform load method, with or without engine excess, is far preferable. It is most exasperating to a computer to be compelled to try four or five different positions of the moving load in order to ascertain which one will give the absolutely greatest stress on a member of a sub-divided panel truss, when he knows that there will be hardly any difference in the stresses for all the positions which he has to try. With a uniform load and two engine excesses separated by two panel lengths (about 50 feet), one calculation would suffice. It would not do for the computer to guess at which position of the train will give the greatest stress, for the railroad company's engineer might throw out his design from competition on account of not being quite up to specifications.

The method which I have adopted in designing long span railroad bridges is to use the moment diagram for the floor system and primary truss members, an equivalent uniformly distributed load for the chords and inclined end posts, and the uniform car load headed by one engine excess and accompanied by another two panel lengths distant from the first for the main web members. This method reduces the labor of computation to a minimum, and at the same time gives results that are exact enough to satisfy any but a very captious engineer.

Mr. Cooper makes a very good point when he advises that we do not increase our unit strains in proportion to our faith in new material—especially in reference to short spans, which are nearly always made too light for rigidity. Moreover, steel, although stronger, is no stiffer than wrought-iron; and as, for short spans, and the floor systems of long spans, rigidity is even more important than is strength, it would, in my opinion, be well to use therein the same intensities of working stresses for both steel and iron. At the same time, in the trusses of long spans, I am in favor of straining steel as high as good judgment, based upon adequate testing, would indicate.

At the risk of being considered tedious, I would again urge upon the profession the organization of an association of bridge engineers and builders for the determination of complete standard specifications for railroad bridges, and for the testing of new forms of members and new material. Surely the Government of the United States could be prevailed upon to spend a considerable sum in testing not only material for and full-sized members of bridges, but also the complete bridges themselves! We do not yet know enough about the effect of impact upon structures and upon the members of structures, and it is going to cost money to learn it.

Could not the American Society of Civil Engineers persuade the

on same [illegible]

Probably Mr. Cooper, when he wrote [illegible] is Erie specifications of 1878, was unaware of the specifications for the Kilbourn and the Rockton Bridges, the first issued in July, 1877, and the last in October of the same year, both esteemed by many engineers as "covering the designing, proportioning and detail of construction with that completeness necessary to give the railroad company the full advantage of the competitive method with a certainty that the resulting structure would in all ways be up to the advanced state of the art."

The Kilbourn Bridge specifications soon after their issuance were made the subject matter of a paper by C. Graham Smith, President of the Liverpool Society of Civil Engineers, which paper was republished by an engineering journal in this country soon afterwards.

A copy of the Kilbourn Bridge specifications is now deposited with the Secretary of this Society.

There were so many calls for the Rockton specifications immediately after their issuance and since, that only one copy remains in my charge, and as it is an official paper, I am unable to part with it.

I presume copies may be found among the archives of many of the principal bridge building firms of 1877. The specifications were largely copied from subsequently.

The author of these specifications was aided materially in their preparation by the late C. Shaler Smith, and whatever merit they possess is largely due to his advice.

Theodore Cooper, M. Am. Soc. C. E.—When the paper under discussion was read before the convention of the Society at Seabright, the author requested errors of fact to be pointed out, that corrections might be made before the final publication. A few minor changes have therefore been made. Where opinions were advanced by the author, criticism is proper and desirable.

The author desires to disclaim any intention of undervaluing the services of many able bridge builders in the advancement of American bridge work from his failure to enter in detail into all the minor steps

ERRATA.

Transactions, American Society of Civil Engineers, Vol. XXI, December, 1889.

Page 600, 16th and 17th lines from bottom, insert comma after the word "as," insert comma after the word "spans," change period to comma after the word "strength," and write "it" instead of "It;" the sentence will then read as follows: "and as, for short spans and the floor systems of long spans, rigidity is even more important than is strength, it would, in my opinion, be well to use therein the same intensities of working stresses for both steel and iron."

lengths (about 50 feet), one calculation would suffice. It would not do for the computer to guess at which position of the train will give the greatest stress, for the railroad company's engineer might throw out his design from competition on account of not being quite up to specifications.

The method which I have adopted in designing long span railroad bridges is to use the moment diagram for the floor system and primary truss members, an equivalent uniformly distributed load for the chords and inclined end posts, and the uniform car load headed by one engine excess and accompanied by another two panel lengths distant from the first for the main web members. This method reduces the labor of computation to a minimum, and at the same time gives results that are exact enough to satisfy any but a very captious engineer.

Mr. Cooper makes a very good point when he advises that we do not increase our unit strains in proportion to our faith in new material—especially in reference to short spans, which are nearly always made too light for rigidity. Moreover, steel, although stronger, is no stiffer than wrought-iron; and as, for short spans, and the floor systems of long spans, rigidity is even more important than is strength, it would, in my opinion, be well to use therein the same intensities of working stresses for both steel and iron. At the same time, in the trusses of long spans, I am in favor of straining steel as high as good judgment, based upon adequate testing, would indicate.

At the risk of being considered tedious, I would again urge upon the profession the organization of an association of bridge engineers and builders for the determination of complete standard specifications for railroad bridges, and for the testing of new forms of members and new material. Surely the Government of the United States could be prevailed upon to spend a considerable sum in testing not only material for and full-sized members of bridges, but also the complete bridges themselves! We do not yet know enough about the effect of impact upon structures and upon the members of structures, and it is going to cost money to learn it.

Could not the American Society of Civil Engineers persuade the

Government to appoint a commission of three bridge experts at adequate salaries for the purpose of conducting a series of such tests, and to furnish all the money needed for same? In my opinion, it would be well for the Society to give this suggestion consideration.

D. J. WHITTEMORE, Past President Am. Soc. C. E.—The thanks of our members are due to Mr. Cooper for his able presentation of the important subject of which his paper treats.

As indicating that the Mississippi Bridge at Minneapolis, designed by the late C. Shaler Smith, M. Am. Soc. C. E., is one of importance, I may say that it consists of three river spans, the central of which is 324 feet, and the shore spans are each 272 feet, and the grade of track on same is 150 feet above the river.

Probably Mr. Cooper, when he wrote the paragraph in regard to his Erie specifications of 1878, was unaware of the specifications for the Kilbourn and the Rockton Bridges, the first issued in July, 1877, and the last in October of the same year, both esteemed by many engineers as "covering the designing, proportioning and detail of construction with that completeness necessary to give the railroad company the full advantage of the competitive method with a certainty that the resulting structure would in all ways be up to the advanced state of the art."

The Kilbourn Bridge specifications soon after their issuance were made the subject matter of a paper by C. Graham Smith, President of the Liverpool Society of Civil Engineers, which paper was republished by an engineering journal in this country soon afterwards.

A copy of the Kilbourn Bridge specifications is now deposited with the Secretary of this Society.

There were so many calls for the Rockton specifications immediately after their issuance and since, that only one copy remains in my charge, and as it is an official paper, I am unable to part with it.

I presume copies may be found among the archives of many of the principal bridge building firms of 1877. The specifications were largely copied from subsequently.

The author of these specifications was aided materially in their preparation by the late C. Shaler Smith, and whatever merit they possess is largely due to his advice.

THEODORE COOPER, M. Am. Soc. C. E.—When the paper under discussion was read before the convention of the Society at Seabright, the author requested errors of fact to be pointed out, that corrections might be made before the final publication. A few minor changes have therefore been made. Where opinions were advanced by the author, criticism is proper and desirable.

The author desires to disclaim any intention of undervaluing the services of many able bridge builders in the advancement of American bridge work from his failure to enter in detail into all the minor steps

of progress. Even were the data obtainable, to have enumerated each kind and variety of structure with the mechanical and structural details peculiar to each, and to have attempted to credit to each able engineer the part taken by him in such advancement, would have been a very difficult and extensive task. He sought therefore to confine his descriptions to the marked stages of development only.

Messrs. Pegram and Strobel in their able discussions differ from the author in his advocacy of engine loads in preference to uniform loads for the specified live loads. They advocate other methods as being fully as correct and of more easy application.

While acknowledging that their methods, differing from each other, may give practically as good results for any particular case as the method advocated in the paper, the author thinks they have neglected to appreciate some important points.

Every experienced computer of bridge strains acquires "short cuts" or methods of approximation which are practically correct in final results. It may be by use of tables of equivalent uniform loads for the various classes of engines, by use of an engine excess, or by a leading concentrated load. Where the judgment of an individual designer only has to be considered, such methods can give as near the maximum strains induced by the traffic of a particular road as any specified engine load.

The objections to the use of such methods for general purposes are as follows:

First.—The methods of approximation employed by different computors are as various as the types of locomotives adopted by other engineers, and an agreement upon any one method would be very difficult.

Second.—Such methods of approximation must always be supplemented by certain corrections in particular cases. Mr. Pegram says: "It is assumed that the uniform load will extend over the panel in front of the concentrated load, and the half panel load at the advanced panel point shall be neglected, a condition in favor of simplicity and involving no error when it is an understood feature of the calculations."

Mr. Strobel says (in calculating floor beams and trestle columns): "It (the span) must be taken equal to two panel lengths for floor beams and to two spans for a trestle column."

Third.—Where competitive plans are submitted the only method of determining the relative merits of the strain sheets is by reference to some positive criterion. For this purpose there is no better or more correct method than a definite engine and train load.

Fourth.—And to the author's mind the most important objection, the use of methods which will be incomprehensible to the officials who have largely the decision in determining the capacity of our bridges. When an engineer recommends to the controlling power of a railroad the desirability of providing for a possible future train load, consisting of locomotives and cars of certain styles and weights, he puts the subject in a comprehensible shape.

On the other hand, the relative merit of a particular uniform load table or a certain excess in the form of a concentrated load cannot be properly appreciated. It must be recognized that as a rule the decision in such matters is not left to the engineer. The average official who has the decision in such matters may be perfectly competent to judge of the necessity and possibility of the adoption of some particular kind and weight of rolling stock for the future, but he cannot grasp the importance of specifying some particular concentrated load in connection with a uniform load to represent the strains induced by such rolling stock.

The methods of providing for the camber given by Mr. Strobel and Mr. Pegram do not appear to be as easy of application nor more correct than the method described by the author.

By increasing the top chord the desired fractional amount in excess of the lower chord, leaving the distances between the chords unchanged and then figuring the proper lengths of the diagonals to correspond, we have a truss in which all the members are of proper length and free from all strain when placed on its side. The method is therefore correct in principle and of very easy application.

Mr. Pegram's assumption that the live loads to be used in the future will increase as rapidly as they have in the past is not likely to be realized. Unless the whole railroad system of the country is radically changed so as to admit cars of a greater carrying cross section than now, there is a definite limit where the loads of the cars per lineal foot of track must stop. The carrying cross section of our freight cars now does not vary far from 55 square feet, and unless they exceed the cross section of a Pullman car, cannot exceed 65 square feet.

Excepting the metals and heavier minerals and a limited number of special articles, the freight transported by our railroads will not exceed in weight, as usually stored, 50 pounds per cubic foot.

This weight per cubic foot will cover nearly all merchandise, all food and grain products, steamer compressed cotton and bale goods, coal, coke and lumber—a classification that will cover a very large percentage of all our freight, and, in many systems where the heavy ores are not handled, the total traffic. It is not likely that the heavier ores and the metals will ever be loaded to any large fraction of the carrying cross section of our present cars.

The maximum even for such material is likely to be limited by the loads upon the wheels and rails, and will not probably exceed what is found as suitable from engine wheel loadings.

It therefore seems to the author that the loads per lineal foot of our maximum trains will not be advanced very much or very rapidly beyond 4 000 pounds.

The "100 000 pounds" car of the Pennsylvania Railroad weighs 51 800 pounds and is 40 feet long over all, which is equivalent to 3 795 pounds per lineal foot.

Special cars may be built which will exceed the limit of 4 000 pounds,

but it is not probable that any system of cars exceeding this limit will be adopted for some years, if ever.

That any cars can ever be used which will produce a greater strain on any part of our bridges than our present locomotives, is not believed by the author. It is also very certain that however heavy the cars may become the engines will keep advancing in nearly equal proportion.

Morever, for the great mass of bridges on our roads, spans less than 150 feet, the maximum strains must be produced by two heavy engines —an ordinary possibility.

Mr. Pegram also suggests providing for the effects of impact by adopting certain relations between the uniform and concentrated loads advocated by him.

The author considers a far better method, and one covering other desirable features, is to use different unit strains for the dead and live loads.

Mr. Waddell renews a suggestion that is popular with certain engineers, the establishing of standard specifications by some authority, either a technical or governmental one. The author cannot agree to any such idea. The great and rapid progress in the past has been without any such means, and at no time in the past could any such act have been performed without a distinct blocking of the evolution and progress of the science of bridge building. Are we prepared to say that we have now reached perfection and need no longer the conflict of various opinions to produce further advancement? A standard would not mean the best result even of our present knowledge on any subject, but a compromise to suit the wishes of the majority—a result of the average intellect, and not the belief of the most advanced or experienced leaders.

The experience of the world has been ages older in the use of other materials than iron or steel, and yet engineers have never agreed upon any standards for other constructions, masonry, wood or earth.

We may each individually believe we know all the knowledge in regard to our specialties, but we have no right to force other minds fully as strong in their faith to accept our ideas as the ultimate.

The specifications named by Mr. Whittemore and Mr. Stowell, as of prior claim to the Erie were unknown to the author. On examination he still thinks they are not such full and complete general specifications as the Erie, although they were excellent ones for that day. They certainly were not known sufficiently to have exerted that influence for advancement that has been credited to those of the Erie Railway. The author, however, did not wish to convey the idea that he received no aid or assistance directly or indirectly from the experience of other engineers in preparing these specifications. He gladly availed himself of the advice and criticisms of some of the best of our bridge engineers. To the late C. Shaler Smith, M. Am. Soc. C. E., to whom the advancement of American bridge building owes so much, he was especially indebted for numerous valuable suggestions.

Mr. H. B. Seaman, "from the several specifications in his possession," finds himself justified in questioning the assertion of the author as to American bridge practice in estimating the lateral forces. He also decides the motive to be, not what the author has asserted in his paper, but "to lessen labor and to facilitate the manufacture of so many tons of iron in a certain limited time." He does not make clear, however, how a different method of estimating the lateral forces for preparing a strain sheet is going to aid the very desirable purpose of facilitating the manufacture.

It would appear from the tone of the discussion upon lattice bridges that some of the advocates for this class of structure have thought injustice has been done to this type by the author. He has had no intention to do other than justice, and has put forth fully his reasons for any judgment or opinion expressed. He disclaims any prejudice against this type. He has acknowledged the excellent character and good record of bridges which have been built upon this type. He is further ready to acknowledge the great improvement of our pin-connected bridges by the adoption of features in use in lattice bridges, especially such points as tend to greater rigidity of the structure, as stiff laterals, rigid connections of the floor beams, etc. He, however, has stated that all the good points of lattice bridges can be obtained by proper designs of the pin-connection form.

Lattice bridges have also been influenced and improved by features originally peculiar to the pin-connected bridge, long panels and the greater concentration of parts.

The essential features of the two types, nevertheless, remain distinct, and upon these points the relative merits of the two systems must be determined. The critical difference between the two systems does not consist in any special forms of the individual members or the methods of connecting the floor beams or braces, but in the methods of connecting together the essential members of the trusses; and, as a necessary result of these connections, the different degrees of accuracy in determining the effective lengths of the members and the correctness of the fittings of the joints. When Mr. Stowell talks of the importance of the connection of floor beams and stringers, he is entirely begging the question. The relative merits of the two systems is in nowise dependent on these connections. The accuracy of all parts of the pin-connected bridge is determined at the shop by the most accurate tools and instruments. The joining of them together during erection is simply performed by driving in the accurately turned pins. When so connected the designer can feel the strongest assurance that his design has not been ruined by any reaming of holes or imperfect riveting,

The accuracy of the lattice bridge is dependent upon, 1st, the correctness of the hand-made templet; 2d, the correctness of the marking of the iron from these templets; 3d, the correctness of the punching, and,

4th, the faithfulness of the workmen during erection in bringing all the members to a perfect fit before driving the rivets. Then finally the perfection of the joint depends upon the perfection of the riveting, —usually done by hand on temporary scaffolding.

That good and satisfactory bridges have been and are made in this manner is a credit to our bridge shops and workmen. That such joints have been considered good enough by the workmen before they accurately coincided or have been so incorrectly made that reaming was necessary, is also undoubtedly true.

It is also true, as related by Mr. Stowell, that lattice bridges have been swung clear of the supports by the temporary use of bolts and drift pins. While an emergency would justify such a proceeding, the strongest advocates of the lattice system would hardly consider this as a desirable condition in which to make the permanent connections by riveting.

Mr. Bush candidly acknowledges that "the best type of pin-connected bridge is superior to any riveted bridge in spans over about 125 feet, or, say, 100 feet in double track bridges."

An experienced builder of lattice bridges has stated to me that his experience has confirmed him in the belief that lattice bridges should not be built exceeding 180 feet.

As plate girders are rapidly cutting off the lower limits and the upper limits are being lowered even by those experienced with the lattice type, it can hardly be credited to the author as a prejudice if he has ventured to analyze the reasons for American engineers preferring certain types over others. The author has never deprecated the publication of bridge accidents, as Mr. Stowell says, but he does object to the collection and publication of sensational and prejudicial accounts of accidents for any partisan purpose, and when such publications are made by one holding an official position, the author desires most distinctly to deny that such reports are to be considered as having in any manner the weight of "official reports" or what would be considered in foreign countries as unbiassed and carefully digested reports based upon an official examination into all the facts.

Mr. Stowell is fully entitled personally to the freedom of any hobby he may wish to indulge in. But he must blame no one for weighing any testimony presented by him in the light of his prejudices. Mr. Stowell states: "I have records of pin-connected bridges wrecked by all of these common accidents (breaking of an axle, etc.), but after years of diligent inquiry I have failed to find a single instance where a riveted bridge fell in consequence of any such accident." Mr. Stowell must have known of the St. George Bridge accident on the Grand Trunk Railway, February 27th, 1889, as the account was fully published in the *Engineering News* of March 9th. From the breaking of a tire the train entered the bridge derailed; one span and part of the train fell. The bridge was a lattice bridge. The failure was in no manner due to the

character of the trusses, but was a fault of management in having a defective floor system. Mr. Stowell's list consists largely of pin-connected bridges destroyed for precisely similar reasons. Why does he make an exception of this case? Are faults of management on pin-connected bridges only to be classed as bridge failures?

The author again asserts that such compilations of bridge accidents have been magnified and distorted and are undoubtedly full of sensation and inaccuracies.

He also regrets to believe that when the law of probability has its full effect, the number of iron bridges over 100 feet span being as 6 500 for pin-connected trusses to 500 (Mr. Stowell's estimate) for lattice bridges, Mr. Stowell's faith that lattice bridges cannot be split open or knocked down by colliding or derailed trains or other faults of management will be severely shocked.

In regard to the publication of accidents to or upon bridges, the author advocates the fullest records regardless of any bias or prejudice for or against any style of bridge. But such records should be classed in the proper manner, faults due to the type, if such there be, faults due to the details or construction of the structure, faults of maintenance or operation. The purpose of such publication should be the truth and a desire to improve the safety of our railroads.

It should also be borne in mind that the bridge designer or builder has little voice in the methods to be employed for guarding or protecting a bridge. This point devolves upon the maintenance of way authorities.

It is therefore harmful to class all accidents of management as bridge accidents and to put any part of the fault upon the style of the structure which belongs not to the bridge engineer but to the train or track management. The remedy will be much sooner applied by making clear the true fault and placing the blame on the proper authorities.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

430.

(Vol. XXI.—December, 1889.)

DISCUSSION

ON

THE SIBLEY BRIDGE.*

By H. V. HINCKLEY, M. Am. Soc. C. E. and
W. H. BREITHAUPT, Am. Soc. C. E.

H. V. HINCKLEY, M. Am. Soc. C. E.—The following statement of prices and cost of the Sibley Bridge is interesting, as also the cost of the Blair and Plattsmouth bridges, which I give here for comparison :

COST EXCLUSIVE OF 3 553 FEET OF TRESTLE, EAST APPROACH, PROPERTY OF RAILWAY COMPANY.

SUPERSTRUCTURE.		
3 spans, 396 feet each.		
Iron, 1 545 684 pounds, at 4¼ cents†	$65 691 57	
Steel, 2 156 252 pounds, at 4½ cents†	97 031 34	
		$162 722 91
1 span, 247 feet.		
561 082 pounds iron and steel.		
1 span, 197 feet.		
371 295 pounds iron and steel.		
2 spans, 172 feet.		
504 516 pounds iron and steel.		
1 436 893 pounds of iron and steel, at 4.45 cents†		63 941 74

* Transactions, Vol. XXI, No. 422, September, 1889.

† Erected and painted two coats.

VIADUCT.

1 793 882 pounds, at 4.32 cents*	$77 495 70
False work as per contract (same stated therein)	23 000 00
For changing height of viaduct	997 03
	$328 157 38
Miscellaneous	8 614 55
Total superstructure	$336 771 83

SUBSTRUCTURE.

Pier No. 1.

Excavation	$277 79	
Concrete, 156.14 cubic yards, at $6.50	1 014 91	
Masonry, 103.57 cubic yards, at $14.75	1 527 66	
Coping, 13.84 cubic yards, at $20	276 80	
Granite bridge seats, 1.78 cubic yards, at $49	87 22	
		$3 184 38

Pier No. 2.

Excavation	$500 91	
Concrete, 383.96 cubic yards, at $6.50	2 495 74	
Masonry, 974.46 cubic yards, at $14.75	14 373 28	
Coping, 59.84 cubic yards, at $20	1 196 80	
Granite bridge seats, 7.78 cubic yards, at $49	381 22	
Rip-rap, 212.61 cubic yards, at $2	425 22	
		19 373 17

Pier No. 3.

Timber in caisson and crib, 251 036.209 feet, at $40	$10 041 45	
Wrought-iron, 31 509.0 pounds, at 4½ cents	1 417 90	
Cast-iron, 1 301.0 pounds, at 4 cents	52 04	
Sinking, 53 947.575 feet, at 18 cents	9 710 56	
Bowlders, 19 329.75 feet, at 35 cents	6 765 41	
Concrete in air chambers, Portland, 46.0 cubic yards, at $15.50	713 00	
Concrete in air chambers, American, 466.62 cubic yards, at $14	6 532 68	
Concrete in crib, 262.644 cubic yards, at $6.50	1 707 19	
Main shaft left in caisson, 32.2 lineal feet, at $10	322 00	
Masonry, limestone, 1 554.16 cubic yards, at $14.75	22 923 86	
Masonry, granite, 156.25 cubic yards, at $35	5 468 75	
Coping, limestone, 69.47 cubic yards, at $20	1 389 40	
Granite bridge seats, 7.34 cubic yards, at $49	359 66	
		67 403 90

Pier No. 4.

Timber in caisson and crib, 241 968.0 feet, at $40	$9 678 72	
Wrought-iron, 29 143.0 pounds, at 4½ cents	1 311 44	
Cast-iron, 790.0 pounds, at 4 cents	31 60	
Sinking (cubic feet displacement), 52 608 0 feet, at 18 cents	9 469 44	
Bowlders (taken out through air lock), 22 796.8 cu. feet, at 35 cents	7 978 88	
Concrete in air chamber, Portland, 30.0 cubic yards, at $15.50	465 00	
Concrete in air chamber, American, 388.81 cubic yards, at $14	5 443 34	
Concrete in crib, 333 451, at $6.50	2 167 43	
Main shaft left in caisson, 30.75 lineal feet, $10	307 50	
Masonry, limestone, 1 511.96 cubic yards, at $14.75	22 301 41	
Masonry, granite, 156.25 cubic yards, at $35	5 468 78	
Coping, limestone, 69.47 cubic yards, at $20	1 389 40	
Granite bridge seats, 7.34 cubic yards, at $49	359 66	
		66 372 57

Pier No. 5.

Timber in caisson and crib, 238 653.0 feet, at $40	$9 546 12	
Wrought-iron, 27 061.0 pounds, at 4½ cents	1 217 75	
Cast-iron, 1 247.0 pounds, at 4 cents	49 88	
Sinking, 51 142.10 feet, at 18 cents	9 205 58	
Bowlders, 21 393.37 feet, at 35 cents	7 489 43	
Concrete in air chamber, Portland, 28.0 cubic yards, at $15.50	434 00	
Concrete in air chamber, American, 322.03 cubic yards, at $14	4 508 42	
Concrete in crib, 546.52 cubic yards, at $6.50	3 552 38	
Main shaft left in caisson, 39.9 lineal feet, at $10	399 00	
Masonry, limestone, 1 369.01 cubic yards, at $14.75	20 192 90	
Coping, limestone, 65.92 cubic yards, at $20	1 318 40	
Granite bridge seats, 6.78 cubic yards, at $49	332 22	
		58 246 08

* Erected and painted two coats.

Pier No. 6.

Timber in caisson and crib, 167 051.01 feet at $40	$6 682 04	
Wrought iron, 18 988.00 pounds at 4½ cents	854 46	
Cast-iron, 1 438.00 pounds at 4 cents	59 52	
Sinking, 43 444.86 feet at 18 cents	7 820 07	
Bowlders, 10 080.00 feet at 35 cents	3 528 00	
Concrete in air chamber, 195.64 cubic yards at $14	2 738 96	
Concrete in crib, 393.45 cubic yards at $6.50	2 557 42	
Main shaft left in caissons, 31.95 lineal feet at $10	319 60	
Masonry, limestone, 659.06 cubic yards at $14.75	9 721 14	
Coping, limestone, 32.25 cubic yards at $20	645 00	
Granite bridge seats, 6.48 cubic yards at $49	317 52	
		$35 241 13

Pier No. 7.

Timber in caisson and crib, 117 422.00 feet at $40	$4 698 88	
Wrought iron, 12 170.0 pounds at 4½ cents	547 65	
Cast-iron, 947.0 pounds at 4 cents	37 88	
Sinking, 33 274.80 feet at 18 cents	5 989 46	
Bowlders, 5 184.0 feet at 35 cents	1 814 40	
Concrete in air chamber, 147 737.0 cubic yards at $14	2 068 32	
Concrete in crib, 272.69 cubic yards at $6.50	1 772 48	
Main shaft left in caisson, 45.0 lineal feet at $10	450 00	
Supply shaft left in caisson, 7.0 lineal feet at $4	28 00	
Masonry, limestone, 503.09 cubic yards at $14.75	7 420 58	
Coping, limestone, 15.77 cubic yards at $20	315 40	
		25 143 05

Pier No. 8.

Excavation	$1 643 77	
Timber in cofferdam, 362 940 feet at $40	1 451 76	
Concrete in cofferdam, 48.89 cubic yards at $6.50	317 78	
Wrought iron in cofferdam, 1 165 pounds at 4½ cents	52 42	
Cast-iron in cofferdam, 508.0 pounds at 4 cents	20 32	
Piling, 3 360 lineal feet at 50 cents	1 680 00	
Timber in grillage, 15 282 feet at $40	611 28	
Timber used second time, 1 282 feet at $25	32 05	
Wrought iron in grillage, 1 479 pounds at 4½ cents	66 56	
Cast-iron in grillage, 30 pounds at 4 cents	1 20	
Masonry, limestone, 418.45 cubic yards at $14.75	6 172 14	
Coping, limestone, 15.07 cubic yards at $20	301 40	
Rip-rap, 378.32 cubic yards at $2	756 64	
		13 107 32

Pedestal Piers.

Excavation	$2 496 13	
Concrete in foundation, 1 776.58 cubic yards at $6.50	11 543 22	
Masonry, limestone, 669.71 cubic yards at $16	10 715 36	
Coping, limestone, 73.90 cubic yards at $20	1 478 00	
Dowel holes, 1 018.0 lineal feet at $1	1 018 00	
Anchor plates fitted and set, 84 at $2.50	210 00	
Timber left around concrete, 2 280 feet at $40	115 20	
Rip-rap, 3 609.78 cubic yards at $2	7 219 56	
		34 795 46
		$322 867 57
Miscellaneous		29 758 43
Total substructure		$352 656 00

Summation.

Superstructure	$336 771 83
Substructure	352 626 00
Surveying and engineering	24 062 84
Miscellaneous, such as shore protection, etc	62 692 11
Grand Total, November 30th, 1888	$776 151 78

Blair Crossing Bridge over the Missouri River. Erected 1882–83. George S. Morison, Chief Engineer.

Superstructure.

	Steel. Pounds.	Wrought Iron. Pounds.	Cast-iron. Pounds.
Three through spans, each 330 feet	884 548	1 447 148	57 322
One deck span on west end, 176 feet	6 753	238 550	4 325
One deck span on east end, 110 feet	4 979	110 503	8 955

Substructure.

(Pier No. 1 is at West End of 330 Feet Span.)

	Pier No. 1. Feet.	Pier No. 2. Feet.	Pier No. 3. Feet.	Pier No. 4. Feet.	Other Masonry.
Height of pier (masonry), about	74	100	100	74	.....
Top of pier to high water, about	45	49	49	45	.....
Depth of water, about	4	7	32	13	.....
Top of pier to bottom of caisson, about	112	117	117	112	.....
Cubic yards, masonry	1 081.7	1 562.4	1 578.9	1 108.4	5.8
Cubic yards, concrete	1 198.7	589.4	592.3	1 205.4	252.3

Cost of foundations of piers Nos. 1, 2, 3 and 4	$94 364 69
Cost of masonry of piers Nos. 1, 2, 3 and 4	125 963 62
Cost of approach piers	6 495 88
Total	$226 824 19

Summary.

	Exclusive of Freight.	Freight.	Total.
Superstructure	$200 336 68	$6 188 80	$206 525 48
Substructure	190 383 39	36 440 80	226 824 19
Approaches	181 655 97	9 044 35	190 700 32
Protection works	214 238 73	187 462 08	401 700 81
Tracks	45 915 56	1 073 99	46 989 55
Buildings and tools	17 547 01	639 18	18 186 19
Real estate	9 112 76	.....	9 112 76
Engineering	27 051 54	.....	27 051 54
	$886 241 64	$240 849 20	$1 127 090 84

Plattsmouth Bridge across the Missouri River at Plattsmouth, Neb. Erected 1879–1880. George S. Morison, Chief Engineer.

Superstructure (Commencing at west end).

120 feet iron viaduct, trestle	61 529	pounds	iron.	.....	pounds	steel.
2 spans of 400 feet double intersection, through trusses	702 706	"	"	983 703	"	"
3 spans of 200 feet, single intersection, deck plate trusses	800 871	"	"	10 290	"	"
1 440 feet iron viaduct	799 557	"	"	.....	"	"
Total	2 364 663	"	"	993 993		

2 000 feet temporary trestle, afterwards filled, connected with east embankment.

COST OF SUPERSTRUCTURE.

Two 400 feet spans	$114 436 12
Three 200 feet spans	35 039 76
Viaducts	36 236 06
Floor and painting	19 457 82
	$205 169 76
Freight charges	16 079 31
Total	$221 249 07

The two 400 feet spans are across the deep water. Highest water during 1877–1880 = 511 and lowest 495 (±).

The three 200 feet spans cross the overflowed land.

SUBSTRUCTURE.

(Pier No. 1 is at west end of first 400 feet span.)

	Pier No. 1.	Pier No. 2	Pier No. 3.	Pier No. 4.	Pier No. 5.	Pier No. 6.	West of No. 1.	East of No. 6.
Elevation top of masonry	559.0	559.0	559.0	529.5	528.5	527.5	.....	.....
Elevation bottom of masonry	495.0	495.0	491.5	497.5	496.5	505.5	.....	.....
Elevation bottom of concrete	461.5	.....	.....	.....	493.7	502.5	.....	.....
Elevation bottom of caisson	.....	464.2	444.3	442.9	.....	.....	.....	.....
Cubic yards masonry	800.16	781.04	786.34	257.97	258.98	141.30	345.5	541.5
Cubic yards concrete	987.2	945.4	1 467.2	965.7	58.4	50.9	44.7	195.0
Cost: Foundation without freight	$16 758 47	$32 567 97	$45 461 92	$36 522 23	$3 945 57	$626 83	$12 517 43	
Cost: Masonry	14 450 57	15 154 92	16 164 45	4 829 94	3 776 06	1 903 85	.....	
Total, freight inclusive.	39 607 23	56 613 86	71 597 48	47 075 85	9 785 40	2 892 02	13 184 40	

TOTAL COST.

Substructure	$240 156 24	
Superstructure	221 249 07	
		$461 405 31 bridge proper.
East approach	$33 942 48	
West approach	59 295 44	
		93 237 92 approaches.
Permanent track		16 209 35
Shore protection		1 611 71
Buildings, tools and temporary tracks		10 526 90
Engineering and incidentals		26 950 54
		$609 941 73

W. H. BREITHAUPT, M. Am. Soc. C. E.—Mr. Scherzer's point* as to compression due to wind in bottom chord of 400-foot spans is well taken. Wind pressure will, with the improbable combination cited, give resultant compression strain in third panel of bottom chord, and such strain might even be greater than that shown, as a train of empty box cars may run under 1 000 pounds per running foot in weight. Opposing it are transverse rigidity of floor beam flanges (12½ inches wide with cover plate on each) up to outer stringer, and eventually friction and anchor bolts at pedestals, together offering resistance with unit strains well

* Transactions, September, 1889, p. 130.

within elastic limit of material in rare cases of occurrence of above strain.

Obviating the bending of intermediate posts when span deflects, as it does in its regular use under passing loads, by enlarging pin holes where diagonals cross as described in the text, must be considered of greater importance than provision by means of this central pin against torsion of post due to eccentricity of lower lateral connections, such torsion being provided against by bottom chord pin, as lower laterals are directly above bottom chords.

The end stiffening strut is fixed to both end post and intermediate post, being riveted to each.

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